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Emanuele Colonnelli

Spyridon Lagaras

Jacopo Ponticelli

Mounu Prem

Margarita Tsoutsoura

Universidad del Rosario, Facultad de Economía
Cl. 12c #4-59. Bogotá, D. C., Colombia
PBX: (031) 2970200 ext. 4163

facultadeconomia@urosario.edu.co
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Revealing Corruption:
Firm and Worker Level Evidence from Brazil

Emanuele Colonnelli Spyridon Lagaras Jacopo Ponticelli
Mounu Prem Margarita Tsoutsoura*

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Abstract

We study how the disclosure of corrupt practices affects firms and their employees. We construct novel firm-level measures of involvement in corrupt practices using randomized audits and public procurement suspensions in Brazil. On average, exposed firms grow larger after the audits. However, this result masks large heterogeneity depending on the degree of firm involvement in the corruption scheme. Using contract-, loan-, and worker- level data, we show that highly corrupt firms suffer after anti-corruption initiatives, while other exposed firms grow by changing their investment strategy when shifting away from doing business with the government.

*Colonnelli: Chicago Booth School, emanuele.colonnelli@chicagobooth.edu; Lagaras: Katz Graduate School of Business, University of Pittsburgh, slagaras@katz.pitt.edu; Ponticelli: Northwestern Kellogg, NBER, and CEPR, jacopo.ponticelli@kellogg.northwestern.edu; Prem: Department of Economics, Universidad del Rosario, francisco.munoz@urosario.edu.co; Tsoutsoura: Cornell University, NBER, and CEPR, tsoutsoura@cornell.edu. We thank many seminar participants and discussants. Mark He and Naoko Yatabe provided excellent research assistance. We are grateful to The University of Chicago Booth School of Business, the Fama Research Fund, the Liew Family Junior Faculty Fellowship, the Initiative on Global Markets, the Stanford SEED, SCID, and IRiSS centers, the CEPR-PEDL Initiative, and the J-PAL Governance Initiative for financial support.

I Introduction

Corrupt practices in the assignment of government contracts are largely diffused across countries. These practices are particularly widespread in emerging markets, where they are considered a barrier to growth due to the extra costs of doing business that they impose on firms and the distortions in the allocation of resources across and inside firms they may generate (Svensson, 2005; Olken and Pande, 2012). In recent years, governments and international organizations around the world have attempted to fight corruption mainly through transparency initiatives aimed at exposing and sanctioning corrupt practices in the allocation of public procurement contracts (Hanna et al., 2011). Such efforts are attracting more and more attention from policy makers and the media, yet there is scarce direct empirical evidence on how they impact the firms involved in such corrupt practices, and on how such effects spill over onto other firms' stakeholders. In this paper, we rely on micro-data from Brazil and on a unique institutional setting to trace the impact of exposing corrupt practices on the exposed firms and their employees.

The main challenges previous work has faced are two. First, it is difficult to estimate the causal impact of transparency initiatives on specific firms involved in shady business with the government. Most such policies are aggregate shocks impacting entire industries, countries, or localities, which makes it complicated to isolate the effects on the specific firms from other aggregate effects of the program.[1] There are also initiatives that do target specific firms, such as enforcement actions as part of the Foreign Corrupt Practices Act (FCPA), but most such initiatives suffer from the endogenous selection of the targets to investigate. A second main challenge relates to the need for detailed micro-data to trace the impact of these policies on the stakeholders of affected firms, such as their employees. This type of data is also crucial to one's ability to dig deeper into the mechanisms of how a corrupt environment affects firm strategic decisions, for instance as they pertain their investment and internal organization.

Our empirical design aims to directly address these issues. The setting of Brazil – a country where both the local and federal administrations are traditionally plagued by widespread corruption – offers several advantages. In 2003, the newly elected Lula's government created a federal agency – the CGU – in charge of fighting corrupt practices in the allocation and use of federal funds. CGU launched a program which randomly audits municipal budgets aimed at uncovering any misuse of federal funds. Importantly for our purposes, while the targets are the municipalities, the audits uncover the identity of specific firms involved in irregular business with the government. Since many of these firms are located far away from the audited municipalities, as they do business across

[1] For example, Zeume (2017) studies the impact of the 2010 Bribery Act on U.K. firms' cost of doing business and Rauter (2019) study mandatory extraction payment disclosures and their impact on multinational firms in foreign host countries. Colonnelli and Prem (2020) study our same anti-corruption program in Brazil, but focus on the economic effects on the targeted municipalities.

different geographical areas, by focusing on these firms we are able to isolate the impact of exposure of corrupt practice on firms from its impact on the local economy of the audited municipality.

To fully investigate the impact of the program on both firm- and worker- level outcomes, we start by constructing a new dataset covering all firms who are exposed by the CGU random auditing program. Our main data source are the audit reports produced by the federal auditors that review the municipal budgets. Both the companies and the local politicians involved in any misuse of federal funds are disclosed in the audit reports, which are then published online and made available to the public and the popular press. From the 1,881 audit reports produced by the CGU between 2003 and 2014, we manually collect information on all the irregularities reported, including the fiscal code of the firms involved, the nature of the irregularity, the type of involvement of the firm, and the value of the contracts. Our dataset contains information on several thousand firms exposed by the random audit anti-corruption program. We then match our firm-level dataset with social security data of the Ministry of Labor (RAIS) – which contains detailed information on all formal workers employed in Brazil – as well as with data on firms' access to credit lines from the Brazilian development bank (BNDES), and on firms' access to local and federal public procurement contracts.

Our analysis proceeds in three main steps. We start by studying the effect of anti-corruption transparency initiatives on the growth of exposed firms. We then study how firm organization and behavior change in response to the disclosure. Finally, we explore how the employees of exposed firms are affected by the revelation of misconduct.

The empirical strategy primarily relies on the random timing of the audits, which are determined by a national televised lottery, thus guaranteeing our time-series variation to be exogenous. Yet, firms that do business with local governments might be selected on multiple dimensions. Hence, we combine a dynamic difference-in-difference design with a matching strategy that aims at identifying a plausible control group for the exposed firms. Both treated and control firms do business with local governments, and both are selected to be located outside of the audited municipalities, so that we can isolate the firm-level effects from the aggregate impact of the audits.

Using a similar matching event study design, we complement our analysis on audited firms with the study of a different but related transparency initiative. Starting in 2008, the government instituted the national registry of companies suspended from participating in bids for government contracts due to previous violations of public procurement rules, a registry called CEIS. Although the CEIS program does not offer the random-by-design variation in the timing of exposure, it helps us both maximize external validity, and obtain more power for the study of mechanisms and for the individual-level analysis. Indeed, firms included in the CEIS registry can be considered as "highly corrupt" firms, where the misconduct has not simply been exposed (as in the case of the audits), but also certified by the government and punished through a suspension from getting government contracts. In addition, we always report the analysis of pre-trends, which helps us gauge the extent to which our estimated effects might be driven by systematic differences relative to their control firms.

We find that firms exposed by the auditing program experience, on average, a 4.8 percent larger increase in size relative to the control group in the three years following exposure. This perhaps surprising average effect masks a rich heterogeneity across firms depending on their involvement in corrupt practices. Using the hand-collected information on the type of irregularity companies were involved in, we document that the majority of firms mentioned in the audit reports (53%) can in fact be considered victims of corruption. One example are firms put at a disadvantage by a rigged bidding process. We define these firms as *Passively Involved*. Another 40% of firms exposed by the audits are instead firms that were actively involved in an irregularity, although we cannot conclusively argue that they were benefiting from it. One example are cases of over-invoicing for a specific good or service, where it is unclear whether rents are extracted solely by the politician or also by the firm. We define such firms as *Actively Involved*. Finally, a small fraction (7%) of the exposed firms were clearly benefiting from the corruption, as in the well-known scheme where private firms provide kickbacks to politicians in exchange for securing procurement contracts. An illustrative example of such cases would be firms that received payments but did not deliver the goods and services required by the procurement contract, in addition of evidence being found of bribe exchanges. We define these firms as *Corrupt*.

Using these micro-level data on the type of irregularity, we show that the effect of the anti-corruption program on exposed firms differs substantially depending on the degree of firm involvement in the corrupt practices exposed by the audit. Passively involved firms display the largest increase in size following exposure. Actively involved firms experience, on average, a smaller but still positive increase in size after exposure relative to the control group. Only corrupt firms experience a significant and large (20 percent) relative decline in size after their exposure. Our evidence therefore indicates that most exposed firms benefit from the transparency program, while a small set of corrupt firms is harmed by the revelation of misconduct. When exploring the impact of being included in CEIS and therefore barred from participating in public procurement, in line with our results on corrupt exposed firms, we find these highly corrupt firms also experience a large and statistically significant decline in size. Specifically, the employment in exposed firms declines by a staggering 90% more than in a control group of similar firms in the post-exposure period.

Next, we explore potential mechanisms behind the documented differences in relative performance of firms following the transparency programs. On the one hand, the revelation of misconduct can harm firms responsible for the misconduct because it leads to punishment by the government, for example via reduced access to government contracts or loans, or by other stakeholders, perhaps because customers or business partners do not want to be associated with a corrupt firm. Such channels may explain why (highly) corrupt firms shrink in size after exposure. On the other hand, understanding why the revelation of corruption leads to the growth of a large share of exposed firms is more challenging, as it is puzzling that even the set of firms who are somewhat actively involved in the misconduct grow after the corruption is exposed. One possibility is that the revelation of corruption coincides with the punishment of the corrupt government officials the firm interacts with (Avis et al., 2018), which will reduce the firm's cost of doing business in future interactions. This is plausible considering that firm-government relationships are sticky (Ferraz et al., 2015). In such a case, we would expect to see firms doing the same amount or even more business with the government. Another possibility is that the revelation of corruption cuts a firm's access to government business, and forces the firm to compete in the private sector. If dealing with the government makes firms operate inefficiently (Dal Bó and Rossi, 2007; Bertrand et al., 2018), this channel might explain at least part of our findings.

To provide evidence in favor or against some of the mechanisms, we proceed as follows. First, we use contract-level data to study how exposure affected firms' ability to access local and federal procurement contracts. Second, we use loan-level data from the development bank (BNDES) to investigate whether exposure affected firms' access to subsidized government credit. Third, we investigate whether exposure affected firms' investment strategy, specifically with respect to internal organization choices.

We document that exposed firms experience a significant reduction in their access to both local and federal procurement contracts. These effects are stronger for the *Corrupt* firms in our sample, but also present for both the *Actively* and *Passively involved* firms, suggesting that the government might decide not to do business with firms whose names were disclosed in the audit reports – independently of the specific context in which they are mentioned. The larger effect on *Corrupt* firms is consistent with what we document for firms exposed by the CEIS program, whose access to local and federal procurement contracts is entirely cut. When focusing on how exposure affects access to investment loans from the government, we find a more mixed picture. On average, audited firms, if anything, obtain more loans after the exposure. On the other hand, we find large and negative effects of firm exposure to the CEIS program on the intensive and the extensive margin of government funding.

Overall, the evidence on the first two mechanisms is consistent with a negative effect of exposure on firms that clearly benefited from the corruption schemes and were exposed by the random audit program. Importantly, these findings also rule out a possible explanation for why the majority of firms exposed by the audits grow. Indeed, since government access to procurement contracts is reduced for these firms as well, it is unlikely that these firms are growing simply because they face lower corruption, and hence costs, in their interaction with the government. This leaves the possibility that the presence of corruption made certain productive firms operate inefficiently, as argued by Cohen and Malloy (2016), who show that firms that rely more on government contracts tend to grow slower and invest less in tangible and intangible capital. In our setting, exposed firms might therefore adapt by changing their internal growth strategy, from one in which they focus on securing government contracts in the pre-audit period, to one in which they invest to compete in the market for private demand after the revelation of corruption. Such a channel is extremely difficult to test using solely administrative data, but we can at least provide some suggestive evidence in its favor. While we do not have data on investment or technology adoption, we can explore whether exposure in audit reports affects firms' internal organization, and specifically skill upgrading, which previous work describes as an important measure of firm restructuring towards long-term growth (Caliendo and Rossi-Hansberg, 2012; Ferraz et al., 2015). Consistent with our hypotheses, we find that *Actively involved* firms in particular experience an increase in both the average education level of their labor force and their share of high-skilled workers.

In the final part of the paper, we analyze the potential spillover effects of exposure on a primary, yet largely understudied group of stakeholders of a firm, namely its employees. We use worker-level data to explore the impact of transparency initiatives on employment status and labor income. In addition to the direct consequences on firms, the audits may independently influence individual outcomes to the extent that the disclosure of misconduct is valued negatively on the labor market (Fama, 1980, Fama and Jensen, 1983). We find that workers that were employed by firms exposed by the random auditing program experience no significant changes in their probability of being employed, nor on their annual labor income. On the other hand, when focusing on the CEIS sample, we find that workers that were employed by firms reported in the registry experience a 7.5 percent decline in their probability of being employed in the three years after their firm has been exposed, a 10.8 percent decline in their annual labor income, and a 3.5 percent decline in their annual labor income when conditioning on workers that remain employed. Furthermore, we find that these short-term career losses are present and large not just for managers, but also for white collar workers and blue-collar employees at the bottom of the organizational hierarchy. These results of real and significant effects on the income of workers of highly corrupt firms are perhaps surprising considering that it is unlikely that non-managers are directly responsible for the misconduct behavior.

Overall, our findings uncover large heterogeneity in the impact of anti-corruption transparency initiatives, which is often masked in aggregate estimates. Highly corrupt firms experience a major decline when exposed, primarily because of a cut in access to government contracts and funding, indicating the importance of transparency initiatives and follow-up legal enforcement in allowing the most efficient firms to prevail. At the same time, our results show that the vast majority of firms mentioned in the audit reports –

and therefore whose activities have been affected by corrupt practices to some degree – are seemingly harmed by their relationship with the government, and subsequently grow after the anti-corruption crackdown. These latter findings are consistent with corruption hindering the growth of productive firms through various operational distortions (Fisman and Svensson, 2007; Olken and Pande, 2012), and they indicate that the cost of operating in a corrupt environment might outweigh the benefits that firms can get from it. While our analysis of mechanisms rules out certain channels and point to certain operational distortions within the firm, future research is needed to pin down what the specific distortions are. Importantly, it remains to be understood why certain firms decide to enter a seemingly damaging business relationship with the government in the first place.

