Journal of Financial Economics 143 (2022) 1097–1119

Contents lists available at ScienceDirect

Journal of Financial Economics

journal homepage: www.elsevier.com/locate/jfec

Revealing corruption: Firm and worker level evidence from Brazil[☆]

Emanuele Colonnelli[a,*], Spyridon Lagaras[b], Jacopo Ponticelli[c], Mounu Prem[d], Margarita Tsoutsoura[e]

[a] University of Chicago, Booth School of Business, 5807 S. Woodlawn Ave, Chicago, IL, 60637, USA. CEPR, 33 Great Sutton Street, London, EC1V 0DX, UK
[b] Katz Graduate School of Business, University of Pittsburgh, 3950 Roberto & Vera Clemente Drive, Pittsburgh, PA, 15260, USA.
[c] Northwestern Kellogg, 2211 Campus Dr, Evanston, IL, 60208, USA. NBER, 1050 Massachusetts Ave, Cambridge, MA, 02138, USA. CEPR, 33 Great Sutton Street, London, EC1V 0DX, UK
[d] Department of Economics, Universidad del Rosario, 6-25 Calle 12C, Bogotá, DC, Colombia
[e] Cornell University, SC Johnson College of Business, 106 E Ave, Ithaca, NY, 14853, USA. NBER, 1050 Massachusetts Ave, Cambridge, MA, 02138, USA. CEPR, 33 Great Sutton Street, London, EC1V 0DX, UK

ARTICLE INFO

Article history:
Received 4 August 2021
Revised 29 December 2021
Accepted 29 December 2021
Available online 13 January 2022

JEL classification:
D21
D22
D73
E22
L10
L22

Keywords:
Firms
Corruption
Brazil
Investment
Government contracts

ABSTRACT

We study how the disclosure of corrupt practices affects the growth of firms involved in illegal interactions with the government using randomized audits of public procurement in Brazil. On average, firms exposed by the anti-corruption program grow larger after the audits, despite experiencing a decrease in procurement contracts. We manually collect new data on the details of thousands of corruption cases, through which we uncover a large heterogeneity in our firm-level effects depending on the degree of involvement in corruption. Using investment-, loan-, and worker- level data, we show that the average exposed firms adapt to the loss of government contracts by changing their investment strategy. They increase capital investment and borrow more to finance such investment, while there is no change in their internal organization. We provide qualitative support to our results by conducting new face-to-face surveys with business owners of government-dependent firms.

1. Introduction

Corrupt practices in the assignment of government contracts are pervasive around the world. These practices are particularly widespread in emerging markets where they are considered a major 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

☆ Toni Whited was the editor for this article. 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.

* Corresponding author.
E-mail address: emanuele.colonnelli@chicagobooth.edu (E. Colonnelli).

https://doi.org/10.1016/j.jfineco.2021.12.013
0304-405X/

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, and several open questions remain about how they impact the business practices and performance of exposed firms and their employees.

In this paper, we rely on micro-data from Brazil and a unique institutional setting to study the real effects of a large anti-corruption program on exposed firms—i.e., on firms revealed by the program to be involved in illegal interactions with the government. Our empirical design relies on a government initiative which randomly audits municipal budgets with the aim of uncovering any misuse of federal funds. Previous literature has documented how this program affected a large set of municipality-level outcomes, including the probability of reelection of local politicians (Ferraz and Finan, 2008) and the performance of the local economy (Colonnelli and Prem, 2021). We exploit a key feature of the program that allows us to directly study its real effects on exposed firms. While the program targets the budget of municipalities, the audits expose the identity of specific firms involved in irregular business with the government. The vast majority of such firms are located outside the boundaries of the audited municipalities. Thus, by focusing on these firms, we can better isolate the direct effect of exposure of corrupt practices on firms from its overall impact on the local economy of the audited municipality. In addition, the random nature of the audits provides us with a unique setting in which the timing of firm-level exposure is plausibly exogenous.

A primary contribution of our paper is the construction of a novel dataset on corruption and firms. We build a dataset covering all firms that are exposed by the random auditing program of the CGU, the federal agency in charge of fighting corrupt practices in Brazil. Our main data source are the audit reports produced by the federal auditors that review municipal budgets. The reports, which are published online and made available to the public and the popular press, disclose the names of the companies involved in any misuse of federal funds. From the 1881 audit reports produced by the CGU between 2003 and 2014, we manually collect information on all the irregularities reported, including: the tax identifier of the firms involved, the nature of the irregularity, the type of involvement of the firm, and the value of the contracts. We match the firm-level dataset with social security data from the Ministry of Labor (RAIS) containing detailed information on all formal workers employed in Brazil, as well as with data on firms' access to public procurement contracts, on firms' investment, and on firms' access to credit lines from the Brazilian Development Bank (BNDES).

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

We start by documenting two key, seemingly contradictory findings. First, firms exposed by the anti-corruption program experience, on average, a 4.8% larger *increase* in size (as measured by total employment in the firm) relative to the control group in the three-year period following exposure. Second, exposed firms experience a significant *decrease* in their access to procurement contracts over the same period. These effects indicate that while negative exposure generated by the anti-corruption campaign decreases a firm's ability to rely on government contracts—consistent with substantial anecdotal evidence indicating that local governments steer away from firms publicly linked to corruption cases—it also benefits firm performance in the medium run, suggesting that firms were on average hindered by the presence of corruption they were directly involved in. At first glance, these findings appear somewhat in contrast with a large body of work on political connections and corporate misconduct showing that firms suffer after they lose their connections or after they are caught engaging in illicit activities.

In the second part of the paper, we investigate potential mechanisms behind the real effects of exposure. We start by exploiting the granularity of the data we collect on all corruption cases described in the audit reports. Specifically, we read all audit reports and manually classify exposed firms based on their degree of involvement in corruption practices. Auditors are required to report all firms involved in the corruption together with a description of the nature of the involvement. We uncover a large heterogeneity in the type of involvement by exposed firms, which helps rationalize our findings. First, we label as *passively involved* those firms seemingly put at a disadvantage by a rigged bidding process which, while exposed by the program, can rather be considered victims of the corrupt system in place. Second, there are firms that are *actively involved* in an irregularity, but for which there is little evidence that the firm actually benefited from the corruption scheme. One example are cases of over-invoicing for a specific good or service which is otherwise delivered to the municipality. Third, there are firms that were clearly benefiting from the corruption scheme, such as those that received payments but did not deliver the goods and services required by the procurement contract (or did so unsatisfactorily). We define these firms as *corrupt*. We find that while all types of exposed firms lose access to government contracts, the increase in firm size is only present for firms that we classify as victims of the corruption scheme and for firms that were actively involved in the corruption case but that did provide good quality goods or services to the local governments. On the other hand, firms who both engaged in irregular dealings with the government *and* performed poorly shrink in size considerably.[1]

[1] We corroborate this finding—that poorly performing corrupt firms suffer after an anti-corruption program limits their ability to obtain government contracts—using data from a different but related transparency initiative named CEIS (Szerman, 2020).