We primarily relate to three strands of literature. First, the paper contributes to the existing literature on corporate corruption, and specifically to the set of papers examining the link between corruption and firm-level growth (see Bardhan, 1997, Svensson, 2005, and Olken and Pande, 2012 for comprehensive reviews of the literature). Interest in this link comes from the importance of a broad set of influential studies arguing that corruption affects aggregate economic growth and has implications for welfare (e.g., Shleifer and Vishny, 1993; Mauro, 1995; Clague et al., 1996; Hall and Jones, 1999; La Porta et al., 1999; Glaeser and Saks, 2006). Yet, the nexus between corruption and firm-level growth remains largely unexplored due to the lack of contexts where causality can be established. Most insights therefore come from studies that place a strong emphasis on the rich evidence coming from new data obtained through innovative methods, and which point to the importance of understanding heterogeneity across firms.[2] We make three contributions to this body of work. First, we contribute from a methodological perspective by manually collecting new data on exposed firms using government audit reports. Second, our empirical design allows us to overcome some of the endogeneity issues related to the link between corruption and firms. Third, we bring in a large set of administrative data sources that allow us to investigate various ways through which corruption affects firms, their operations, and their employees. Indeed, an important contribution is that we shed light on various within-firm distortions associated with corruption, largely unexplored in the academic literature (Dal Bó and Rossi, 2007, Smith, 2016). Our findings emphasize the importance of corruption for various strategic choices by the firm, such as those related to funding sources and market access. A novel related empirical finding is that corruption in the local environment seems to affect firms' choices of organizational design. This is a new motive that connects corruption to the literature on boundaries of the firm

[2] Svensson (2003) use survey data from 250 firms in Uganda discussing when and how much they pay in bribes, and Fisman and Svensson (2007) use the same data to study the relationship between bribes, taxes and firm growth. Sequeira and Djankov (2014) collect data from 120 South African firms and their bribe payments at ports to argue that firms change production decisions due to bribe demands. Similarly innovative data is collected by Cole and Tran (2011) in an Asian country. Finally, Decarolis et al. (2019) use data from the Italian FBI to study criminal firms in public procurement.

(Holmström and Roberts, 1998, Rajan and Wulf, 2006, Roberts, 2007, Seru, 2014).

Second, by looking at firms potentially receiving preferential treatment from local politicians, we broadly relate to studies that assess the importance of political connections to firms. A number of studies have explored various ways through which politically connected firms might receive unfair advantages (Fisman, 2001). For instance, Khwaja and Mian (2005) show that politically connected firms obtain preferential access to finance, while Faccio (2006) studies political connections across countries. Other examples include Faccio et al. (2006), Claessens et al. (2008), Goldman et al. (2009), Cooper et al. (2010), Cohen et al. (2011), Duchin and Sosyura (2012), Cingano and Pinotti (2013), Akey (2015), Fisman and Wang (2015), Akey and Lewellen (2017), Colonnelli et al. (2020), González and Prem (2020), and Bertrand et al. (2020). More specifically, we relate to the literature linking political connections to preferential access to government contracts, such as in the studies by Goldman et al. (2013), Schoenherr (2019), and Brogaard et al. (2019).

Finally, a growing literature provides direct evidence on the effectiveness of anti-corruption initiatives (e.g., Smith et al., 1984; Karpoff et al., 2014; Griffin et al., 2016; Zeume, 2017; Giannetti et al., 2017; Chen and Kung, 2019; Christensen et al., 2020) and more specifically government audits in reducing corruption (e.g., Olken, 2007; Bobonis et al., 2016). In particular, following the seminal work of Ferraz and Finan (2008, 2011), several papers have investigated the effects of the Brazilian random audit program (e.g., Bologna et al., 2015, Zamboni and Litschig, 2018, Avis et al., 2018). Closest to our paper is Colonnelli and Prem (2020), who analyze the impact of the program on the local economy of audited municipalities, finding that economic activity increases mainly through the growth of government-dependent sectors.[3] Our paper is unique to the extent that we are the first to assemble information on specific firms that were directly associated with an irregularity, which allows us to explore the link between government misconduct and corporate misconduct directly.[4]

The paper is organized as follows. Section II describes the institutional setting and provides a detailed description of the anti-corruption initiatives we study. Section III presents the new firm-level dataset on corrupt practices revealed by the random auditing program that we construct from the text of the audit reports. Section IV presents our identification strategy, and section V describes all the main empirical results of the paper. Section VI concludes.

[3]The more recent CEIS registry received significantly less attention, with the exception of the study by Szerman (2020), who find similarly negative effects to the ones we uncover on corrupt firms. Our key difference is the reliance on randomized audits for identification, and the use of additional data on local public procurement and government loans as part of the analysis of mechanisms.

[4]Importantly, while all studies focus on the consequences of the audits on the targeted municipalities, we estimate our effects on firms that are located outside of targeted municipalities, thus removing any aggregate impact of the audits from our estimates. As a result, our empirical strategy is different from previous work, and our results complementary to the understanding of the consequences of the anti-corruption program.

II Institutional Background

Brazil has constantly battled with corruption. The primary institutions involved in preventing corruption practices in Brazil are the Office of the Comptroller General (Controladoria-Geral da União), the Federal Court of Accounts (Tribunal de Contas de União), the Federal Public Prosecutor's Office (Procuradoria-Geral da Repùblica), the Federal Justice (Justiça Federal), and the Federal Police (Policia Federal). These institutions function in a complementary manner to promote accountability and reduce corruption in the management of public assets and funds.

The Office of the Comptroller General (CGU) was established in 2003 as the first federal executive body specializing in anti-corruption policies and internal control. The scope of CGU is to promote transparency and identify and prevent corruption in the federal administration and the management of public resources. The Federal Court of Accounts (TCU) is responsible for monitoring the budgetary performance of government bodies and applying administrative penalties related to the misuse of public resources, while the Federal Public Prosecutor's Office (MPF) is in charge of bringing the cases to the Federal Justice (JF) for initiating criminal and civil prosecution.

Among the major anti-corruption initiatives carried out by the CGU were the random municipal audits program, which has been the flagship anti-corruption program for more than a decade, and the creation of the National Register of Companies Suspended and Not in Good Standing (CEIS). Below we provide more details on these two initiatives employed by the federal government to fight corruption, which we focus on in our analysis of how corruption affects firms and workers.

II.A Random Audits

The anti-corruption program that features municipality-level random auditing was initiated in May 2003. The purpose of the program is to identify and prevent corruption in the use of federal resources by local governments. The municipal audits are conducted by the CGU and focus on the allocation and use of federal funds that have been transferred to the municipality mostly during the two years prior to the audit. The program began by selecting 26 municipalities (one from each state in Brazil) per lottery, and later expanded to 60 municipalities per lottery. The program consisted of 39 lottery rounds of randomized audits, with replacement, over the 2003-2014 period. For transparency purposes, the lottery draw event invites the press, political parties, and the civil society to join and spectate. Only municipalities below a certain population threshold are eligible to enter the lottery, and state capitals are excluded. The population threshold was originally 100,000, but it was successively increased to 300,000 soon after the launch, and then rose to 500,000 for the remaining years of the program. As of 2014, more than 99% of Brazil's 5,570 municipalities were eligible, and 1,881 had been selected at least once.

The audit is performed by CGU auditors who travel to the municipality, manually review the municipalities' expenditures documents and, in most cases, physically inspect the execution of federal-funded programs. To limit corruption in the audit process, the auditors are hired competitively through a public examination and earn competitively high salaries. The audit starts immediately after the lottery draw and lasts about ten days. Following the fieldwork, the auditors write a detailed audit report, which can span up to 300 pages. The report documents any irregularities associated with the use of federal resources, together with any justification presented by local government officials for these irregularities and auditors' judgement on these justifications.

The reports are forwarded to the relevant administrative and judicial government agencies so as to proceed with the prosecution of any cases of corruption and pursue any administrative or legal fines and sanctions. In addition to the Federal Court of Accounts (TCU), the Federal Public Prosecutor's Office (MPF), the Federal Police (PF), and the municipal legislative branch, the results of the audits are released on the internet and to the media. According to Ferraz and Finan (2008), the news of revealed corruption will likely reach the public through the local radio. From the mayors' side, corruption commonly takes the form of frauds, usage of phantom firms, over-invoicing, and diversion of public resources. The firms involved in the irregularities are reported along with the local government officials in the audit reports.

There are several potential consequences for firms that are exposed by the auditing program. Firms can be barred from participating in future tendering processes for federal contracts. For example, Planam, an ambulance company with mafia connections, was found to overprice for the services provided. The company was subsequently declared illicit by the TCU and barred from future public proposals. Furthermore, exposed firms might have to pay penalties or return misused funds. In certain instances, firm owners might face judicial action.[5]

II.B Federal Procurement Suspensions

In 2010, in an effort to further promote transparency the CGU officially established the publicly available register of companies that have been declared ineligible to transact with government agencies (Cadastro Nacional de Empresas Inidôneas e Suspensas, CEIS). Specifically, CEIS provides information on the identities of firms and individuals that have been sanctioned and suspended from participating in public procurement tenders or entering into a contract with public agencies at any government level. In addition, CEIS includes information on the administrative sanctions imposed on the ineligible entities along with the relevant sanctioning public agency. The set of potential administrative sanctions includes (I) temporary suspension from bidding and contracting with

[5]According to the audit reports: "Irregular practices are forwarded to the Public Ministry and the TCU for penal action, the CGU for civil action of returning misused funds, and to Congress."

the public administration, (II) impediment of bidding and contracting with a particular public entity,or (III) declaration of unsuitability to bid or contract with the public administration.

The objective of CEIS was to consolidate and publicly disclose information on firms and individuals that have violated rules related to the execution of public contracts or the procurement process. While information on firms' sanctions or ineligibility was previously available, it was not indexed with other major publicly available databases. Over time, public entities started to explicitly introduce the non-inclusion of a firm in CEIS as a prerequisite of participation in a bidding process. For example, the construction company Vegeele Construções e Pavimentações Ltda was disqualified from a bidding competition in 2013 in the State of Rio de Janeiro after found to be ineligible in CEIS. The CEIS database naturally also includes some cases of firms that were exposed by the random municipal audits. For example, the audit report from the 24th lottery draw for the municipality of Montanhas/RN uncovered a collusion scheme among three construction companies. The companies were subsequently sanctioned by the TCU and added to the CEIS list.

An important limitation of CEIS is that information on firms and individuals that have been sanctioned is voluntarily provided by the public agencies. Therefore, there is potential selection in the firms included in CEIS. According to CGU's management reports, the database included 1,063 sanctioned entities at the end of 2008 and was subsequently increased to 3,757 in December 2010, and 5,708 in December 2011. In 2013, all federal entities were systematically providing information on CEIS and only Amazonas, Amapà, Rondônia, Goiàs, Paranà, Santa Catarina, Rio de Janeiro and Rio Grande do Norte at the state level were not updating CEIS with the relevant information. In January 2014, the Anti-Corruption Law (Law 12.846/2013) came into force that significantly expanded the reach of CEIS by requiring public entities from all branches (Executive, Legislative, Judiciary) and all government levels (Municipalities, States and the Federal Government) to maintain and update a registry of companies ineligible to transact with the public administration.

III Data

The main dataset used in the analysis is constructed from the combination of the CGU anti-corruption reports and the administrative matched employer-employee data on the Brazilian formal sector. In this section, we also describe the data on municipal and federal public procurement contracts we use to investigate mechanisms. A final dataset we rely on consists of confidential loan-level data on government funding to firms, which are obtained directly by the the Brazilian Development Bank (BNDES)

III.A A new dataset on firm-level corruption from audit reports

We construct novel measures of corruption starting from the CGU audit reports with the goal of understanding the link between corruption in local public spending and firms. We cover all 39 audit rounds and the 1,881 different municipalities randomly selected to be audited in the period 2003-2014.[6]

We read and code each irregularity manually, collecting information on each case and constructing a final dataset at the irregularity-firm level. We focus exclusively on irregularities where the tax identifier or the company name of a private-sector firm appear in the auditors' description of the case. This approach represents an important contribution relative to the previous literature using these data. Indeed, while Ferraz and Finan (2008), Brollo et al. (2013) and Zamboni and Litschig (2018) have used CGU audit reports to measure corruption, all these studies collect aggregate municipal measures, but do not identify the specific firms involved in the irregularities.

For each irregularity we record, among other details, the tax identifiers and names of the firms involved (e.g., both winners and losers of public procurement bids), the amount of the contract, the date of contract award and completion, and the extent of firms' involvement, with the aim of understanding whether it is the firm or the public official that is responsible for the irregularity. Given our focus on firms, we capture irregularities mostly in public procurement. Audited contracts that show no irregularity are not reported by the auditors, and hence are not observed. Similarly, we do not capture cases of politicians' embezzlement, such as the personal appropriation of funds that were supposed to be allocated to low-income families as part of federal cash transfer programs.

It is important to note that irregularities are extremely heterogeneous, which has implications for our analysis to the extent that some firms may benefit from corruption while other firms may be the victim of it. We return to this crucial point in Section V. We therefore pay special attention to collecting a measure aimed at capturing the extent of a firm's involvement in the irregularity. We distinguish between three main cases, by manually going over each irregularity description in the audit reports.

First, we label as *corrupt* all cases where a firm is actively involved in the corruption and there is clear evidence it illegally benefited from it. A typical case is one where firms paid a bribe or did not provide the goods or services described in the procurement contract (or provided them in an unsatisfactory manner). These clear-cut cases of corruption represent a minority –approximately 7%– of all irregularities we observe. All corrupt firms seem to be *inefficient* firms who benefit from the corruption, as evidenced by the lack or the low quality of their output.

Second, we consider firms who are *actively involved* in an irregularity, but where we cannot conclusively argue they are the perpetrators of a crime. The most common case

[6] For municipalities audited multiple times, we only consider the first audit, in chronological order. As discussed later in the text, results are robust to their exclusion from the analysis.

is one of over-invoicing for a specific good or product, which is a typical scheme where a firm is invoiced for an amount larger than the actual good or product sold to the government, so that rents can be extracted from public funds from either the politician, the firm, or both. The majority of other such cases involve a firm that wins a public procurement contract where auditors uncover that funds were mismanaged by the public official, for example because funds aimed at a specific government program were used to purchase goods from a firm in a completely different sector. Importantly, we do not directly observe any inefficiency related to these firms, as the evidence shows the quality of goods or services is satisfactory, unlike the case of corrupt firms.

Finally, we consider *passively involved* firms. These are firms who are mentioned in the audit report as being linked to an irregularity, but they seem to be the victim of it. In this case, the most common example is the one of losing bidders of an irregular public procurement process.

Of course, all these distinctions depending on the degree of involvement of firms are arbitrary. Indeed, they capture the inherent difficulty in attributing guilt in corruption cases, where it is often challenging even for prosecutors to charge specific parties. To provide further contextual evidence and make these measures more transparent, we also report a random sample of detailed examples of irregularities in Appendix A.2. Throughout the text, we will generically refer to all firms identified within the audit reports as "audited" firms.

III.B Additional datasets

Matched Employer-Employee Data

The firm and worker level information we use as outcomes in the analysis come mainly from the RAIS (Relação Anual de Informações Sociais) database, managed by the Brazilian Ministry of Labor. The RAIS has been used in a growing recent number of studies, and it is widely considered an extremely reliable Census of formal sector activity in Brazil (Dix-Carneiro, 2014). Except for the informal sector and a subset of self-employed businesses, its coverage is almost universal.

RAIS is a matched employer-employee dataset, which therefore allows us to track individual employment careers over time across both firms and business establishments. Individuals are tracked using a unique administrative worker tax identifier, similar to the social security number in the U.S.. In the data we also observe the tax identifiers of both the firm and the establishment of the worker, as well as the 5-digit industry they are operating in and the municipality they are located in. Similarly to other employer-employee matched data, such as the US Longitudinal Employer-Household Dynamics (LEHD) database, we have key information on the individual payroll and hiring and firing dates. Additionally, RAIS contains individual specific data on gender, nationality, age and education, as well as data on hours worked, reason of hiring and firing, and various contract details (such as temporary, short term, apprenticeship contracts). Each job in a given year is assigned an occupational category, which allows us to characterize the managers of each firm, as well as lower level occupational layers such as blue collar and white collar workers.

Public Procurement

We use three different sources of data on public procurement. Data on federal public procurement come from the Ministry of Planning, Budget, and Management (Ministério do Planejamento, Orçamento e Gestão - MP), covering the universe of contracts awarded by federal agencies of the government over the 2000-2014 period. We refer to Ferraz et al. (2015) for a detailed explanation of the data.

The second dataset comes from the Court of Auditors of the State of Sao Paulo (Tribunal de Contas do Estado de São Paulo - TCE-SP), and includes information on all public procurement contracts awarded by the 645 municipalities in the state of Sao Paulo over the 2008-2017 period. This dataset represents the most comprehensive municipality-level dataset on public procurement, since most other municipalities only started to report such information on specific transparency websites starting in 2016.

A third dataset allows us to identify suspensions of firms that are due to irregularities in public procurement. As discussed in the previous section, the data come from the National Registry of Ineligible and Suspended Companies (Cadastro Nacional de Empresas Inidôneas e Suspensas - CEIS), also referred to as the "public procurement blacklist." These data cover the period 2007-2017.

III.C Sample Selection and Descriptive Statistics

We collect 14,316 tax identifiers of firms that appear in all available audit reports. Figure I shows the number of audited firms over time. We find that the program was particularly intense in its first few years, with close to 1,500 firms being involved in irregularities at its peak in 2005. Approximately 1,000 firms appear in our dataset during the central phase of the program from 2006 to 2010, while the number drops significantly after that, in line with the reduced intensity of the CGU program. We do not observe significant differences in the extent of involvement of firms over time, with a large and mostly equal share of passively involved and actively involved firms, and a small share of corrupt ones across the entire sample period.

To construct our analysis sample, we start by matching audited firms to the RAIS administrative database using the tax identifier. We match 9,454 of firms to RAIS, but the number drops to 4,085 when we restrict the focus to the years in which the local procurement data is available (i.e., post 2008).[7] We then focus only on firms that have at least one employee in each of the three years leading up to the audit, and the year of audit: this reduces the sample to 2,910 firms. We then drop 1,604 firms that do not have a matched control firm, as discussed in Section IV. As a result, our most restrictive analysis sample includes a total of 1,306 audited firms.

In Table I, we report summary statistics on the final sample of audited firms using data in the three years before the audit. Firms have a mean of 47 employees and a median of 12, both larger than the population averages of 16 and 3 employees.[8] The average total monthly wages amount to USD 525. A non-trivial share of firms receive government-subsidized loans from BNDES (17%) and federal procurement contracts (5%). These loans and government contracts can be quite substantial, with means of USD 491 and USD 1,297, respectively. For simplicity, we classify the firms into bins depending on size, as shown in Figure II, which also illustrates how the size distribution of firms in our data evolved over time and across firms involved in irregularities with the local governments to different extents.

Table II reports the distribution of firms across sectors, compared to the national distribution in Brazil. 59% and 19% of firms are in the Retail and Construction sectors (column 1), respectively, compared to 40% and 7% in the economy (column 3). On the other hand, Services are under-represented. This distribution reflects the higher prevalence of these sectors in public procurement more generally and highlights the importance of accounting for sectoral heterogeneity when estimating the effects of anti-corruption policies. Regarding the size distribution, large and medium firms are more prevalent among the audited firms relative to the population, while small and micro firms are underrepresented. This is consistent with the fact that larger firms are more likely to bid and receive local procurement contracts.

Finally, as shown in Figure III, we find that the vast majority of audited firms are located *outside* of the audited municipality.[9] Indeed, we find that 72% of firms are registered outside the audited municipality, consistent with the fact that several players in public procurement are larger multi-region firms.

IV Identification Strategy

In this section, we present our main identification strategy to estimate the effect of exposure of corrupt practices on a set of firm-level outcomes. In Section V.D we briefly

[7]The reasons for the imperfect matching can mainly be linked to two issues: (i) there are formal firms that are not included in RAIS, such as firms without employees (e.g., sole proprietorship) or self-employed individuals (typical for example of consultancy services hired by the government); (ii) there are mistakes in the tax identifier in the audit reports, due for example to misspellings of the auditors.

[8]The population averages are computed using the 2008 firm-level RAIS database.

[9]The location is the physical location of the establishment for single-plant firms. For multi-plant firms, we define it to be the headquarter of the firm.

discuss how our empirical specifications are adapted to estimate worker-level outcomes.

We define exposed firms as those mentioned in the audit reports, as described in section III. Although the timing of firm exposure is plausibly exogenous due to the random nature of the audits, firms that do business with local governments might be selected on multiple dimensions. The main challenge we face is thus to identify a plausible control group for the exposed firms, capturing how those firms would have performed in the absence of the CGU anti-corruption program.

To make progress on this front, we complement a dynamic difference-in-difference specification with a matching strategy based on detailed data on firm characteristics. A key aspect of our strategy is that we are able to match each exposed firm with a non-exposed firm that is also involved in public procurement with local governments. To identify such firms, we rely on contract-level data covering all public procurement contracts with the municipalities of the State of Sao Paulo. This data allows us to select counterfactual firms that – similarly to the exposed ones – provide goods and services to local governments and that had a procurement contract in place in the year of the audit. The existing literature has documented that firms that receive a procurement contract tend to experience a temporary increase in size after the assignment of the contract, as shown in Ferraz et al. (2015) and Carrillo et al. (2018). Thus, matching on existing access to local procurement alleviates the concern that our estimated effects are driven by the dynamics of firm growth when obtaining procurement contracts rather than by the effect of the anti-corruption program itself.

Let us now describe how we construct the sample of firms used in our empirical analysis and how we select the counterfactual firms in more detail. In addition to matching on access to local procurement contracts, we rely on detailed firm-level data sourced from RAIS to match on a set of observable characteristics. More specifically, we implement a Coarsened Exact Matching (CEM) procedure (Iacus et al., 2012), which consists of two rounds of sequentially less restrictive matching requirements based on a firm sector of operation, size, and characteristics of its location. In the first round, we match exposed firms with potential controls that: (i) operate in the same *five*-digit sector according to the CNAE classification, (ii) are in the same ventile of employment and payroll for the three years before the audit, (iii) are in the same quartile of the distribution of the following municipality characteristics: total number of plants, total employment, and total payroll. For both exposed firms and potential control firms, we restrict our sample to firms located in municipalities that were never audited by the CGU during the period under study. This last restriction is crucial to avoid any potential confounding effects derived from the impact of the auditing program on the local economy and political context, as documented by previous work (Ferraz and Finan, 2008; Colonnelli and Prem, 2020). In the second round, we then relax the sector requirement to firms operating in the same *two*-digit sector, and we match on deciles, rather than ventiles, of the empirical distribution of firm characteristics.

We apply a similar matching procedure to study the effect of CEIS suspensions. In this case, we first identify potential control firms among those that have a federal procurement contract in place in the year of the suspension of the exposed firms, and then apply a CEM procedure on observable characteristics similar to the one described above.[10]

In both matching strategies, when multiple potential control firms are found for a given exposed firm, we select as the counterfactual firm the one with the closest propensity score. The propensity score is computed based on a linear probability model that includes lagged employment levels.[11] At the end of the full matching procedure, we were able to match 1,306 exposed firms in the CGU audit program and 165 exposed firms in the CEIS program. In this case, the time-series variation is given by the year of inclusion of a firm that committed an irregularity in the CEIS database.

V Results

V.A Effects of random audit exposure on firm size

In this section, we provide direct empirical evidence on the impact of the anti-corruption programs on firm-level outcomes. We start by documenting the effect of exposure by the random auditing program on firm size using the following specification:

$$\log(1 + L)_{it} = \alpha_i + \alpha_t + \beta_1 Post_{it} + \beta_2 (Post_{it} \times 1(Exposed)_i) + \varepsilon_{it} \tag{1}$$

Our main outcome variable, firm size, is defined as the natural logarithm of one plus the number of employees of firm i at the end of each calendar year t. The dummy $Post_{it}$ captures the years after the audit, while $1(Exposed)_i$ is an indicator function equal to one for exposed firms and zero for the matched control firms as described in section IV.

Table III reports the results. As shown in column (1), we find that firms exposed by the CGU random auditing program experience, on average, an increase in size in the years after their exposure relative to the control group. The magnitude of the estimated coefficient β_2 indicates that exposed firms experience a 4.8 percent larger increase in size after being exposed by the audit compared to their control.

[10] In the first round, we match exposed firms with potential control firms with the following characteristics: (i) operate in the same five-digit sector according to the CNAE classification, (ii) are in the same ventile of employment and payroll in the year before the suspension, (iii) are in the same quartiles of the distribution of the following municipality characteristics: total number of plants, total employment, and total payroll. With respect to point (ii), the reason why we match only on characteristics the year before, rather than each of the three years before, is that the sample is small and would be reduced too severely otherwise. In a second round, we relax the sector requirement to firms operating in the same two-digit sector, and use deciles for firm level characteristics.

[11] Consistent with the matching approach, we use three lags for the CGU random audit firms and one lag for the CEIS firms.

To explore the timing of the effects as well as to study the pre-audit trends, we estimate the following dynamic specification:

$$\log(1+L)_{it} = \alpha_i + \alpha_t + \sum_{k=-3}^{k=+3} \alpha_k 1(t=k) + + \sum_{k=-3}^{k=+3} \beta_k (1(t=k) \times 1(Exposed)_i) + \varepsilon_{it}. \quad (2)$$

The estimated coefficient β_k are reported in Figure IV Panel A. We find no differential pre-audit trends between audited firms and their controls, in line with our reliance on a dynamic difference-in-differences matching estimation strategy. In terms of the post-audit dynamics, we find that the effect of the anti-corruption program materializes in the same year in which the firm is exposed to it (year 0), intensifies in the year following exposure (year +1), and then stabilizes in terms of magnitude in the two following years.

To sum up, the results reported in column (1) of Table III indicate a relative increase in size of firms exposed by the random auditing program. However, as discussed in Section III and shown in Table II, a large share of firms mentioned in the audit reports can be considered, in fact, victims of corruption, while only around 7% were clearly benefiting from the exposed corrupt practices. Thus, in order to shed light on the potential mechanisms behind this positive average effect, in the next section we start by studying the heterogeneous effects by "type" of exposure in the random auditing program using the detailed information on the type of irregularity.

V.B Heterogeneous effects by type of exposure and the impact of public procurement suspensions

Firms whose names appear in the audit reports of the random audit program can be classified in three categories depending on their degree of involvement in the exposed corruption, as outlined in Section III.A: passively involved, actively involved, and corrupt. To study how the effect of the random audit program on firm size differed by type of exposure, we estimate the following specification:

$$\begin{aligned} \log(1+L)_{it} &= \alpha_i + \alpha_t + \beta_1 Post_{it} + \beta_2 (Post_{it} \times 1(Exposed)_i) \\ &+ \beta_3 (Post_{it} \times 1(Exposed)_i \times 1(Active)_i) \\ &+ \beta_4 (Post_{it} \times 1(Exposed)_i \times 1(Corrupt)_i) \\ &+ \beta_5 (Post_{it} \times 1(Active)_i) \\ &+ \beta_6 (Post_{it} \times 1(Corrupt)_i) + \varepsilon_{it}. \quad (3) \end{aligned}$$

The results are reported in column (2) of Table III. The coefficient on the main interaction with the exposure dummy β_2 captures the effect of the anti-corruption program on the passively involved firms, which represent the excluded category. The magnitude of the point estimate is larger than the one reported in column (1) of Table III, indicating that passively involved firms experience a larger increase in size than the average firm exposed by the anti-corruption program. In particular, passively involved firms experienced a 7.1% larger increase in size with respect to the control group after exposure, against the 4.8% average effect documented in column (1). Passively involved firms that appear in the audit reports are often firms that were victims of the corruption scheme, such as competitors put at a disadvantage by a rigged bidding process for procurement contracts. The results show that these firms benefited by the corruption crackdown.