To better understand the underlying economic mechanisms at play, we combine multiple sources of data, motivated by the existing literature linking doing business with the government in the presence of corruption with firm-level distortions (Olken and Pande, 2012). Specifically, we argue that the revelation of corruption, by restricting a firm's access to government contracts, forces exposed firms to change their investment and business practices to be able to compete for private demand. While this is a mechanism previous literature has hinted at (Shleifer and Vishny, 1993; Fisman and Svensson, 2007; Cole and Tran, 2011), data limitations make it difficult to tease it out. For this purpose, we obtain restricted access to confidential data on firms' investment and access to credit. In particular, we use information on firm investment from a comprehensive survey of Brazilian manufacturing firms (PIA), and loan-level data from the development bank BNDES, which is a key provider of corporate loans for capital investment in Brazil (Torres and Zeidan, 2016). We find that exposed firms experience a larger increase in capital investment in the post-exposure period, as well as higher borrowing to finance such investments. Our findings relate to those 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 adapt to a negative shock to their access to government contracts by changing their 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.

We continue our analysis of mechanisms by focusing on worker-level data. This allows us to study the effects of exposure on a primary, yet largely understudied group of a firm's stakeholders, namely its employees. In particular, we use worker-level data to explore the impact of audits on incumbent workers' employment status and labor income. We find that workers who 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. This evidence is informative for two reasons. On the one hand, to the extent that corruption exposure is valued negatively on the labor market, the audits may independently influence individual outcomes in addition to the direct consequences on firms (Karpoff et al., 2008; 2014). We do not find evidence that employees suffer, further emphasizing some of the surprisingly positive effects of the audits on the average exposed firm. On the other hand, and importantly, the limited impact of audits on the workers of exposed firms helps rule out a further alternative explanation for our firm-level findings, in which audits lead firms to fire corrupt managers or other employees that were engaging in corruption for personal gain, leading to a change in their internal organization.

In a context like ours, which is representative of many contexts where private firms interact with local government officials, conclusively testing for mechanisms whereby firms change strategy when moving away from doing business with the government would require detailed data on firm decisions that are typically unavailable. We do, however, attempt to provide further, qualitative support for these channels by means of a new, face-to-face survey we conducted with the owners of 115 firms in Brazil representative of the ones in our main analysis sample. In our survey, we ask a series of questions about how operating in the presence of corruption affects firm strategy. The qualitative evidence from our survey points to corruption introducing several distortions in firm decisions, and specifically in firm investment strategy, thus corroborating our earlier findings.

Overall, our analysis uncovers new micro-level findings on the real effects of anti-corruption transparency initiatives, which are often masked in aggregate estimates. Highly corrupt firms experience a major decline in size when their corruption is exposed, seemingly driven by their inability to shift their customer base away from the government. However, the vast majority of firms mentioned in the audit reports subsequently grow after the anti-corruption crackdown. Our evidence points to an explanation according to which firms are often stuck in a business relationship with the government, in which corruption and other frictions hinder their growth through operational distortions (Fisman and Svensson, 2007; Olken and Pande, 2012).

Related Literature

The primary literature we contribute to is a growing one on the effectiveness of anti-corruption initiatives. In particular, following the seminal work on the political economy of audits by Ferraz and Finan (2008), several papers have investigated the effects of the Brazilian random audit program on municipality-level outcomes (e.g., Bologna and Ross, 2015, Zamboni and Litschig, 2018, Avis et al., 2018). Closest to our paper is Colonnelli and Prem (2021), who analyze the impact of the anti-corruption program on the local economy of audited municipalities, finding that local economic activity increases mainly through the growth of government-dependent sectors and that local politically connected firms—which are not exposed by the audit—suffer. A related set of papers explores the 2012 anti-corruption campaign in China, with most studies focusing on implementation rather than enforcement, as outlined by Goldman and Zeume (2020). For example, Griffin et al. (2016) uncover the presence of significant political targeting in the investigations, highlighting the difficulty of cleanly identifying the firm-level effects of anti-corruption enforcement. Indeed, similar to the studies on Brazil, the vast majority of studies on China focus on industry-level and aggregate effects, such as the work by Giannetti et al. (2021), who study how the performance of firms that operate in an ex-ante more corrupt environment (as measured by the share of entertainment expenditures) changes after the anti-corruption crackdown. A final set of related papers in this area focus on international initiatives. Following Zeume (2017), who studies the impact of the 2010 Bribery Act on U.K. firms' cost of doing business, recent examples include the work by Christensen et al. (2020b) and Christensen et al. (2020a), who describe the indirect consequences of Foreign Corrupt Practices Act (FCPA) enforcement on economic development and foreign investments in high-corruption areas, respectively. Relatedly, Goldman and Zeume (2020) examine the indirect effects of FCPA on unpunished firms and industries, showing how anti-bribery enforcement can result in the reallocation of economic activity and lead to a more level playing field.

A crucial difference between our paper and previous work on anti-corruption is that while previous studies focus on the *aggregate* consequences of anti-corruption and on proxies for firm-level exposure to the shock, our empirical analysis can identify the effect on *firms* that were directly involved in the corruption cases. Moreover, by focusing on exposed firms located outside of municipalities audited by the anti-corruption program, we are able to isolate the *direct* effects on firms from other *indirect* effects of anti-corruption enforcement. For example, the main results in Colonnelli and Prem (2021)—which only come from firms located inside the audited municipalities—are attributed to the impact of audits on political turnover, higher transparency, and other changes to the functioning of the local bureaucracies. In our paper, we are able to hold these indirect effects as fixed and study instead how firm-level outcomes change once firms are exposed by the anti-corruption program.[2] We provide a number of empirical tests in the paper to directly show that our effects, in fact, are not driven by the primary local economic mechanisms identified by Colonnelli and Prem (2021) or the local political effects discussed by Ferraz and Finan (2008) and Avis et al. (2018). A further important contribution with respect to the above body of work is the construction of an extremely rich micro-dataset to unpack economic channels. First, we contribute from a methodological perspective by manually collecting new data on exposed firms using government audit reports. Second, we bring in a large set of administrative data sources as well as original survey data that allow us to investigate various ways through which anti-corruption affects firms, their operations, and their employees.

By looking at firms potentially receiving preferential treatment from local politicians, we also add 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.[3] Our study adds nuance to this literature by highlighting that even firms that are directly involved in the corruption with local politicians might benefit from an anti-corruption campaign, because the benefits of shifting away from corrupt business with the government—e.g., lower operating frictions—seemingly outweigh those obtained through favoritism in the allocation of procurement contracts.

Finally, the paper broadly relates to the existing literature on the link between corruption and firm-level growth (see Bardhan, 1997, Svensson, 2005, Fisman and Svensson, 2007, and Olken and Pande, 2012 for comprehensive reviews of the literature), a nexus which remains largely unexplored due to the lack of settings where causality can be established. Thanks to our new data, we are able to shed some light on various within-firm distortions associated with corruption, which we know little about in the academic literature (Dal Bó and Rossi, 2007, Smith, 2016). Specifically, our findings emphasize the importance of corruption for various strategic choices by the firm, such as those related to funding sources and market access.

The paper is organized as follows. Section 2 describes the institutional setting and provides a detailed description of the anti-corruption initiatives we study. Section 3 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 4 presents our identification strategy and describes all the main empirical results of the paper. Section 5 concludes.

2. Institutional background: anti-corruption in Brazil

Brazil has constantly battled with corruption. The primary institution involved in monitoring corruption practices in Brazil is the Office of the Comptroller General (Controladoria Geral da União - henceforth CGU), which 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, by working directly with several other national enforcement agencies. In particular, 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.

The major anti-corruption initiative carried out by the CGU consists of randomized municipal audits. This flagship program started in May 2003 with the purpose of identifying and preventing corruption in the use of federal resources by local governments. The municipal audits focus on the allocation and use of federal funds that have been transferred to the municipality, covering all procurement contracts between the local government and firms that span the two years prior to the audit. The program began by selecting 26 municipalities per lottery (one from each state in Brazil), and later expanded to 60 municipalities per lottery. The program consisted of 39 lottery rounds of randomized audits, with replacement, over the 2003–

[2] A few studies on multinationals and publicly listed firms have indeed analyzed the direct impact of enforcement on corrupt firms (Karpoff et al., 2017b; Cheung et al., 2012; 2020). However, their focus has primarily been on the cost-benefit analysis of the value obtained from bribery vis-a-vis the legal costs of penalties in court. The unique random feature of the audits allows us to make progress on a typical challenge in this literature, namely the fact that the timing of enforcement actions is typically endogenous.

[3] 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), Goldman et al. (2013), Cingano and Pinotti (2013), Akey (2015), Fisman and Wang (2015), Akey and Lewellen (2017), Schoenherr (2019), Brogaard et al. (2019), O'Donovan et al. (2019), Colonnelli et al. (2020c), Colonnelli et al. (2020b), González and Prem (2020), and Bertrand et al. (2020).

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 5570 municipalities were eligible, and 1881 had been selected at least once.

The audit is performed by CGU auditors who travel to the municipality, manually review the municipality expenditures' documents and, in most cases, physically inspect the execution of federally-funded programs. To limit corruption in the audit process, the auditors are hired 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 that can span up to 300 pages. The report documents any irregularity associated with the use of federal resources, together with any justification presented by local government officials for these irregularities and the auditors' judgement on these justifications.

The reports are forwarded to the relevant administrative and judicial government agencies so they can 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. As discussed in Ferraz and Finan (2008), the news of revealed corruption easily reaches the public through the local radio networks and is heavily used in political campaigns. From the mayors' side, corruption commonly takes the form of fraud, usage of phantom firms, over-invoicing, and diversion of public resources. The firms involved in the irregularities are identified publicly along with the local government officials in the audit reports, as long as they are linked in any way to the irregular contract.

There are several potential consequences for firms that are exposed by the auditing program. In particular, if later found guilty, firms can be barred from participating in future tendering processes for federal and local contracts. For example, Planam, an ambulance company with mafia connections, was found to charge the local government for services not provided, and as a result was subsequently declared illicit by the courts 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. Even when not directly prosecuted, several anecdotes indicate that local governments steer away from doing business with firms involved in exposed irregularities, due to reputational and political considerations.[4] Many argue these are some of the undesirable consequences of transparency initiatives that might damage both culpable as well as innocent firms (Liu et al., 2021).

3. Data

In this section, we discuss the main data sources we use in the paper as well as the sample selection procedure to arrive at the final estimation sample. The main dataset used in the analysis combines information from the new measures we create from the CGU anti-corruption reports and administrative matched employer-employee data on the Brazilian formal sector. We also rely on data on public procurement contracts, on confidential loan-level data on government funding to firms, and on data on investment and sales for a sample of manufacturing firms.

3.1. Main data sources

3.1.1. 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 1881 different municipalities randomly selected to be audited in the period 2003–2014.

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 appears in the auditors' description of the case. This approach represents an important contribution relative to the previous literature using these data for measurement or prediction of corruption. Indeed, while Ferraz and Finan (2008), Brollo et al. (2013), and Zamboni and Litschig (2018) have used CGU audit reports to measure corruption, and Colonnelli et al. (2020a) use them in machine-learning models to predict corruption, all these studies only focus on aggregate municipal measures without identifying specific firms involved in the irregularities. Throughout the paper, we refer interchangeably to firms that are identified as being linked to an irregularity as "audited" or "exposed."

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 a contract was awarded and completed, and the extent of a firm's 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. We discuss more details of the data construction process in Section 4.3 and in Appendix A.1.

[4] See, for example, https://valor.globo.com/politica/noticia/2019/12/16/a-lava-jato-destruiu-empresas-diz-toffoli-a-jornal.ghtml and https://www.corecon-rj.org.br/anexos/C1D017FCEE732F4E1B9B4E13C46AD36E.pdf (last accessed on November 17th, 2021).

It is important to highlight a crucial caveat with respect to our new dataset, namely that the revelation of corruption depends on both the actual corruption of the firm and the fact that auditors are able to detect the given irregularity. Hence, it is possible that firms identified as corrupt in the audit reports are not fully representative of all corrupt firms in Brazil. In particular, it is plausible that firms that benefit the most from corruption—and suffer most from detection—might also be the ones that are better able to escape detection in the first place. This is a typical concern in the corruption literature, which should be kept in mind when thinking about external validity interpretations of our empirical findings.

3.1.2. Matched employer-employee data

The firm and worker level information we use as outcomes in the analysis comes 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 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 US. In the data, we also observe the tax identifiers of both the firm and the establishment of the worker, as well as the five-digit industry they operate and the municipality they are located. 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, and 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.

3.1.3. Public procurement contracts

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 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 in 2016. We rely on this dataset to match audited firms to control firms in the analysis.

A third dataset allows us to identify suspensions of firms due to prosecuted irregularities in public procurement. 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 2008–2017. 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.

3.1.4. Access to finance, sales, and investment

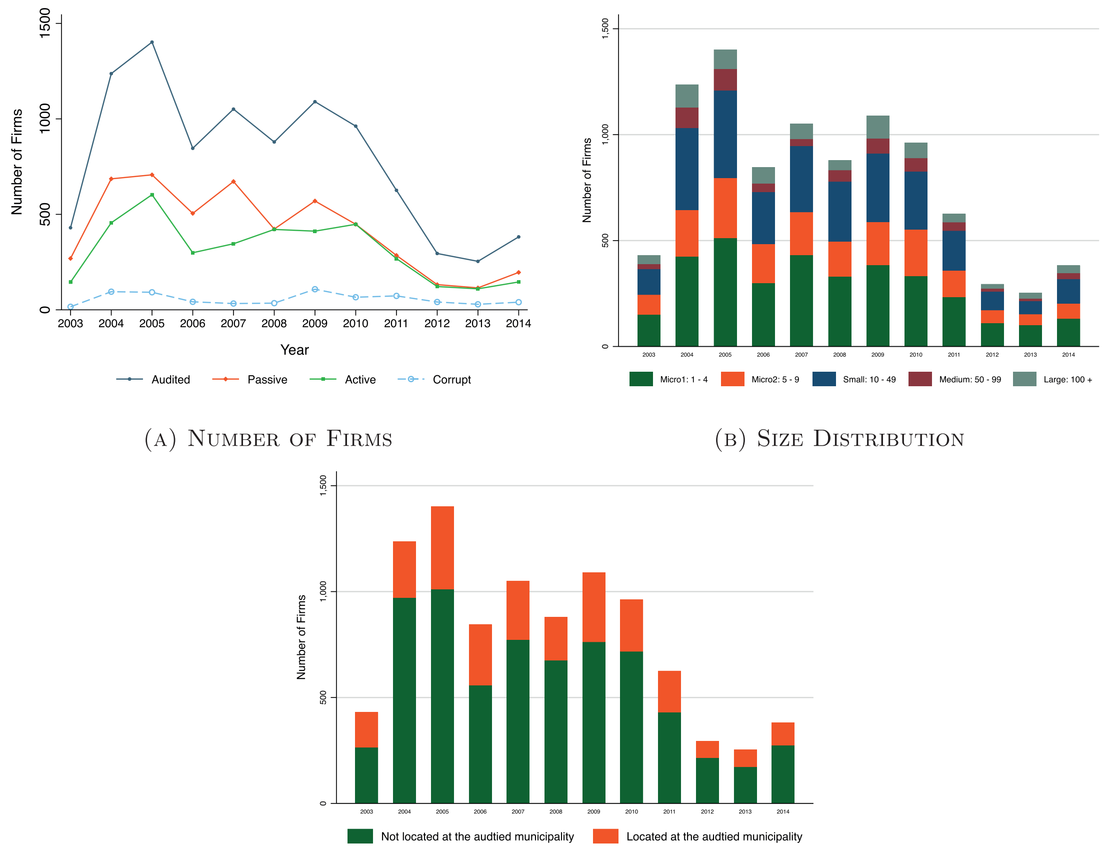
We obtain loan-level data from the Brazilian Development Bank (Banco Nacional do Desenvolvimento - BNDES), the only source of government loans in Brazil. The BNDES is the second largest development bank in the world (after the Chinese Development Bank), and the major lender to Brazilian companies. BNDES provides more than 70% of long-term bank lending in Brazil, and it is the largest source of investment in industry and infrastructure (Colby, 2012). For each loan, we have information on the tax identifier of the firm receiving the loan and the date the loan was received.