Interestingly, we find that firms reported as actively involved in the corruption scheme experience a smaller but not significantly different increase in size. We will investigate the potential mechanisms of this result in Section V.C. On the other hand, corrupt firms experience a strong and significant decline in employment after their exposure in the audit reports. The sum of the estimated coefficients β_2 and β_4 indicates an 20% relative decline in size for corrupt firms.

In column (3) of Table III, we turn to study the impact of the CEIS transparency program –which focus on suspending firms from participating in public procurement because of corruption– on firm size. We think of this as a useful benchmark for the impact on firms of an anti-corruption program that involves hard sanctions. To estimate these effects, we use the same specification described in equation (1), where the indicator function for exposure is equal to one if a firm was reported in the CEIS dataset of corrupt firms, and zero otherwise. We find a large and negative effect of this anti-corruption program on firm size, with employment in exposed firms declining by a staggering 90% more than in the control firms in the post-exposure period.

We also explore the heterogeneous effects by type of exposure in a dynamic specification. The results are reported in Figure IV, panels B to E. As shown in the figure, the positive effects on passively involved firms materializes already in the year of exposure, and increase in magnitude in the first three years before stabilizing. The effect on actively involved firms is also positive although not statistically significant at standard levels and smaller in magnitude with respect to the passively involved firms. The large and negative effect on the corrupt firms materializes with a slight lag from the time of exposure, while the effect for suspended firms materialized in the year of suspension.

To sum up, the results reported in Table III and Figure IV show that the average effect masks a large heterogeneity. The effect of the random audit anti-corruption program on firm size has been heterogeneous across firms that were exposed by the audit in different ways. In particular, the magnitude of the estimated effect is monotonically decreasing with the degree of involvement of firms in the corruptions schemes: from the positive effect of the passively involved to the large and negative effect of the corrupt firms. Moreover, the effects are extremely large and the most negative for firms that have been suspended from participating in public procurement as part of the CEIS program.

V.B.1 Firm survival

To evaluate the impact of the anti-corruption programs on the extensive margin of firm activity as well, in Table IV we study the impact of both audits and CEIS on the probability of firm exit. We find that the random auditing program had no significant impact on exit probability of exposed firms. As shown in column (2), we find no significant heterogeneous effects on exit by degree of firm involvement in corrupt practices, although the magnitude of the point estimates on the interaction terms is increasing with the degree of firm involvement in corrupt practices. On the other hand, as shown in column (3), firms exposed by the CEIS program are also 12.5 percentage points more likely to exit in the post-exposure period relative to the control group.

V.C Mechanisms

In this section, we explore the potential mechanisms via which the anti-corruption programs studied in this paper can affect firm performance. On the one hand, the revelation of misconduct can harm firms responsible for the misconduct because it leads to punishment by the government, for example via reduced access to government contracts or loans, or by other stakeholders, perhaps because customers or business partners do not want to be associated with a corrupt firm. Such channels may explain why (highly) corrupt firms shrink in size after exposure. On the other hand, understanding why the revelation of corruption leads to the growth of a large share of exposed firms is more challenging, as it is puzzling that even the set of firms who are somewhat actively involved in the misconduct grow after the corruption is exposed. One possibility is that the revelation of corruption coincides with the punishment of the corrupt government officials the firm interacts with (Avis et al., 2018), which will reduce the firm's cost of doing business in future interactions. This is plausible considering that firm-government relationships are sticky (Ferraz et al., 2015). In such a case, we would expect to see firms doing the same amount or even more business with the government. Another possibility is that the revelation of corruption cuts a firm's access to government business, and forces the firm to compete in the private sector. If dealing with the government makes firms operate inefficiently (Dal Bó and Rossi, 2007; Bertrand et al., 2018), this channel might explain at least part of our findings.[12]

We therefore focus on three mechanisms. First, we study how exposure affects firms' ability to access local and federal procurement contracts. Second, we study the impact on firms' access to subsidized government credit by using data on loans originated by the Brazilian development bank BNDES. Notice that our evidence on these first two

[12]Notice that all these mechanisms would be consistent with our firm-level heterogeneous effects to the extent that corrupt firms (exposed by the audits or the CEIS program) are inefficient firms who operate *because* of the corruption, as discussed in section III.A, while passively and actively involved firms are productive firms who operate *despite* the presence of a corrupt environment.

mechanisms will not be able to establish whether any observed changes were driven by firms' own decisions or were forced by government decisions, as that would require data on procurement bids or loan applications that are unavailable. For example, local governments might decide not to do business with firms whose names were disclosed in the audit reports fearing the political repercussions. Alternatively, firms themselves might decide to rely less on government contracts for future business after their name has been published in audit reports. Finally, we investigate whether exposure in the audit reports affected firms' investment strategy. The rationale of the latter exercise is to study how firm internal organization and behavior might have adjusted in response to the disclosure of corporate misconduct.

V.C.1 Access to procurement contracts

We start by studying whether being exposed by anti-corruption programs has an impact on firms' ability to obtain new procurement contracts. To this end, we rely on data on procurement contracts from municipalities within the state of Sao Paulo – hereafter defined as "local" procurement contracts – as well as from the federal government. Both dataset are described in section III.

The results are reported in Table V. In columns (1) and (2) we estimate equation (1) and use as outcome variable an indicator variable capturing whether the firm obtained a procurement contract in a given year. We find that firms exposed by the random audit program are on average 2.4 percentage points less likely to receive local procurement contracts after exposure, which represents a reduction in the probability of receiving a contract of 5% with respect to the mean. In Table A1 we split the dummy for having a local procurement by having a local procurement contract from the audited municipality, or having one from another municipality in Sao Paulo. Interestingly, we find that the decrease is mainly driven by a reduction in the probability of having a contract from the audited municipality instead of a reduction from another municipality, i.e., 45% versus 6% of the mean, respectively.

In the case of receiving a federal procurement contract, we find that exposed firms experience a reduction of 2 percentage points in the probability, which represents a decrease of 40% with respect to the mean. In columns (3) and (4), we study how these effects vary by type of exposure. We find that the reduction in access to procurement contracts is stronger for the *Corrupt* firms in our sample, but also present for both the *Actively* and *Passively involved* firms. This result is consistent with both local and federal governments deciding to reduce their business with firms whose names were disclosed in the audit reports –independently of the specific context in which they are mentioned. These findings may have important policy implications to the extent that firms who are not the perpetrators of corruption may suffer negative consequences after the disclosure of their involvement in corrupt public procurement deals.

The large effect on *Corrupt* firms is consistent with what we document for firms exposed by the CEIS program in columns (5) and (6). With respect to CEIS, we find that access to local and federal procurement contracts is entirely cut. These findings are expected considering that, by design, inclusion in CEIS implies a formal punishment by the government.

The findings in this section rule out a possible explanation for why the majority of firms exposed by the audits grow. In fact, government access to procurement contracts is reduced for firms who are not responsible for the corruption, as well as for firms who are actively involved but might not have benefited from the corruption. As a result, it is unlikely that the growth of these firms is simply due to the potentially lower corruption, and hence costs, that they face in their interaction with the government, as according to such a channel we should have expected these firms to obtain the same amount or even more public procurement contracts.

V.C.2 Access to government funding

Next, we study whether being exposed by anti-corruption programs affects firms' ability to obtain government funding. To this end, we use data on credit contracts originated from BNDES – the development bank controlled by the Brazilian government and one of the largest lenders in the country when it comes to corporate loans – and match them with our firm-level dataset.

The results are reported in Table VI. We start in columns (1) to (3) by studying the average effect of firm exposure to the random audit program. Overall, the results are consistent with the absence of any "punishment" on exposed firms when it comes to allocation of credit via BNDES. In fact, we find a positive and significant effect on the number of loans, with no effect on the loan balance and average interest rate. Reconciling this positive effect on access to government funding with a negative effect on government contracts is possible considering that audits are directly used by local government entities in screening potential contractors, while financial institutions like the BNDES are less affected by reputational considerations linked to the municipal audits. In this sense, the positive average impact of the audits on BNDES loans might suggest that firms are themselves requesting more loans for long-term investments, even though we cannot conclusively assert that without data on loan applications.

Next, in columns (4) to (6), we explore the heterogeneous effects of exposure on access to subsidized lending by the government. We find that passively involved firms experience a significant increase in the number of loans from BNDES, although we find no significant change in the overall amount borrowed. The effects on actively involved and corrupt firms are not significantly different from those on passively involved firms, although the point estimates are negative and increasing in absolute value with the degree of exposure to corrupt practices. Interestingly, we find that corrupt firms experience a significantly higher increase in the average interest rate charged by BNDES after their exposure in the audit reports, which points to the presence of a negative signal on the firm "type" that the lender receives from the information disclosure.

Finally, in columns (7) to (9), we study the effect of exposure to the CEIS program. In this case, our results show negative and significant effects of firm exposure on government funding. The magnitude of the estimated coefficients is consistent with exposed firms experiencing an almost complete cut in BNDES loans in the post-exposure period relative to the control group. Of course, the documented effects on credit quantity could be explained by both a relative decline in demand from exposed firms, or a relative decline in supply from the government lender. In this sense, the lack of any differential change in interest rate would be consistent with both effects being at play.

V.C.3 Change in firm growth strategy

Overall, the evidence on the first two mechanisms is consistent with a negative effect of exposure on corrupt firms, i.e., firms that clearly benefited from the corruption schemes and were exposed by the random audit program, or firms who were formally sanctioned by the government through the CEIS program because of severe cases of corruption prosecuted in court. As discussed, the evidence so far also suggest the growth of many of the exposed firms reported in section V.B is unlikely due to lower corruption implying lower costs in their interaction with the government. This leaves the possibility that the presence of corruption made certain productive firms operate inefficiently. Existing literature has shown, for instance, that firms that rely more on government contracts tend to grow slower and invest less in tangible and intangible capital (Cohen and Malloy, 2016). In our setting, we hypothesize that exposed firms might change their internal growth strategy, from one in which they focus on securing government contracts in the pre-audit period, to one in which they invest to compete in the market for private demand after the revelation of corruption. Such a channel is extremely difficult to test using solely administrative data, but we can at least provide some suggestive evidence in its favor.

Our employer-employee dataset does not contain information on different types of investment or technology adoption. Thus, to explore this question, we focus on whether exposure in audit reports has an effect on firms' skill upgrading. We measure skill upgrading as average education level (in years of education) of the workforce of a given firm and with the share of high-skill workers (defined as share of individuals that completed a high-school degree). In the presence of capital-skill complementarities – a plausible assumption in our setting – skill upgrading can be considered a proxy for capital investment. In fact, measures of skill upgrading like ours have been shown to be good proxies for a firm's long-term investment decisions (Ferraz et al., 2015), and have been extensively discussed in the literature on firm and economic growth (Caliendo and Rossi-Hansberg, 2012).

The results of the internal organization analysis are reported in Table VII. Columns (1) and (2) show that, on average, firms exposed by the audit program experienced no significant change in either the average level of education or the share of high-skill workers among their employees. However, in columns (3) and (4), we find that these effects vary depending on the type of exposure. In particular, actively involved firms experience a 0.11 larger increase in average years of education among their employees compared to their control, which corresponds to an increase of 1% with respect to the mean. Moreover, we find a 2.7 percentage points relative increase in the share of high-skilled workers in their labor force compared to their control. This evidence can contribute to rationalize our findings on the positive employment effects of exposure for actively involved firms. In particular, it is consistent with such effects being at least in part driven by a shift of exposed firms' growth strategy from reliance on government contracts to changing the investment strategy so as to more effectively compete with other firms in the market for private clients.

V.D Worker-level evidence: employment status and wages

Our analysis so far has focused on the impact of anti-corruption initiatives on firm growth and on firm's access to procurement contracts and funding. Another disciplining mechanism is the labor market for employees of audited and sanctioned firms. The idea of labor market punishing misconduct through a "reputation" channel goes back at least to Fama (1980) and Fama and Jensen (1983). Consistent with such an argument, prior literature has shown that directors lose board seats and the market for directors disciplines them when they are involved in mismanagement or misconduct (Srinivasan, 2005, Fich and Shivdasani, 2007, and Bennedsen et al., 2019). Moreover, Karpoff et al. (2008) finds that CEOs and cupable executives and employees face career penalties when involved in financial misrepresentation. In the setting of financial advisors, Egan et al. (2019) find increased turnover rates for advisors who previously engaged in misconduct, with the labor market penalty being higher for more severe acts of misconduct.

On the other hand, there is evidence that there might be tolerance for misconduct in the labor market. Agrawal et al. (1999) find that turnover of CEOs and outside directors is unchanged after fraud. Moreover, Egan et al. (2019) finds that 44% of advisers who lost their jobs after misconduct find employment in the industry within a year. Thus, there is evidence that the hiring of employees with prior misconduct partially undoes some of the firm-level discipline. It is also possible that employees who have worked in firms involved in misconduct, and especially managers and high-level employees, might be desirable candidates because of their experience in responding to such audits or related legal matters. For example, Helland (2006) suggests such a channel is at play with respect to directors accused of misconduct in his sample.

To investigate the extent of labor market discipline in our context, in this section we exploit worker-level data to study the impact of firm exposure by the anti-corruption initiatives on employment status and labor income.

We focus on those workers that were employed at the time of exposure and estimate a worker-level version of equation (1) with three main outcomes. The first outcome is an indicator variable capturing the employment status of each worker in each year. This indicator is equal to one if an individual is employed at the end of the year and zero otherwise. Second, we look at the average monthly wage of each worker, independently of whether the individual is employed or not after the year of exposure, and imputing zeros for the unemployed.[13] Our third dependent variable is the average monthly wage of each individual, conditional on the individual being employed at the end of the year.

The results are reported in Table VIII. We start by presenting results for all workers in panel A. Next, in panel B and C, we split workers between those employed in managerial and those employed in non-managerial positions, given that market discipline might be different between the two groups. Managers of exposed firms might suffer a higher reputational cost of being associated with corrupt practices because they are often considered to have more influence on company decisions, including the decision to embark in corrupt practices to win a procurement contract from the local government. On the other hand, managers' experience in dealing with the government might be considered a valuable asset in the labor market, providing them with good outside options after the revelation of misconduct.

By and large, we find that workers that were employed by firms exposed by the CGU random auditing program experience no significant changes in their probability of being employed, nor on their average pay, either conditioning or not conditioning on being employed. This indicates that exposure by the random audit program did not significantly affect the employability or the compensation of the workers employed at the time of exposure relative to those employed by the control group. Notice that this is somewhat consistent with the average growth in size of exposed firms following the audits, but it is not obvious ex-ante considering that we do find some evidence that exposed firms suffer also negative consequences that might be driven by reputational concerns, as in the case of the reduced access to government contracts.

On the other hand, we find negative and significant effects of the CEIS program on all of our main worker-level outcomes. More specifically, workers that were employed by firms exposed under the CEIS program experience a 7.5 percentage points decline in their probability of being employed in the years after their firm has been exposed and a 45% decline in their average wages. These results indicate that the CEIS program had real and significant effects on workers' income. Figure V shows that these effects materialize at the time their employer was exposed by the anti-corruption program and were still large

[13] That is, this outcome variable is constructed as the natural logarithm of one plus the average monthly wage of the individual.

in magnitude three years after exposure. The figure shows no clear sign of convergence to the pre-exposure level in both the probability of employment and the unconditional labor income.

Next, in panels B and C, we split our sample into workers employed in managerial versus non-managerial positions. We find no heterogeneous effects for workers employed by firms exposed by the CGU random auditing program: both managers and non-managers experience small and non-significant changes in their employment probability or compensation in the years following exposure of their employer. In the case of the CEIS program, we find negative and significant effects for both managers and non-managers, with the effect on managers being on average larger in absolute value. For example, our estimates indicate that managers of firms exposed by the CEIS program are on average 10.2 percentage points less likely than the control group to be employed after exposure, against the 7.4 percentage points decline for non-managers. This translates into a large effect on average unconditional wages, which decline by about 71% for managers and 44.5% for non managers. Notice that, in the case of managers, even those that are still employed experienced a significant 6.8 percent relative decline in their average wage relative to the control group.

Overall, our results on the effects of the anti-corruption initiatives on employees mirror our firm-level findings. Employees of firms exposed by the random auditing program, which were often victims of the corruption firms, are not negatively affected by the disclosure of corrupt practices, independently of their position within the firm. On the other hand, employees of firms who actively engaged in the corruption scheme, as evidenced by the inclusion in the CEIS registry, suffer significant negative consequences in the labor market. Together, the evidence is consistent with that of Srinivasan (2005) and Egan et al. (2019), among others, who show that the labor market punishment is stronger in more severe cases of misconduct. The CEIS evidence of negative effects on both managers' and lower-ranked workers' careers further indicates potentially strong negative spillovers of misconduct onto workers, even if the latter are unlikely to be at fault for the corporate misdeeds.

VI Concluding Remarks

Shady deals in the assignment of government procurement contracts are widespread in many countries around the world, especially in developing economies. Such practices can distort the efficient allocation of production factors to firms and have implications for firm growth. Understanding how corruption affects firms is therefore key to better design policies to fight corruption.

In this paper, we use micro-data from Brazil to trace the impact of exposing corrupt practices on the exposed firms and their employees. We isolate variation in firm-level exposure of corrupt practices using randomized anti-corruption audits and a public procurement transparency initiative. To understand how corruption affects firms at the micro-level, we construct novel measures of corruption at the firm-level using the audit reports. We further study a large transparency initiative aimed at uncovering severe cases of corrupt firms doing business with the federal government. We document that firm involvement in corruption is extremely heterogeneous; most firms appear as victims of corruption, some firms have an ambiguous involvement, while others are culprits. Consistent with this heterogeneity, we uncover wide variation of the effect of anti-corruption crackdown depending on the degree of firm involvement in corrupt practices. Firms that are victims of corrupt practices experience higher growth, while firms that are involved in corrupt practices decline relative to their peers.

In order to understand the mechanisms behind these effects, we first examine how firm-level access to procurement contracts and government loans is affected. We show that, after exposure, exposed firms on average lose access to procurement contracts, though to different degrees depending on their degree of involvement. On the other hand, we document mixed effects on access to finance, with no evidence of punishment when it comes to loans originated by the Brazilian development bank BNDES. We find, instead, suggestive evidence that exposed firms adapt their internal organization after the revelation of corruption, which might be partly responsible for the positive effects on firm growth we observe for a large set of audited firms. Finally, we examine how the anti-corruption crackdowns affect employees. In line with our firm-level findings, we find a large degree of heterogeneity in worker outcomes, with large losses for the employees of highly corrupt firms. Taken together, our results indicate a large heterogeneity in the affects of unavailing corruption, which is of crucial importance to both policy and theory focused on the intersection between corruption and firms.

We see several avenues of future research. First and foremost, more work is needed to fully identify the links between corruption and firms' growth strategies, and to understand the specific ways through which operating in a more or less corrupt environment might affect firm behavior. Indeed, our evidence on firm's growth through organizational upgrading remains suggestive. Survey data and experimental designs might help further unpack these and other mechanisms, due to the difficulties to test them using administrative data only. We also think it is of crucial importance to understand why firms decide to do business with the government in the first place, even in contexts where engaging in public procurement might entail high costs. A large literature on management practices shows that firms might not adopt efficiency-enhancing changes to their operations simply because they lack information or because they have not been exposed to alternative scenarios (Bloom et al., 2013; Cai and Szeidl, 2018). We believe such a path linking firm-government interactions to information frictions to be particularly promising.

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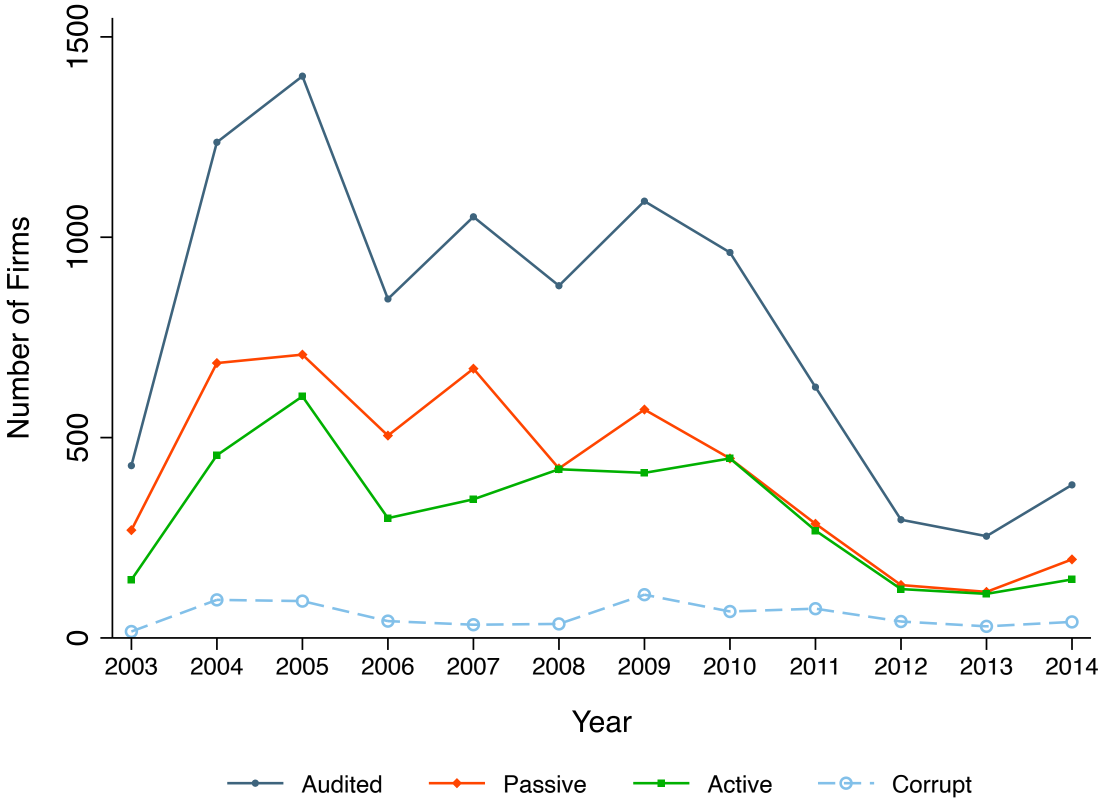
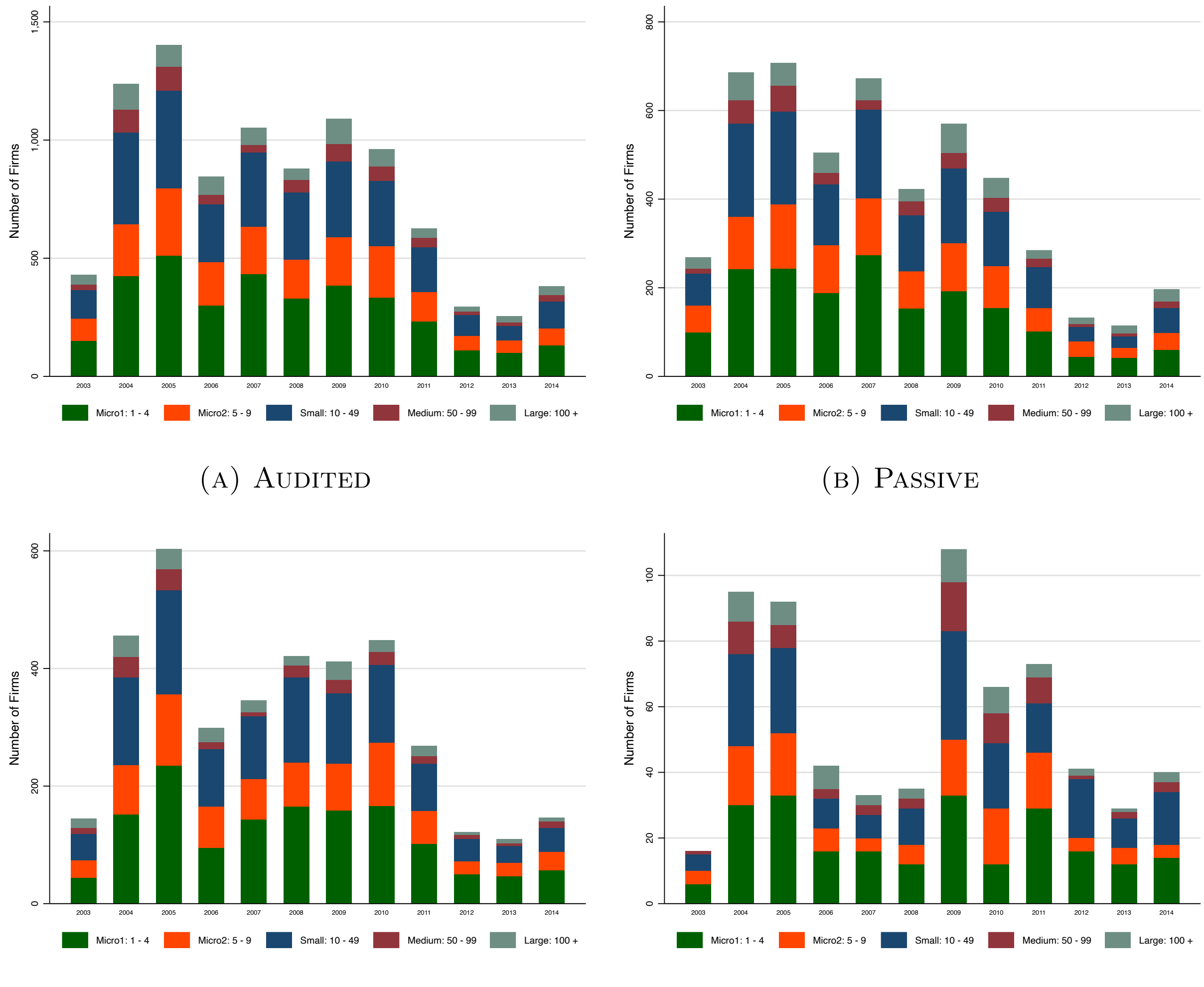
Figure I: Number of audited firms by year



Notes: Number of plants audited by year and type of exposure by the audit from 2003 to 2014.

Figure II: Audited firms by size, type, and year



(a) Audited

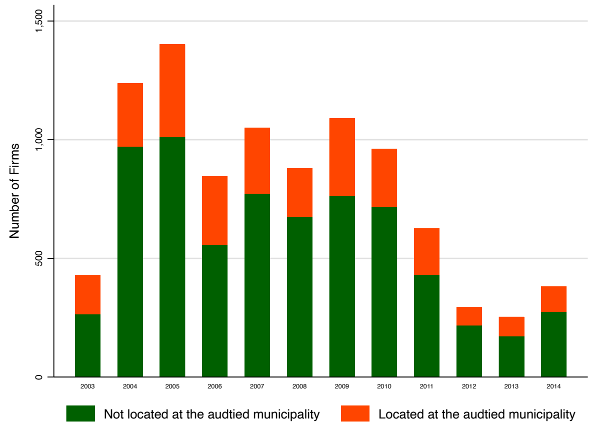
(b) Passive

(c) Active

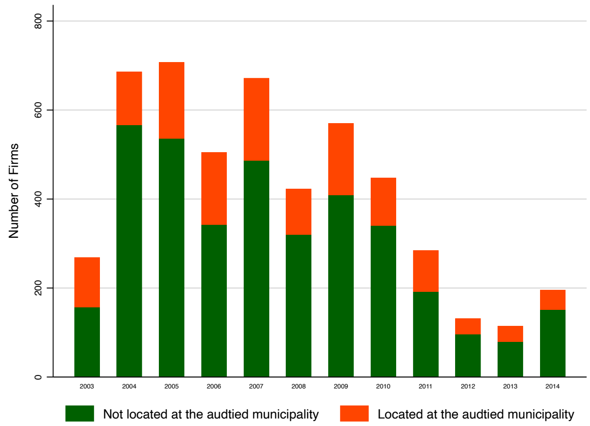
(d) Corrupt

Notes: This figure shows the distribution of plants based on size categories over time for all audited plants, as well as by the type of exposure they had.