A shortcoming of the RAIS dataset is that it lacks balance sheet information, an issue that is common to matched employer-employee datasets on the universe of private sector firms. We alleviate this issue by accessing a unique administrative dataset collected by the Brazilian Institute of Statistics (IBGE), the primary data collection government agency in Brazil. The dataset is called the Annual Industrial Survey (Pesquisa Industrial Anual - PIA), and it is the equivalent of the US Census Annual Survey of Manufacturers. The sample is constructed using two strata: the first stratum (estrato amostrado) includes a nationally representative sample of single-establishment firms with less than 30 employees; the second stratum (estrato certo) consists of all larger firms, which are sampled with probability one. As it is standard in the literature, we use only information from the estrato certo (Ponticelli and Alencar, 2016). Even if for just a small share of our sample, PIA allows us to observe investment and total sales at the firm level.

3.2. Estimation sample and descriptive statistics

We collect 14,316 tax identifiers of firms that appear in all available audit reports. Figure 1 shows the number of audited firms over time. We find that the program was particularly intense in its first few years, with close to 1500 firms being involved in irregularities at its peak in 2005. Approximately 1000 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.[5]

[5] When focusing on firms doing business with municipalities in the State of São Paulo, we find that 2.6% of them were involved in irregularities exposed by the anti-corruption program. Without taking into account the probability of detection, this number is similar to the 2% inci-



(A) Number of Firms

(B) Size Distribution

(C) Location Distribution

Fig. 1. Audited Firms, Location, and Size Distribution.
Notes: This figure presents the number of audited firms and their size and location distribution by year. Panel A presents the number of firms audited by year and type of exposure by the audit from 2003 to 2014. Panel B shows the distribution of firms based on size categories over time for all audited firms. Panel C shows the distribution over time of audited firms based on whether they are located inside or outside the audited municipality.

To construct our analysis sample, we start by matching audited firms to the RAIS administrative database using the tax identifier. We match 9454 of firms to RAIS, but the number drops to 4085 when we restrict the focus on the years in which the local procurement data is available (i.e., post 2008), which we require for the matching.[6] 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 2910 firms. We then drop 1604 firms that do not have a matched control firm, as discussed in Section 4.1. As a result, our most restrictive analysis sample includes a total of 1306 audited firms.[7]

In Table 1, we report summary statistics on the final sample of audited firms using data for the three-year period before the audit. The table highlights that while audited firms are typically small and medium-sized firms, they are relatively larger and more likely to receive a federal procurement contract and a government-subsidized loan compared to the population of firms in Brazil. Specifically, audited firms have a mean of 47 and a median of

dence of auditor-detected fraud among Arthur Andersen clients estimated by Dyck et al. (2021) after the Arthur Andersen demise. Assuming a similar detection probability as Dyck et al. (2021), the estimated overall share of corrupt firms among those involved in public procurement in our setting would be about 10%. Of course, our estimates do not cover all types of corruption a firm can be involved in, but only those related to public procurement. In related work, Decarolis et al. (2020) find that 17% of procurement contracts in Italy are awarded to firms investigated for corruption. In other contexts, Wang et al. (2010) examine IPOs in the US and find that firms have 10–15% probability of engaging in financial fraud. Karpoff et al. (2017a) estimate that 22.9% of Compustat firms with foreign sales are involved in bribery programs. An important caveat in comparing estimates across different studies is that they focus on different types of corruption, as well as on different types of firms in terms of size or private vs publicly traded status.

[6] 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.

[7] The drop in sample size is typical of studies using dynamic difference-in-differences strategies combined to an exact matching approach like the one we discussed in Section 4.1. See Jäger (2019) for a discussion of the trade-offs regarding this approach.

Table 1
Summary Statistics.

	(1)	(2) RAIS Population	(3)	(4)	(5) Audited Firms	(6)
	Mean	Median	Standard Deviation	Mean	Median	Standard Deviation
Employees	15.66	3	351.15	46.52	12	112.62
Managers	0.71	0	20.04	2.47	0	6.67
Non-Manager	14.33	3	308.60	43.27	11	103.47
Wage	486.17	381	2569.26	524.97	438	310.39
Manager's Wage	1,150.38	752	4,279.22	1,282.43	907	1,159.86
Non-Manager's Wage	460.84	373	2415.63	496.80	425	260.80
Any Federal Contracts	0.00	0	0.05	0.05	0	0.21
Number of Federal Contracts	2.30	1	4.59	3.33	2	4.55
Amount of Federal Procurement	317.38	17	3,677.10	1,296.98	70	9,639.44
Any Public Loan	0.03	0	0.16	0.17	0	0.37
Number of Public Loans	3.41	2	8.29	4.07	2	6.76

Notes: This table presents the summary statistics of firms in the Brazilian economy (RAIS Population) as well as audited firms. Wages are in Reais. The amount of federal procurement is in thousand USD.

12 employees, both larger than the population averages of 16 and 3 employees. A non-trivial share of firms receive government-subsidized loans from BNDES (17%) and federal procurement contracts (5%). On average an audited firm has a total amount of federal procurement contracts of USD 1,297,000, with a median of USD 70,000.

Digging deeper into the firm size distribution, Table 2 classifies firms into bins depending on size and shows that the distribution of audited firms is skewed to the right relative to the population of firms. Large and medium-sized firms are more prevalent among the audited firms, while small and micro firms are underrepresented. In particular, around 44% of audited firms have at least 10 employees versus only 21% in the population of firms. The difference is particularly striking in the number of medium-sized firms that have 10 to 49 employees. This finding is consistent with the fact that larger firms are more likely to bid and receive local procurement contracts. Panel B of Fig. 1 plots the evolution of the size distribution of firms involved in irregularities with the local governments in our data and illustrates that the distribution has been relatively stable over time.

Table 2 also reports the distribution of audited 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.

Finally, Panel C of Fig. 1 demonstrates that the vast majority of audited firms are located *outside* of the audited municipality. Notice that the location is the physical location of the establishment for single-plant firms, while for multi-plant firms we define it to be the headquarters of the firm. Indeed, we find that 74% of firms are registered outside the audited municipality, consistent with the fact that several participants in the public procurement process are larger multi-region firms. This is a key feature motivating our research design, as it allows us to study growth patterns at the firm-level while abstracting away from any municipality-level outcome of the audits.

Table 2
Size and Sector Distribution.

	(1) RAIS Population	(2) Audited Firms	(3)
	Percentage	Number	Percentage
Size Distribution:			
Micro (1–4 Employees)	59.98	3442	36.41
Micro (5–9 Employees)	18.45	1881	19.90
Small	17.62	2816	29.79
Medium	2.02	576	6.09
Large	1.93	739	7.82
Sector Distribution:			
Retail	40.40	5563	58.84
Services	29.58	1034	10.94
Construction	7.10	1773	18.75
Other	22.92	1084	11.47

Notes: This table presents the distribution of firms in the Brazilian economy (RAIS Population) and audited firms, by sector and size categories. *Other* sectors include manufacturing, agriculture, mining, and utilities. The size categories are *Micro*: 1–4 employees; *Micro*: 5–9 employees; *Small*: 10–49 employees; *Medium*: 50–99 employees; *Large*: more than 100 employees.

4. The impact of anti-corruption audits on firms

In this section, we start by describing our identification strategy based on a dynamic difference-in-differences design with exact matching (Section 4.1). We then provide direct empirical evidence on the impact of the random auditing program on two main firm-level outcomes: size and access to procurement contracts (Section 4.2). Next, in Section 4.3, we discuss heterogenous effects based on a new classification of firms depending on their degree of involvement in corruption. In Section 4.4, we then investigate economic mechanisms and additional results using firm- and loan- level data on investment and access to finance, worker-level data, and a new survey we administered to a sample of government-dependent firms.

4.1. Identification strategy

The setting we study has several attractive features from an identification perspective. In particular, the timing of firm exposure is plausibly exogenous due to the random nature of the audits, which contrasts several other enforcement actions against firms that are typically triggered by endogenous events linked to the exposure of firms to corruption cases. Nevertheless, 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, as is standard in the literature when the time-series variation is exogenous but the cross-sectional variation is not (Jaravel et al., 2018). 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. These data allow us to select counterfactual firms that—similarly to the exposed ones—provided goods and services to local governments and had a procurement contract awarded and completed 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 contract is completed (Ferraz et al., 2015; 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.

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's 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 the employment and payroll distributions 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, 2021). In the second round, we 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.

When multiple potential control firms are found for a given exposed firm, we select the counterfactual firm as the one with the closest propensity score. The propensity score is computed based on a linear probability model that includes lagged employment levels. As mentioned in Section 3.2, at the end of the full matching procedure we were able to match 1306 firms exposed by the CGU audit program. Table 3 reports diagnostics on the matching using firm characteristics from the year before the audit. First, in columns (1) to (4), we compare exposed firms in our sample to all firms in Brazil using the same set of observable characteristics. It is important to remember here that exposed firms in our sample are those with procurement contracts with the public administration and that are observed consistently in the three years before exposure in the RAIS dataset. Thus, not surprisingly, such firms tend to be larger than the average firm in Brazil (about 70% larger in terms of number of employees). Exposed firms also tend to be growing faster, to use more skilled workers, and to be more present in the retail and construction sectors. These differences, of course, emphasize the need of constructing a plausible set of comparable firms as control. Next, in columns (5) to (8), we compare treated and control firms after matching. As shown, after matching, treated and control firms are comparable along all characteristics, including those that were not used in our matching strategy, such as measures of turnover and skill composition of the labor force.[8]

Several recent papers have discussed important identification problems in staggered difference-in-differences regressions with time and group fixed effects (see, among others, De Chaisemartin and d'Haultfoeuille, 2020 and Goodman-Bacon, 2021). The key issue raised by this literature is that treatment effects might be heterogeneous across groups and over time. This heterogeneity might lead to severe bias, especially when already treated units are used as control group for newly treated units, and the treatment has long-run effects—as in this case, the weights associated to the average treatment effects of different groups can become negative. Our identification strategy, on the other hand, relies on matching each exposed firm with a non-exposed firm that is *never treated* during the period under study. This ensures that the control group of exposed firms is "clean," in the sense that is composed only of similar firms that were never exposed by the auditing program, removing the potential issue of negative

[8] Our matching strategy uses access to public procurement contracts with the municipalities of the State of São Paolo to construct the control group. As such, it is conductive of selecting control firms located in São Paolo. In Table A1, we compare firms in the state of São Paolo with firms in the rest of Brazil along a large set of observable characteristics. As shown, firms in the state of São Paolo are larger in size, have relatively more skilled labor force, are more likely to operate in the services sector and less likely to operate in agriculture. These differences do not invalidate our empirical analysis, since we always compare treated firms with their appropriate controls. Still, they suggest that the effects documented in our paper are more informative of firms operating in urban, industrialized areas of developing countries than of those operating in rural, agricultural ones.

Table 3
Balance Table Before and After Matching.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Before Matching				After Matching			
			Difference				Difference	
	Treated	Rest of Brazil	Mean	Distribution	Treated	Controls	Mean	Distribution
Log Employment	2.280	1.593	0.687***	0.000	2.728	2.754	−0.026	0.886
	(1.411)	(0.995)	(0.015)		(1.434)	(1.410)	(0.055)	
Δ Employment	0.069	0.014	0.054***	0.000	0.063	0.060	0.003	0.804
	(0.494)	(0.369)	(0.006)		(0.228)	(0.214)	(0.009)	
Log Payroll	7.936	7.223	0.713***	0.000	8.634	8.748	−0.114	0.289
	(1.802)	(1.451)	(0.019)		(1.824)	(1.859)	(0.072)	
Δ Payroll	0.136	0.058	0.078***	0.000	0.117	0.112	0.005	0.419
	(0.622)	(0.532)	(0.007)		(0.292)	(0.275)	(0.011)	
Log Employment: Managers	0.492	0.295	0.197***	0.000	0.669	0.611	0.058*	0.084
	(0.864)	(0.601)	(0.009)		(0.887)	(0.897)	(0.035)	
Log Employment: Non-Managers	2.194	1.476	0.718***	0.000	2.672	2.713	−0.040	0.613
	(1.408)	(1.003)	(0.015)		(1.427)	(1.392)	(0.055)	
Δ Employment: Managers	0.065	0.042	0.023***	0.000	0.037	0.047	−0.010	0.260
	(0.414)	(0.343)	(0.005)		(0.270)	(0.248)	(0.010)	
Δ Employment: Non-Managers	0.059	−0.007	0.066***	0.000	0.062	0.057	0.005	0.410
	(0.533)	(0.417)	(0.006)		(0.240)	(0.222)	(0.009)	
Share of Hiring	0.768	0.632	0.135***	0.000	0.322	0.329	−0.007	0.202
	(1.863)	(3.050)	(0.020)		(0.306)	(0.261)	(0.011)	
Share of Firings	0.488	0.366	0.122***	0.000	0.202	0.201	0.001	0.294
	(1.337)	(1.854)	(0.014)		(0.192)	(0.188)	(0.007)	
Share of White Collars	0.541	0.501	0.040***	0.000	0.619	0.613	0.006	0.350
	(0.373)	(0.422)	(0.004)		(0.300)	(0.303)	(0.012)	
Share of Blue Collars	0.377	0.384	−0.006	0.000	0.465	0.476	−0.011	0.431
	(0.366)	(0.417)	(0.004)		(0.278)	(0.281)	(0.011)	
Share of High Skill Workers	0.620	0.574	0.046***	0.000	0.657	0.653	0.005	0.342
	(0.330)	(0.399)	(0.003)		(0.304)	(0.299)	(0.012)	
Average Education	10.828	10.561	0.267***	0.000	11.368	11.309	0.059	1.000
	(2.140)	(2.631)	(0.022)		(2.109)	(1.927)	(0.147)	
Share in Agricultural Sector	0.035	0.124	−0.089***	0.000	0.002	0.001	0.002	1.000
	(0.185)	(0.330)	(0.002)		(0.048)	(0.027)	(0.002)	
Share in Service Sector	0.127	0.326	−0.200***	0.000	0.056	0.067	−0.012	1.000
	(0.333)	(0.469)	(0.003)		(0.229)	(0.250)	(0.009)	
Share in Manufacturing Sector	0.079	0.121	−0.042***	0.000	0.053	0.053	0.001	1.000
	(0.270)	(0.326)	(0.003)		(0.225)	(0.223)	(0.009)	
Share in Retail Sector	0.596	0.399	0.197***	0.000	0.791	0.777	0.014	1.000
	(0.491)	(0.490)	(0.005)		(0.407)	(0.416)	(0.016)	
Share in Construction Sector	0.163	0.030	0.133***	0.000	0.071	0.075	−0.004	1.000
	(0.369)	(0.171)	(0.004)		(0.256)	(0.263)	(0.010)	