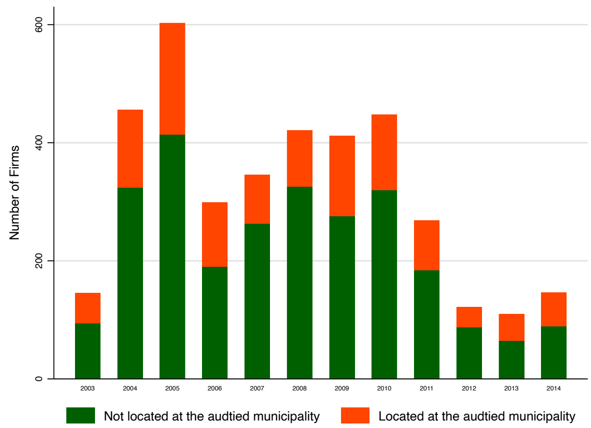
Figure III: Audited firms by location, type, and year



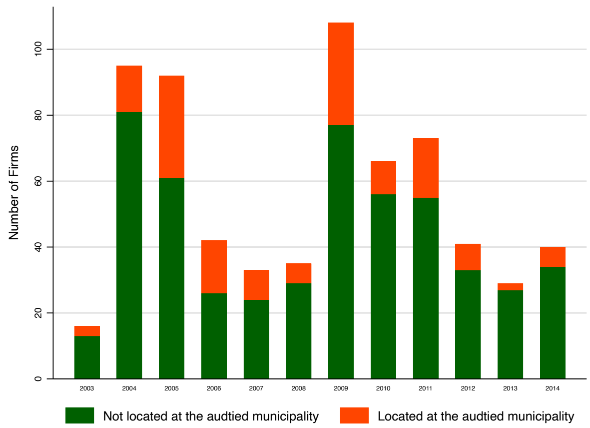
(a) Audited



(b) Passive



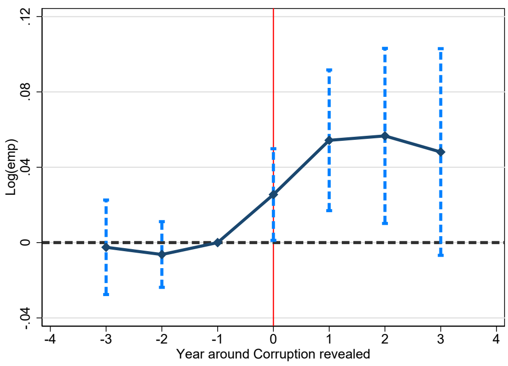
(c) Active



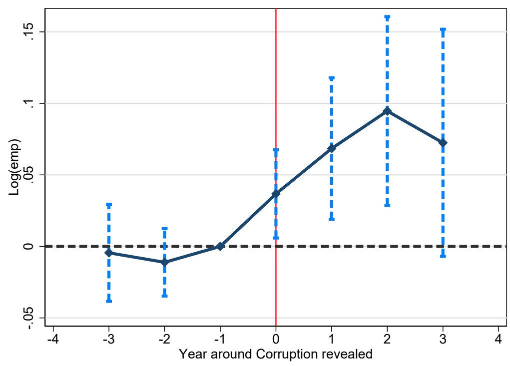
(d) Corrupt

Notes: This figure shows the distribution over time of plants based on whether they are located inside or outside the audited municipality. We present the distribution for all audited plants, as well as by the type of exposure they had.

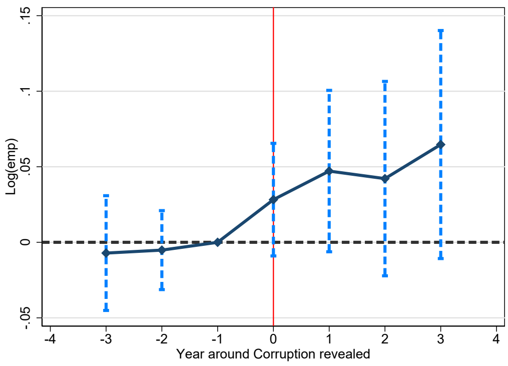
Figure IV: Audits, suspension, and employment



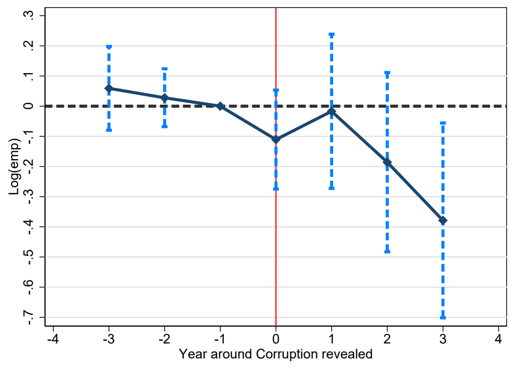
(A) Audited



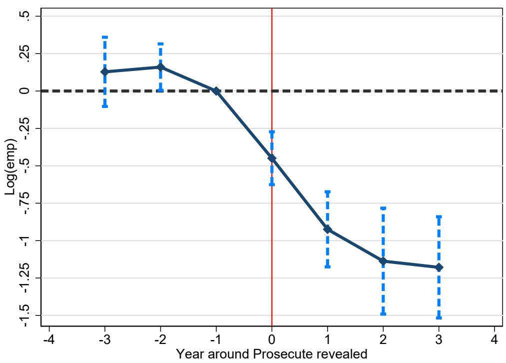
(B) Passive



(C) Active



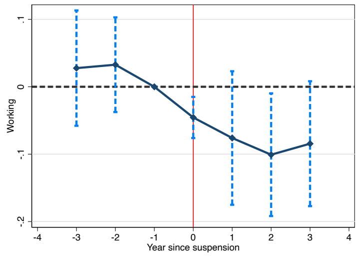
(D) Corrupt



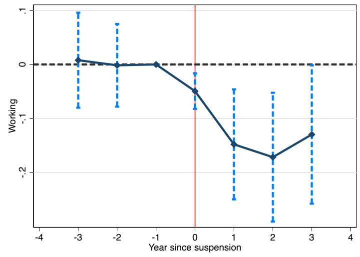
(E) Suspended

Notes: **Notes**: This figure present the estimation from the following specification: $Log(1 + Emp_{fpt}) = \alpha_f + \alpha_t + \sum_{k=-3,\ldots,3}^{k\neq 0} \alpha_k * I\{t = k\} + \sum_{k=-3,\ldots,3}^{k\neq 0} \beta_k * I\{t = k\} * Corrupt + \epsilon$, where it controls for firm fixed effect and year fixed effect. The dependent variable is the logarithm of one plus employment. In Panels A to D, we present the effects of the audits, while Panel E shows the effect of suspension. The firms in the audit regression sample are audited firms and their matched control. The firms in the suspension regression sample are suspended firms and their matched control. Section IV details the matching method.

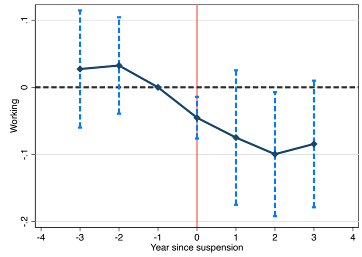
Figure V: Suspension and employees



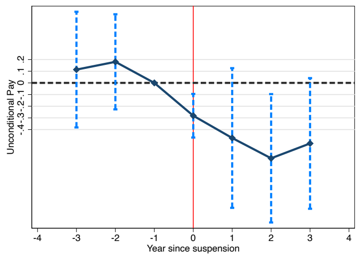
(a) Working: All



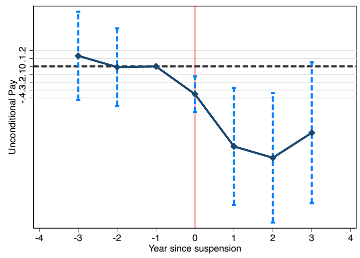
(b) Working: Managers



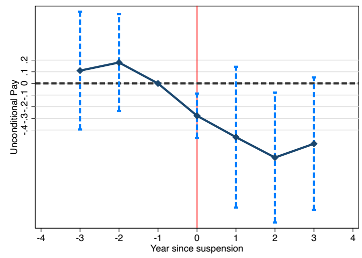
(c) Working: Non-Man



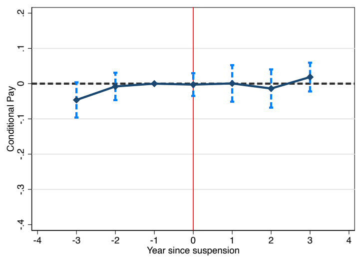
(d) Pay: All



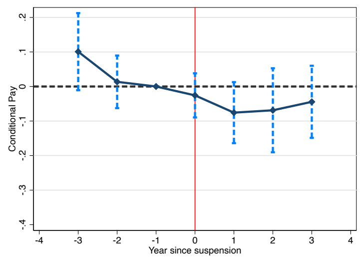
(e) Pay: Managers



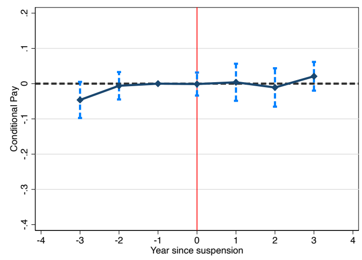
(f) Pay: Non-Man



(g) Pay cond: All



(h) Pay cond: Managers



(i) Pay cond: Non-Man

Notes: This figure reports the event study of the effect of suspension on worker-level outcomes. We define the layer of the employees based on their position before the audit in particular: All workers (All), Managers, and Non-Manager (Non-Man). *Working* is a dummy that takes the value if the individual is working. *Pay* is the logarithm of one plus the wage, while *Pay cond* is the logarithm of the wage.

Table I: Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Full RAIS			Audited			Audited:Passive			Audited:Active			Audited:Corrupt			Suspended		
	Mean	Median	Standard deviation	Mean	Median	Standard deviation	Mean	Median	Standard deviation	Mean	Median	Standard deviation	Mean	Median	Standard deviation	Mean	Median	Standard deviation
Employees	15.66	3	351.15	46.52	12	112.62	54.43	12	129.24	37.00	11	93.32	58.03	15	93.60	128.23	13	363.83
Managers	0.71	0	20.04	2.47	0	6.67	2.97	0	7.78	1.91	0	5.35	2.85	1	5.31	3.07	1	7.02
Non-Manager	14.33	3	308.60	43.27	11	103.47	50.34	11	117.78	34.61	10	86.82	55.00	14	88.95	124.96	12	359.58
Wage	486.17	381	2569.26	524.97	438	310.39	551.85	449	343.17	497.34	426	277.51	519.38	452	228.16	534.03	454	294.68
Manager's wage	1150.38	752	4279.22	1282.43	907	1159.86	1393.69	971	1273.58	1180.27	849	1053.80	1084.11	873	699.85	1012.12	762	685.80
Non-Manager's wage	460.84	373	2415.63	496.80	425	260.80	518.26	435	285.04	474.22	416	236.51	497.25	437	203.54	502.05	440	250.84
Any SPPP contract	0.02	0	0.13	0.37	0	0.48	0.37	0	0.48	0.37	0	0.48	0.36	0	0.48	0.14	0	0.35
Number of SPPP contracts	2.71	1	8.98	5.78	2	18.12	7.41	2	23.61	4.06	1	9.84	5.69	2	13.05	7.37	3	13.02
Any federal contracts	0.00	0	0.05	0.05	0	0.21	0.06	0	0.23	0.04	0	0.20	0.04	0	0.20	0.33	0	0.47
Number of federal contracts	2.30	1	4.59	3.33	2	4.55	3.61	2	4.54	3.14	1	4.74	1.54	1	1.36	2.87	2	3.65
Amount of federal procurement (USD)	317.38	17	3677.10	1296.98	70	9639.44	1664.38	76	12585.80	870.18	56	3381.14	503.06	178	740.21	1001.45	75	5174.20
Any public loan	0.03	0	0.16	0.17	0	0.37	0.16	0	0.36	0.17	0	0.38	0.22	0	0.42	0.16	0	0.37
Number of public loans	3.41	2	8.29	4.07	2	6.76	4.40	2	7.85	3.75	2	5.59	4.18	2	6.26	4.81	2	7.61
Loan amount (USD)	155.99	10	6927.54	491.10	17	4975.01	959.08	18	7290.86	94.20	14	783.47	123.57	45	218.96	89.34	14	330.22

Notes: This table presents the summary statistics of firms in our audit and suspension regression sample. The firms in the audit regression sample are audited firms and their matched control. The firms in the suspension regression sample are suspended firms and their matched control. Section IV details the matching method.