This table presents the difference between treated firms and the population of firms in Brazil before and after matching. Columns 1 and 2 (5 and 6) present the average and standard deviation for treated firms and the population of firms (treated firms and their matched controls). Column 3 (7) presents the average difference between columns 1 and 2 (5 and 6), and the standard deviation of the difference. Columns 4 and 8 present the p-value for Kolmogorov-Smirnov test for the difference in the distribution of the characteristics between treated firms and the population of firms in Brazil. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

weights. In this sense, our empirical specification is similar to the stacked regression estimator approach discussed in Baker et al. (2021), a recent application of which can be found in Cengiz et al. (2019).

4.2. Main effects on firm growth and access to procurement contracts

We start by documenting the effect of exposure on firm size, which is the main firm-level measure we can observe from the RAIS dataset and that captures firm growth. In particular, we estimate the following specification for a time-window of 7 years around the audit:

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

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 for the exposed firm and their control, 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 4.1. α_i and α_t are firm and year fixed effects that aim at capturing any observed and unobserved firm characteristic that is fixed over time, and aggregate level shocks at the year level that affect all firms similarly. ε_{it} is an error term that we cluster at the firm level.[9] Our parameter of interest is β_2, which captures the change

[9] We cluster standard errors at the firm-level rather than at the municipality level because neither treated firms nor their respective controls are located in audited municipalities, attenuating concerns of spatial correlation. In Table A2, we show that the results documented in Table 4 are ro-

Table 4
The Impact of Audits on Firms.

	(1)	(2)	(3)	(4)
			Federal Procurement	
	Employment	Exit	Any Contract	Ln Amount
Post × 1(Exposed)	0.048***	0.002	−0.020***	−0.214***
	(0.017)	(0.005)	(0.006)	(0.074)
Post	−0.023*	0.044***	0.014***	0.153**
	(0.012)	(0.005)	(0.005)	(0.066)
Observations	16,986	16,986	15,284	15,284
R-squared	0.952	0.245	0.612	0.642
Mean Dep. Variable	2.610	0.000	0.050	0.640
Firm Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES

Notes: This table presents the effects of the audit on firms. Column (1) presents the effect on the logarithm plus one for total employment, column (2) on a dummy that takes the value one if the firm exits the market that year, column (3) on a dummy that takes the value one if the firm had at least one federal procurement contract, and column (4) on the logarithm of the total amount contracted with federal procurement plus one. The sample consists of audited firms and their matched controls. Section 4.1 details the matching method. Standard errors clustered at firm level reported in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

after the audit in the outcome variable of exposed firms relative to the matched controls, taking into account any fixed characteristics at the firm-level as well as year-level shocks.

Table 4 reports the results. As shown in column (1), we find that firms exposed by the random auditing program experience an increase in employment after their exposure. In particular, the magnitude of the estimated coefficient β_2 indicates that exposed firms experience, on average, a 4.8% larger increase in size in the three years after being exposed by the audit relative to the control group.

Next, in column (2), we study the impact of exposure on the probability of firm exit. One potential explanation of our result on employment is that worse-performing exposed firms are more likely to exit after exposure, leaving in our sample only those exposed firms that grew after exposure. To analyze the impact of composition, we estimate Eq. (1) using as outcome a dummy equal to one for firm exit. We find that the random auditing program had a small and statistically insignificant impact on the exit probability of exposed firms relative to the control group, which indicates that our effects on firm size are not simply driven by compositional changes.

Finally, we study the impact of exposure by the random audit program on firms' ability to obtain procurement contracts. Access to government contracts is a key outcome in our analysis, given the nature of the program we study, which targets firms involved in corruption cases with government officials. To this end, we rely on data on procurement contracts from the federal government, which we can access for all firms in our sample.

The results are reported in columns (3) and (4) of Table 4. In column (3), we estimate Eq. (1) where the outcome is an indicator variable capturing whether the firm obtained *any* procurement contract in a given year. We find that firms exposed by the random audit program are on average 2 percentage points—in any given year—less likely to receive federal procurement contracts after exposure, which represents a considerable decrease of around 40% with respect to the sample mean.

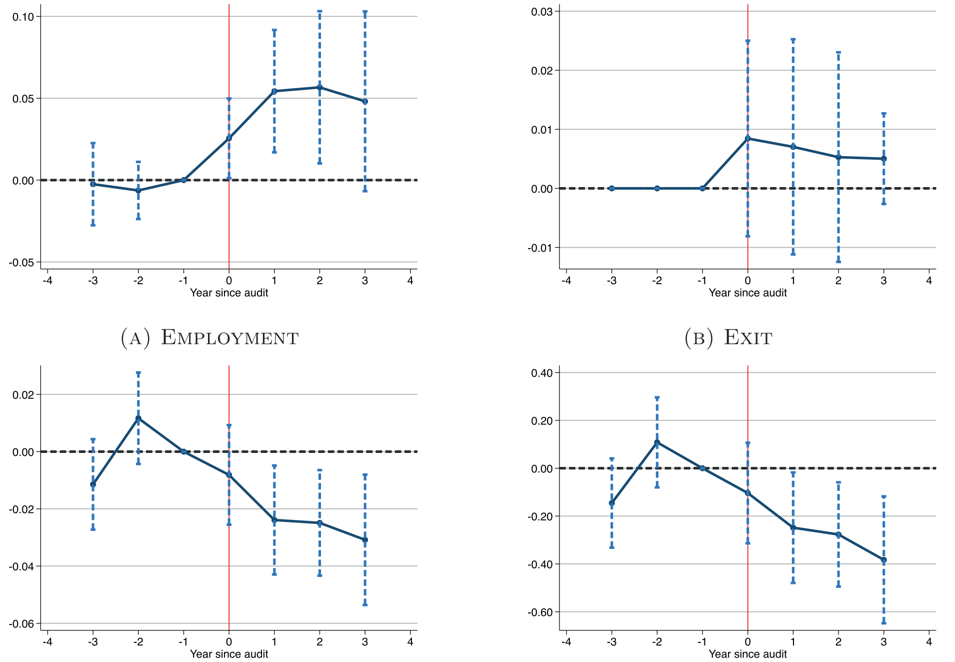
Next, in column (4), we study the impact of exposure on the value of the procurement contracts obtained by the firm. The outcome variable is the log of the total value of all federal procurement contracts obtained by a given firm. The estimated coefficient indicates a relative decline in the value of procurement contracts of about 21% in the three years after exposure.