Table II: Plant Distribution by Sector and Size

	(1) Full RAIS	(2) Audited	(3)	(4) Audited: Passive	(5)	(6) Audited: Active	(7)	(8) Audited: Corrupt	(9)	(10) Suspended	(11)
	Percentage	Number	Percentage	Number	Percentage	Number	Percentage	Number	Percentage	Number	Percentage
Panel A: All sample											
Sector distribution:											
Retail	40.40	5563	58.84	2824	56.39	2532	67.06	207	30.90	1277	33.69
Services	29.58	1034	10.94	595	11.88	354	9.38	85	12.69	1156	30.50
Construction	7.10	1773	18.75	1001	19.99	475	12.58	297	44.33	724	19.10
Other	22.92	1084	11.47	588	11.74	415	10.99	81	12.09	633	16.70
Size distribution:											
Micro1	59.98	3442	36.41	1798	35.90	1415	37.47	229	34.18	3443	36.37
Micro2	18.45	1881	19.90	991	19.79	768	20.34	122	18.21	1882	19.88
Small	17.62	2816	29.79	1459	29.13	1160	30.72	197	29.40	2816	29.75
Medium	2.02	576	6.09	310	6.19	201	5.32	65	9.70	576	6.08
Large	1.93	739	7.82	450	8.99	232	6.14	57	8.51	749	7.91
Panel B: Matched sample											
Sector distribution:											
Retail		1033	79.10	498	78.30	496	81.98	39	60.00	76	46.06
Services		75	5.74	49	7.70	26	4.30			44	26.67
Construction		91	6.97	36	5.66	36	5.95	19	29.23	33	20.00
Other		107	8.19	53	8.33	47	7.77	7	10.77	12	7.27
Size distribution:											
Micro1		379	29.02	183	28.77	176	29.09	20	30.77	49	29.70
Micro2		213	16.31	102	16.04	104	17.19	7	10.77	33	20.00
Small		441	33.77	199	31.29	225	37.19	17	26.15	40	24.24
Medium		121	9.26	62	9.75	51	8.43	8	12.31	13	7.88
Large		152	11.64	90	14.15	49	8.10	13	20.00	30	18.18

Notes: This table presents the distribution of plants in the Brazilian economy, audited plants, and suspended plants, by sector and size categories. Panel A presents the distribution for all the plants, while Panel B presents the distribution for the matched sample. *Other* sectors include manufacturing, agriculture, mining, and utilities. The size categories are *Micro1*: 1-4 employees; *Micro2*: 5-9 employees; *Small*: 10-49 employees; *Medium*: 50-99 employees; *Large*: more than 100 employees.

Table III: Audits, Suspensions, and Employment

	(1)	(2)	(3)
	Audited		Suspended
Post * 1(Exposed)	0.0481***	0.0713***	-0.897***
	(0.0174)	(0.0238)	(0.107)
Post * 1(Exposed) * Active		-0.0220	
		(0.0337)	
Post * 1(Exposed) * Corrupt		-0.269**	
		(0.112)	
Post * Active		0.0151	
		(0.0224)	
Post * Corrupt		0.0537	
		(0.0655)	
Post	-0.0228*	-0.0322**	0.383***
	(0.0121)	(0.0159)	(0.0827)
Observations	16,986	16,986	1,694
R^2	0.952	0.952	0.855
Mean Dependent Variable	2.61	2.61	3.27
Plant Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES

Notes: This table presents the estimation of the audit and suspension on the logarithm plus one for total employment. In columns (1) and (2) we present results for the audited sample which consists of audited firms and their matched control. In column (3) we present results for the suspended sample which consists of suspended firms and their matched control. Section IV details the matching method. Standard errors clustered at firm level reported in brackets. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table IV: Audits, Suspensions, and Exit

	(1)	(2)	(3)
	Audited		Suspended
Post * 1(Exposed)	-0.0032	-0.0078	0.125***
	(0.0044)	(0.0061)	(0.0198)
Post * 1(Exposed) * Active		0.0077	
		(0.0088)	
Post * 1(Exposed) * Corrupt		0.0196	
		(0.0226)	
Post * Active		-0.0042	
		(0.0064)	
Post * Corrupt		-0.0030	
		(0.0157)	
Post	-0.0101**	-0.0080	-0.0595***
	(0.0042)	(0.0053)	(0.0166)
Observations	17,960	17,960	2,090
R^2	0.189	0.189	0.256
Mean Dependent Variable	0.01	0.01	0.01
Plant Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES

Notes: This table presents the estimation of the heterogeneous effect of audit and suspension on a dummy for exiting. In columns (1) and (2) we present results for the audited sample which consists of audited firms and their matched control. In column (3) we present results for the suspended sample which consists of suspended firms and their matched control. Section IV details the matching method. Standard errors clustered at firm level reported in brackets. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table V: Access to Public Procurement

	(1)	(2)	(3)	(4)	(5)	(6)
	Any Contract					
	Audited Plants				Suspended Plants	
	SPP	FPP	SPP	FPP	SPP	FPP
Post * 1(Exposed)	-0.0244**	-0.0199***	-0.0206	-0.0126	-0.0769**	-0.447***
	(0.0108)	(0.0062)	(0.0140)	(0.00864)	(0.0343)	(0.0523)
Post * 1(Exposed) * Active			-0.0075	-0.0099		
			(0.0196)	(0.0122)		
Post * 1(Exposed) * Corrupt			-0.0035	-0.0597*		
			(0.0479)	(0.0330)		
Post * Active			0.0155	-0.00167		
			(0.0186)	(0.00589)		
Post * Corrupt			-0.0057	0.0187		
			(0.0437)	(0.0172)		
Post	0.132***	0.0145***	0.125***	0.0146**	0.0348	0.406***
	(0.0106)	(0.0054)	(0.0134)	(0.0065)	(0.0293)	(0.0471)
Observations	13,552	15,284	13,552	15,284	1,286	1,326
R^2	0.822	0.612	0.822	0.612	0.791	0.569
Mean Dep. Variable	0.46	0.05	0.46	0.05	0.21	0.37
Plant Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: This table presents the estimation of the effect on public procurement contracts. Columns (1) to (4) present the effect of audit, while columns (5) and (6) show the effect of the suspension. The audit sample consists of corrupt firms and their matched control firms. The suspension sample consists of suspended firms and their matched control firms. Section IV details the matching method. In Columns (1), (3), and (5) the outcome is the indicator for having at least one contract in the Sao Paulo public procurement, while columns (2), (4), and (6) the dependent variable is an indicator for having at least one contract in the federal public procurement. Standard errors clustered at firm level reported in brackets. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table VI: Government Funding

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Audited Plants						Suspended Plants		
	# of Loans	Ln(Amount+1)	Interest Rate	# of Loans	Ln(Amount+1)	Interest Rate	# of Loans	Ln(Amount+1)	Interest Rate
Post * 1(Exposed)	0.237**	0.105	0.186	0.315**	0.068	0.356	-0.991**	-1.552***	0.152
	(0.100)	(0.120)	(0.271)	(0.149)	(0.169)	(0.396)	(0.388)	(0.386)	(0.554)
Post * 1(Exposed) * Active				-0.134	0.160	-0.696			
				(0.200)	(0.234)	(0.517)			
Post * 1(Exposed) * Corrupt				-0.346	-0.762	2.588**			
				(0.564)	(0.671)	(1.220)			
Post * Active				-0.083	-0.226	0.111			
				(0.119)	(0.147)	(0.337)			
Post * Corrupt				0.296	0.375	-1.200			
				(0.247)	(0.435)	(0.978)			
Post	-0.186**	-0.130	-0.212	-0.159	-0.038	-0.177	0.303	0.971***	0.101
	(0.083)	(0.106)	(0.217)	(0.097)	(0.129)	(0.278)	(0.263)	(0.345)	(0.482)
Observations	16,510	16,510	2,435	16,510	16,510	2,435	1,580	1,580	229
R^2	0.451	0.514	0.753	0.451	0.515	0.755	0.480	0.519	0.831
Mean Dep. Variable	0.401	1.296	9.264	0.401	1.296	9.264	0.760	1.547	10.2
Plant Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: This table presents the estimation of the effect on the access to government funding. Columns (1) to (6) present the effect of audit, while columns (7) to (9) show the effect of suspension. The audit sample consists of audited firms and their matched control. The suspension sample consists of suspended firms and their matched control. Section IV details the matching method. The outcomes are the number of outstanding loans (columns 1, 4, and 7), the logarithm of the total amount plus one (columns 2, 5, and 8), and the average interest rate (columns 3, 6, and 9). Standard errors clustered at firm level reported in brackets. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table VII: Skill Upgrading

	(1)	(2)	(3)	(4)	(5)	(6)
	Audited Plants				Suspended Plants	
	Education	High-Skill Share	Education	High-Skill Share	Education	High-Skill Share
Post * 1(Exposed)	0.0176	0.0011	-0.0363	-0.0130	0.1073	0.0150
	(0.0333)	(0.0052)	(0.0441)	(0.0087)	(0.1105)	(0.0193)
Post * 1(Exposed) * Active			0.1138*	0.0271**		
			(0.0656)	(0.0127)		
Post * 1(Exposed) * Corrupt			0.0197	0.0064		
			(0.1680)	(0.0298)		
Post * Active			-0.0246	-0.0109		
			(0.0440)	(0.0091)		
Post * Corrupt			-0.0570	-0.0029		
			(0.1078)	(0.0232)		
Post	-0.0294	-0.0001	-0.0158	0.0063	-0.1875**	-0.0121
	(0.0263)	(0.0064)	(0.0318)	(0.0065)	(0.0916)	(0.0159)
Observations	17,130	17,130	17,130	17,130	1,876	1,876
R^2	0.842	0.790	0.842	0.821	0.814	0.822
Mean Dep. Variable	11.12	0.66	11.12	0.66	10.93	0.65
Plant Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: This table presents the estimation of the effect on the level of workforce skills. Columns (1) to (4) shows the effect of audit, while columns (5) and (6) show the effect of suspension. The audit sample consists of audited firms and their matched control. The suspension sample consists of suspended firms and their matched control. Section IV details the matching method. The dependent variables are: Education which is the average years of education among the employees, and High-Skill Share which defined as the share of workers that completed a high-school. Standard errors clustered at firm level reported in brackets. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table VIII: Worker-level Evidence

	(1)	(2)	(3)	(4)	(5)	(6)
	Audited Plants			Suspended Plants		
	Working	Pay Unconditional	Pay Conditional	Working	Pay Unconditional	Pay Conditional
Panel A: All Workers						
Post * 1(Exposed)	0.0027	0.0291	0.0102	-0.0741**	-0.450**	0.0047
	(0.0078)	(0.0502)	(0.0115)	(0.0369)	(0.222)	(0.0192)
Post	0.0076	0.114**	0.0692***	0.0327	0.234	0.0284***
	(0.0081)	(0.0476)	(0.0177)	(0.0266)	(0.161)	(0.0102)
Observations	879,565	879,565	780,049	277,614	277,614	231,795
R^2	0.173	0.270	0.629	0.225	0.254	0.550
Mean dep variable	0.89	5.85	6.56	0.85	5.12	6.03
Panel B: Managers						
Post * 1(Exposed)	-0.0069	-0.0187	0.0138	-0.102***	-0.710***	-0.0684*
	(0.0113)	(0.0852)	(0.0212)	(0.0376)	(0.255)	(0.0383)
Post	0.0353***	0.272***	0.0062	0.0810**	0.555**	0.0412
	(0.0078)	(0.0684)	(0.0245)	(0.0362)	(0.234)	(0.0280)
Observations	52,479	52,479	48,091	7,130	7,130	6,132
R^2	0.218	0.316	0.717	0.315	0.343	0.739
Mean dep variable	0.95	7.09	7.45	0.91	5.93	6.50
Panel C: Non-Managers						
Post * 1(Exposed)	0.0035	0.0310	0.0078	-0.0737*	-0.445*	0.0063
	(0.0079)	(0.0510)	(0.0103)	(0.0379)	(0.227)	(0.0196)
Post	0.0068	0.121***	0.0818***	0.0331	0.236	0.0278***
	(0.0078)	(0.0392)	(0.0281)	(0.0274)	(0.165)	(0.0102)
Observations	820,843	820,843	725,834	269,764	269,764	224,961
R^2	0.177	0.271	0.652	0.226	0.255	0.557
Mean Dep. Variable	0.89	5.77	6.49	0.85	5.10	6.02
Plant Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES

Notes: This table presents the estimation of the effect of both audit and suspension on the worker-level outcomes. Columns (1) to (3) show the effect of audit while columns (4) to (6) show the effect of suspension. The audited sample consists of workers from audited firms and their matched control. The suspended sample consists of workers from suspended firms and their matched control. Section IV details the matching method. Working is a dummy that takes the value if the individual is working in that year. Pay unconditional is the logarithm of one plus the wage, while pay conditional is the logarithm of the wage. Standard errors clustered at firm level reported in brackets. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

ONLINE APPENDIX

A.1 Coding of CGU Audit Reports

A.1 Digitization process

We extract information from the audit reports thanks to the support of a team of research assistants. The coding of the information is performed starting with the download and careful analysis of a random sample of 100 audit reports by the team of coauthors. Based on this extensive reading, we develop a detailed instruction manual, highlighting the specific pieces of information we want to extract.

Four research assistant and a research supervisor are each assigned a set of audit reports to analyze. All researchers are native speakers, have at least a university degree, they are paid a competitive hourly wage, and they obtain a performance-based bonus based on speed and quality. The job is tracked online on a leading freelancing platform. Each team member is also assigned 30 audit reports that are also assigned to other team members. This provides a double check of 150 audit reports. Incompatible entries are checked and corrected by the research supervisor.

The researchers first code the data for the given audit report, namely round, municipality, state, date the audit took place, and date the audit was publicly disclosed. Then, the process consists of first looking for the word "cnpj" in the pdf file. The CNPJ is the tax identifier of firms in Brazil. For each occurrence, the researcher investigates the full evidence and discussion of the given irregularity, and extract the following information:

- Tax identifier and firm name
- Amount associated to the irregularity (i.e. value of public procurement contract)
- Description of irregularity, including excerpts from the text
- Involvement of the firm, i.e. cases of active or passive involvement, and uncertain cases, as illustrated in A.2
- The date the contract awarding and completion, when available (these are often months or years earlier than the date the audit takes place)
- Open-ended comments about additional information and issues

The same process is then repeated for the cases of firms who appear by name only and not by CNPJ. These are potentially informal firms. The only difference with this process is that the researchers originally look for one of the following keywords (and small spelling variations of them): empresa, companhia, firma, negocio, corporacao, entidade, estabelecimento, sociedade, parceria, empregador. Cases that already appeared in the previous CNPJ search are discarded from this round. The data extraction then remains the same. For all these "informal" firms, we then try to obtain the CNPJ from publicly available sources online. Data on firms with no CNPJ is not used in this paper, as we rely on the matching of the audits data with the RAIS database on formal firms.

On average, the coding of one report takes between 60 and 90 minutes. During the above process, we would conduct regular random checks of the data collected, and one-on-one weekly individual discussions with the team members.

At the end of the process, we conduct a thorough cross-validation of the information collected. This phase is carried out by the research supervisor and two new research assistants who are asked to go over a large random sample of the data.

A.2 Examples of Irregularities in CGU Audit Reports

We report here a list of 40 representative irregularity cases extracted from the audit reports. We split the types of cases so as to be representative of the distribution in the full sample. We omit tax identifiers and edit the precise extracts from the audit reports to improve readability.

A.3 Corrupt

1. In the municipality of Alto Alegre do Pindare, auditors uncovered the misappropriation of public funds related to a tender for the purchase of food products needed to prepare school meals. The winning company, which was paid for this contract, presented fake "fiscal good standing" certificates, in order to access the bidding. In addition, the auditors found out that the products purchased from the company were never delivered to the schools, and that the invoices issued by the company were invalid, due to the lack of printed authorization from the Treasury Authority.

2. The company Construtora Mavil Ltda was hired by the municipality of Quixaba to provide drilling services and to install 11 deep tubular wells with pipeline and storage systems, for an amount of R$ 203,245.13. The auditors found that two years and four months after the beginning, the work was far from being concluded, and the company had abandoned the project without providing explanations or contacting the municipality.

3. The municipality of Japoata awarded a construction procurement to a company for an amount of R$ 415,248.44. The company was supposed to provide two water supply systems. The physical inspection by the auditors uncovered that the work had stopped and and the two water supply systems were never activated.

4. The municipality of Maribondo opened a tender to provide school transportation services. The tender was awarded to Ideal - Locacoes e Servicos Ltda. Upon inspection, it was discovered that the vehicles used to transport the students did not match the models specified in the contract (type and vintage). The vehicles were visibly old and lacked safety measures, and the drivers were not qualified to drive the vehicles. In addition, the company fully subcontracted the services to a third party company which was never mentioned in the original bidding documents.

A.4 Actively Involved

1. Against provision 8,666/93, the municipality of Sao Desiderio did not hold an open bidding tender for the purchase of food products. Instead, the municipality hired directly the company Distribuidora Lord Ltda and other two suppliers to provide the products.

2. The company Mary e Nidia Comercio Ltda. provided food products to the Municipality of Saloa, but the municipality did not conduct a price survey that would guarantee the acquisition of materials at prices advantageous to the local government. The company's fiscal situation was also not in full compliance, which should have prevented its participation in public procurement.

3. Upon audit, it was found that three bidders may have colluded during the bidding process of a tender pertaining the provision of medicines. G. Odisio Com. Rep. Ltda was the winning company, while the other two bidders presented bids with prices that were only 1% and 1,5% lower.

4. The auditors uncovered several irregularities for a tender held by the municipality of Pequi, where multiple companies did not submit the required documents to participate in the tender. Three of the participating companies also had shared family ties. Additionally, the municipality did not publish the results of the tender.

5. The company Ometac Dental won a contract requiring to provide medicines and other medical items to the municipality of Amarante do Piaui. Ometac Dental however produced additional invoices, as part of the same contract, which included items that were not originally part of the tender.

6. The Municipality of Angical do Piaui published a tender for the purchase of schools' material. Although the winning company was Dinamica Comerical Distribuidora, G. DE S. Coelho - MEE the municipality paid the company Babylandia Variedades, Neida Marques Fernandes for the provision of such material. The latter company did not participate in the original auction.

7. A firm won a competitive auction for the provision of building material to the municipality of Placido de Castro. The audit uncovered company invoices that were larger than the value established in the contract documents.

8. The company provided several equipment pieces needed to set up a chicken production unit the municipality of Graccho Cardoso. While the company's original bid was for one total large amount, the municipality asked for four invoices, each smaller than R$8,000, so as to bypass specific public procurement requirements.

9. The auditors uncovered over-invoicing irregularities in the municipality of Ronda Alta, in relation to a contract for the repair and modernization of infrastructure for recreational and leisure uses, provided by a specific company. Contracted prices were higher than those foreseen in the National System of Prices and Indices for Civil Construction (SINAPI). The SINAPI system must be observed in the execution of tenders contracted by means of federal transfer to the municipalities, as stated in the art. 115 of Law 11.439 / 2006, Budgetary Guidelines Law.

10. The company Jose Ozana Goncalves, which won the bidding for a specific procurement contract regarding the purchase of educational material, presented expired "fiscal good standing" certificates. Moreover, the auditors found that the company contravened to art. 43, item IV of Law No. 8.666 / 93, as it did not cross-validate its prices to external price surveys.

11. The municipality of Urussanga opened a tender to rent tents, stages, power generators, and other large infrastructure to host a high-profile artistic show. Three companies presented their bids. After the bids were submitted, the municipality canceled the original tender, and hired a company through direct procurement for the same exact contract, through legal exemptions that the auditors found suspicious. Other small irregularities were uncovered during the audit of that tender.

12. The municipal government of Bagre opened a tender with the purpose of purchasing medicines and hospital materials. The company Medcfarma Ltda - ME won the procurement and provided the goods as per contract. However, the auditors uncovered overpricing, by comparing the price the municipality paid with the average prices recorded in the Ministry of Health's Price Bank.

13. The municipality of Jundia diverted funds coming from the FUNDEB, through various invoices paid to the company Auto Posto Novo Lino Ltda. The invoices did not specify product characteristics that would allow to validate their relevance for the FUNDEB federal transfer program.

14. The auditors discovered that a tender in the municipality of Alianca, with the purpose of hiring specialized companies to provide school transport services, lease of vehicles, machinery and other equipment, presented signs of lack of competition. The municipality added specific requirements to the tender, so that the only company who could fit the criteria was Personalite Locacoes Ltda. Other four bidding companies were excluded from consideration because they could not respect the restrictive requirements imposed by the municipality.

15. In a tender for the renovation of a school in the municipality of Laranjeiras do Sul, there was significant evidence of both over-invoicing and manipulation of the bidding documents to force a specific winning company.

16. A case of over-invoicing was uncovered in the municipality of Altos, which hired the company Construtora Ribeiro Veloso to provide renovation-related services of 21 public schools. While the company was paid R$ 327,344.48, the value of renovations provided amounted to a total value of just R$ 83,081.79.

17. The municipality of Salitre did not monitor the construction of a sports' court, which was a contract awarded to the company Construtora Astron Ltda. After auditing the relevant documentation it was discovered that the lack of monitoring led to the acceptance of services which presented some defects and inaccuracies.

18. An audit found that invoices related to a specific federal transfer were not marked with reference to the specific title and number of the Funds Transfer Instrument, therefore disregarding the legal requirement. Several companies have issued suspicious invoices pertaining to that transfer.

A.5 Passively Involved

1. In the municipality of Itabaianinha, the invoices presented as proof of the expenses for the Educational Program PDDE did not match the name of the program paying for the expenses. N.P.R. Papelaria Moveis p/ Escritorio e Assistencia Tecnica Ltda was the company which provided the material and issued the invoices.

2. The company Comercial Marinho supposedly participated and won a bidding invitation for a value of R$ 22,000.00 to provide schoolbags to students in the municipality of Alto Alegre do Pindar. The audit contacted the company owner, who denied participation in the procurement, and who did not receive any payments. The receipts presented by the municipality were false, according to the company representative.

3. The municipality of Carinhanha used two false invoices of the company Imunosystems Comercial Ltda to prove the acquisition of medicine for the Health Unit Centers. To counter the municipality's allegation, the company owner declared to the CGU that its company had never done business with the municipality, and presented all invoices ever issued by the company as proof.

4. The auditor verified that invoices in the total amount of R$ 20.599,50 regarding the acquisition of medicine from the company in question were false. The company owner declared to the auditors that his company has never provided medicine to the municipality.

5. The bidding process for various repair services in the "Altino Arantes" park exhibition took place through an open invitation promoted by the municipality of Igarapava. Proposals were submitted by three companies: Construtora Batista e Martins Ltda., Construtora Souto Andrade Ltda. and Laterza Construtora Ltda. The first one was declared winner of the event, with a proposal of R$ 150,000.00, but the auditors found the owner to be the same as the second company. The third company, Laterza Construtor, which we label Passively Involved, did not win the contract.

6. In the municipality of Catolandia, the auditors verified that in a tender regarding the acquisition of fuel, the companies Posto Dourado and Centro Automotivo Pneus Dourado appeared to participate in the tender, but the evidence showed this never happened. Instead, there seems to be evidence that the municipality faked the information to simulate the existence of the tender for the use of pubic funds.

7. The municipality of Jatoba held a procurement for the repair of five public schools. The audit uncovered the simulation of a tender which in practice did not take place. The company Construtora Esmeralda Ltda twas listed as a participant to the auction, but information about the company (e.g., address) were fake.

8. The company DIVEPEL â Distribuidora de Veiculos e Pecas Ltda participated in the tender held by the municipality of Jatoba for the acquisition of vehicles. The auditors did not find the documentation provided by the company to be in full compliance with what was required in the public announcement of the tender. The company nonetheless did not win the procurement contract.

9. The municipality of Coroata had supposedly hired the cleaning service company P. S. Sousa e Cia Ltda. The auditors found all documentation of the contract in place, but some of the invoices attached in the documentation as proof of expenses were seemingly issued by another company, namely Remax Distribuidora Ltda. When contacted by CGU, the owners of Remax Distribuidora Ltda claimed that to have never issued invoices, and to have never taken part of any procurement with the municipality. The amount supposedly paid to the company was R$ 626,199.40.

10. Instead of following the prescribed procedure, which requires the municipality to pay contractors using company-specific bank accounts, the municipality of Teotonio Vilela paid several different companies using personal checks. The procedure adopted is not adequate, as federal funds should be kept in separate accounts (one for each federal program), and specific payment methods are required for trace-ability.[14]

11. The company NEL Projetos Ltda., which we label as Passively Involved, participated in a bidding invitation held by the municipality of Campos Belos, for a contract to build a public market space, but it did not win the contract. The contract was won by the company VM Vieira e Mendonca Construcoes e Servicos Ltda, which delayed the execution of the works without justification. The municipality had not taken any action to address this issue.

12. The company Siqueira Comercio e Servicos de Encadernacao Ltda was legally hired to provide market price survey services for a bidding invitation regarding the acquisition of goods by the municipality of Acopiara. The auditors found out that the latter procurement (in which different companies were involved) was tainted by irregularities, namely the lack of documentation authenticated by appropriate notarial registries.

13. In lack of compliance of Interministerial Ordinance MF / MPAS 5.402 / 1999, the municipality of Pitimbu did not retain 11% of the total value of a payment made to the company JI Construcoes Civis Ltda, which was due to social security for tax purposes.

14. The municipality of Itatira misappropriated the resources coming from the Ministry of Education's Fund for Maintenance and Development of Basic Education (Fundeb), aimed at financing basic public education. The local government used the funds to pay for expenses not included in the program: the financial resources were used to pay for expenditure of vehicles for the transport of teachers. These services were provided by the company A&M Construcoes e Serv. Ltda.

15. Contravening to art. 22, paragraph 6, of Law no. 8,666 / 2003, the municipality of Passagem repeatedly invited the same set of companies to bid in two tenders involving the purchase of medicines. The law requires that, in these cases, the municipality should invite at least one new company. The company Farmacia Frei Damiao - Ana MariaTorres Leite ME participated as a bidder in both tenders, and did not win any contract.

[14]The companies paid through these methods were classified as passively involved.

16. In the municipality of Sao Gabriel, the auditors uncovered multiple irregularities related to the use of federal funds. There was evidence of multiple cases in which signatures, invoices, and documents of real companies were falsified to simulate real contracts and use of funds. For example, the company Magazine Aquarela supposedly presented a bid to provide school furniture. However, the company owner informed the auditors that the company had never done any business with the municipality, and that his signatures were forged by local public officials.

17. The municipality of Luziani diverted resources belonging to the Ministry of Health's Basic Attention in Health Program away from its original purpose. The local government paid the company Sport Car Pecas e Servicos Ltda for several contracts regarding vehicle repairs, which were not the intended use by the Ministry of Health.

18. Upon inspection of invoices provided by the company Mercearia J.L., the auditors reported the lack of documentation regarding an itemized list of the specific products provided, which the municipality should have kept according to the provisions of art. 18 of Resolution CD / FNDE No. 6.

A.2 Additional Figures and Tables

Table A1: Audits, local procurement, and municipality of audit

	(1)	(2)	(3)	(4)
	Any contract			
	From audited municipality	From other municipality	From audited municipality	From other municipality
Post * 1(Exposed)	-0.0135***	-0.0274***	-0.0127***	-0.0213
	(0.00314)	(0.0105)	(0.00426)	(0.0138)
Post * 1(Exposed) * Active			-0.00149	-0.0115
			(0.00637)	(0.0193)
Post * 1(Exposed) * Corrupt			-0.00176	-0.0123
			(0.0148)	(0.0472)
Post * Active			0.000933	0.0220
			(0.00175)	(0.0180)
Post * Corrupt			0.000402	0.00121
			(0.000453)	(0.0435)
Post	0.00390**	0.124***	0.00343**	0.114***
	(0.00182)	(0.0103)	(0.00167)	(0.0130)
Observations	13552	13552	13552	13552
R^2	0.867	0.830	0.867	0.830
Mean dep variable	0.03	0.46	0.03	0.46
Firm Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes

Notes: This table presents the estimation of the effect of audit other local procurement outcomes. The dependent variables are a dummy for any Sao Paulo public procurement contract with the audited municipality where the firm was exposed (columns 1 and 3) and a dummy for any contract from other municipalities (column 2). The sample consists of audited firms and their matched control who had a contract with Sao Paulo public procurement in the year of audit and shares similar characteristics on years t-3 to t-1. Standard errors clustered at firm level reported in brackets. Significance level: *** p<0.01, ** p<0.05, * p<0.1.