To assess the validity of our identifying assumptions and explore the timing of the effects on firm size, exit, and access to procurement contracts, we further estimate the following dynamic specification, where we normalize the coefficients relative to the year before the audit:

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

In Fig. 2, we report the estimated coefficients β_k for each of the main outcomes. As shown in Panel (a), we find no differential trends in firm size between audited firms and their controls in the period before exposure. The effect of the anti-corruption program on firm size starts materializing 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. Consistent with the results reported in Table 4, we find no significant effect on exit for exposed firms in the post-exposure period, while the estimated difference between the two groups is zero by construction in the years before exposure (as we condition on firms being in operation in the three years before exposure for both

bust to allowing for different levels of clustering, including: municipality of audit (Panel A); treated firm-control firm pairs (Panel B); municipality of audit and year of the audit (Panel C). Finally, in Panel D, we collapse the data pre and post-audit to avoid underestimation in the standard errors because of serially correlated outcomes (Bertrand et al., 2004). In all cases, our results remain significant with p-values < 0.01.



(A) Employment (B) Exit

(C) Any Procurement contract (D) Ln amount Procurement contracts

Fig. 2. Audits, Firm Growth, and Access to Procurement Contracts.

Notes: This figure presents the estimation from the following specification: $y_{it} = \alpha_i + \alpha_t + \sum_{k=-3,\ldots,3}^{k\neq-1} \alpha_k * I\{t=k\} + \sum_{k=-3,\ldots,3}^{k\neq-1} \beta_k * I\{t=k\} * Exposed + \epsilon$, where it controls for firm fixed effect and year fixed effect. The dependent variables are the logarithm of one plus employment (Panel A), a dummy for exiting (Panel B), a dummy for having any federal procurement contract (Panel C), and the logarithm of the total amount contracted in federal procurement plus one (Panel D). The firms in the regression sample are audited firms and their matched control. Section 4.1 details the matching method.

treatment and control group). Finally, Panels C and D show the dynamic effect on access to and value of federal procurement contracts, again showing no major differences in the pre-period and a stark (negative) effect in the post-period.

In sum, the combination of the findings presented in Table 4 and Fig. 2 show that while exposure from the anti-corruption campaign decreases the firms' ability to rely on government contracts, it also benefits firm performance. In the next sections, we analyze this seemingly counter-intuitive empirical finding in more detail. First, in Section 4.3, we study whether the effect of exposure on firm-level outcomes varies by degree of involvement in corrupt practices. Second, in Section 4.4, we explore several potential mechanisms in more details.[10]

4.3. Heterogeneous effects by degree of involvement in corruption cases

As a first step towards understanding the results documented in the previous section, we dig deep into the granular data we collect on all corruption cases described in the audit reports. In fact, a unique feature of our setting is that it allows us to differentiate exposure of firms in the audit reports by the degree of their involvement in corrupt practices. This is because auditors are required to report all possible firms involved in the corruption together with a description of the nature of the involvement. After manually going over all the descriptions of thousands of cases in our data, we uncover a large degree of heterogeneity in firm exposure. Our ability to differentiate across corruption *types*—emphasized to be crucial yet under-explored in one of the first ever studies of firm-level corruption by Svensson (2003)—represents a main contri-

[10] Tables A3 and A4 report two important robustness tests of the results reported in Table 4. The first is about repeated treatment. In our setting, there are two potential instances of repeated treatment: (i) firms might be exposed multiple times in different anti-corruption audits, and (ii) municipalities might be audited multiple times during the period under study. While less than 1% of firms experience repeated treatment, there are 231 firms that were exposed during the second or more audit of the same municipality. In this case, the intensity of the treatment could be different, given existing evidence showing that the level of corruption is reduced after a municipality is audited the first time (Avis et al., 2018). Table A3 shows that the results reported in Table 4 are robust to restricting our sample to firms that were exposed only once and their controls (Panel A) and to firms exposed during only the first audit of municipalities and their controls (Panel B). The magnitude of the estimated coefficients remains similar to the one obtained with our full sample, indicating that repeated treatment is not driving our main results. Next, in Table A4, we show that our results are robust to alternative specifications with additional fixed effects. In particular, we augment Eq. (1) with: municipality of audit times year fixed effects (Panel A), municipality of location times year fixed effects (Panel B), and matching pair (stratum) times year fixed effects (Panel C). All results are robust to estimating these more saturated models.

bution of our paper. Based on our reading, we therefore manually classify firms whose names appear in the audit reports in three categories depending on their degree of involvement in the exposed corruption: passively involved, actively involved, and corrupt.[11]

First, we consider as *passively involved* those firms who are mentioned in the audit report as being linked to an irregularity, but seem to be the victim of it. In this case, the most common example is the one of losing bidders to an irregular public procurement process. They are exposed by the program, because auditors are instructed to do so, but there is no direct evidence that they are corrupt firms benefiting from preferential treatment.

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 is one of over-invoicing for a specific good or product. Over-invoicing 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 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, the evidence shows the quality of goods or services provided by these firms to the local government is satisfactory, unlike the subsequent case of corrupt firms.

Finally, 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 did so in an unsatisfactory manner). These clear-cut cases of corruption represent a minority—approximately 7%—of all irregularities we observe.

Panel A of Fig. 1 plots the number of audited firms over time based on the degree of a firm's involvement in corrupt practices. We do not observe significant differences in the extent of involvement of firms over time, with a large and mostly equal share of *passively* and *actively involved* firms, and a small share of *corrupt* ones across the entire sample period. Table 5 provides summary statistics on audited firms depending on the degree of involvement. *Actively involved* firms appear to be the smallest, employing on average 37 employees, whereas *passively involved* and *corrupt* firms have a mean of 54 and 58 employees, respectively. Average wages are, instead, comparable across firms with different degrees of involvement. *Corrupt* firms have, on average, a lower number of federal procurement contracts and a higher access to government-subsidized lending compared to *passively* and *actively involved* firms. Overall, however, we do not observe large differences in terms

[11] Firm classification in different categories was done manually by a team of Brazilian research assistants. Appendix A.1 describes the data collection process in detail, including the instructions provided to the RAs for the classification. Trained supervisors were responsible for quality checks of all data entered.

Table 5
Summary Statistics by Type of Exposure.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Exposed: Passive			Exposed: Active			Exposed: Corrupt			Suspended		
	Mean	Median	Standard Deviation	Mean	Median	Standard Deviation	Mean	Median	Standard Deviation	Mean	Median	Standard Deviation
Employees	54.43	12	129.24	37.00	11	93.32	58.03	15	93.60	128.23	13	363.83
Managers	2.97	0	7.78	1.91	0	5.35	2.85	1	5.31	3.07	1	7.02
Non-Manager	50.34	11	117.78	34.61	10	86.82	55.00	14	88.95	124.96	12	359.58
Wage	551.85	449	343.17	497.34	426	277.51	519.38	452	228.16	534.03	454	294.68
Manager's Wage	1,393.69	971	1,273.58	1,180.27	849	1,053.80	1,084.11	873	699.85	1,012.12	762	685.80
Non-Manager's Wage	518.26	435	285.04	474.22	416	236.51	497.25	437	203.54	502.05	440	250.84
Any Federal Contracts	0.06	0	0.23	0.04	0	0.20	0.04	0	0.20	0.33	0	0.47
Number of Federal Contracts	3.61	2	4.54	3.14	1	4.74	1.54	1	1.36	2.87	2	3.65
Amount of Federal Procurement	1,664.38	76	12,585.80	870.18	56	3,381.14	503.06	178	740.21	1,001.45	75	5,174.20
Any Public Loans	0.16	0	0.36	0.17	0	0.38	0.22	0	0.42	0.16	0	0.37
Number of Public Loans	4.40	2	7.85	3.75	2	5.59	4.18	2	6.26	4.81	2	7.61

Notes: This table presents the summary statistics for audited firms by type of exposure (columns (1)–(9)) and for suspended firms (columns (10)–(12)). Wages are in Reais. The amount of federal procurement is in thousand USD.

Table 6
Type of Corruption Exposure and Employment Growth.

	(1)	(2)	(3)	(4)	(5)
	Audited				
	Passive	Active	Corrupt	Total Sample	Suspended
Post × 1(Exposed)	0.072***	0.048**	−0.188*	0.071***	−0.897***
	(0.024)	(0.025)	(0.110)	(0.024)	(0.107)
Post × 1(Exposed) × Active				−0.022	
				(0.034)	
Post × 1(Exposed) × Corrupt				−0.269**	
				(0.112)	
Post × Active				0.015	
				(0.022)	
Post × Corrupt				0.054	
				(0.065)	
Post	−0.024	−0.022	−0.032	−0.032**	0.383***
	(0.016)	(0.018)	(0.079)	(0.016)	(0.083)
Observations	8274	7882	830	16,986	1694
R-squared	0.959	0.948	0.925	0.952	0.855
Mean Dep. Variable	2.676	2.511	2.976	2.615	3.266
Firm Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	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) to (4), we present results for the audited sample which consists of audited firms and their matched controls. In column (5), we present results for the suspended sample which consists of suspended firms and their matched control. Section 4.1 details the matching method. Standard errors clustered at firm level reported in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

of firm characteristics for the median firm across different types of exposure to corrupt practices. Finally, Fig. A1 plots the evolution of the size and spatial distribution of audited firms depending on the degree of involvement.

To study how the effect of the random audit program on firm size differs by type of exposure, we start by estimating Eq. (1) separately for each group of firms depending on their degree of involvement. The results are reported in columns (1) to (3) of Table 6. The magnitude of the point estimate in column (1) indicates 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 7.2% larger increase in size with respect to the control group after exposure, against the 4.8% average effect documented in column (1) of Table 4. These results are consistent with the fact that *passively involved* firms that appear in the audit reports are often firms that were victims of the corruption scheme, who end up being exposed in the audit reports due to the requirements for the auditors to list all possible firms related to the specific government contract under examination.

Interestingly, in column (2) of Table 6, we find that firms reported as *actively involved* in the corruption scheme experience a smaller but still positive and significant increase in size, which is similar in magnitude to the effect on the average firm in our sample. This finding indicates that our effects are not just driven by the unique feature of the CGU anti-corruption audits that expose non-guilty firms, because also firms directly involved in the corruption perform better after the audits. On the other hand, *corrupt* firms experience a strong and significant decline in employment of about 20% after their exposure in the audit reports, as shown in column (3).

To study whether differences in the effect of exposure across firms with different degrees of involvement are statistically significant, we also estimate the following specification:

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

The results are reported in column (4) of the same table. 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. Consistently, the magnitude of the point estimate on the excluded category is similar to the one reported in column (1). Firms reported as *actively involved* in the corruption scheme experience a smaller but not significantly different increase in size. On the other hand, the effect on *corrupt firms* is significantly different than the one on *passively involved* firms. The sum of the estimated coefficients β_2 and β_4 indicates that the 20% relative decline in size for corrupt firms is statistically significant.

One concern with our measures of heterogeneous exposure to corruption is that it is inherently difficult to attribute guilt in corruption cases, where it is often challenging even for prosecutors to charge specific parties. Hence, we might be worried about mis-classification, and that all or most exposed firms might instead be politically connected firms that continue obtaining government favors even after the audit. However, this story is in direct contradiction with the negative effects on access to procurement contracts for exposed firms. We provide further contextual evidence and make our measures more transparent by reporting a random sample of detailed examples of irregularities in Appendix A.2.

In addition, we provide corroborating evidence that corrupt firms shrink in size after they lose preferential access to government contracts, by studying the impact of the CEIS program on firm size. Briefly introduced in Section 3.1.3, CEIS is a different but related transparency initiative started by the federal government in 2008, whereby highly corrupt firms found guilty in court of wrongdoing in dealings with the government are formally banned from participating in public procurement. While the CEIS program does not offer the random-by-design variation in the timing of exposure, it helps maximize external validity. 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 courts and punished through a suspension from getting government contracts. To estimate these effects, we use a similar matching strategy and estimate the same specification described in Eq. (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. The time-series variation is given by the year of suspension from accessing public procurement contracts. As we report in Table 6, column (5), 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. This large and negative impact on suspended firms is consistent with our results on corrupt firms exposed by the random audit program, thus providing some validation for our categorization of firms across the corruption involvement spectrum.

Figure 3 reports the heterogeneous effects by type of exposure in a dynamic specification. As shown, the positive effects on *passively involved* firms materialize already in the year of exposure. The effect on *actively involved* firms is also positive but milder when compared 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 materializes in the year of suspension. Reassuringly for our identification strategy, we find a widespread lack of differential preexisting trends.

Finally, in Table 7, we analyze heterogeneous effects by type of exposure on two additional outcomes: firm exit and access to procurement contract. Two results emerge. First, although we find no significant differences in the probability of exit across firms with different degree of involvement, point estimates suggest that *Corrupt* firms are more likely to exit in the post exposure period relative to passively and actively involved firms. Second, firms with different degrees of involvement are all negatively affected in terms of access to federal contracts. However, these negative effects are larger for *Corrupt* firms, both in terms of access to contracts and their monetary value. We also show in Table 7, columns (4) to (6), that firms blacklisted as part of the CEIS program experience a higher likelihood of exits and a complete loss of public procurement access, respectively.

To sum up, the results reported in this section show that the effect of the random audit program on firm-level outcomes is heterogeneous across firms with different degrees of involvement in corruption. In particular, the positive impact of exposure on firm growth is limited to those that we classify as either victims of the corruption scheme, or firms that were actively involved in the corruption case but that did provide the goods or services requested by local governments. We also show that all firms rely less on federal procurement contracts after exposure, although the most corrupt ones experience the largest decline.

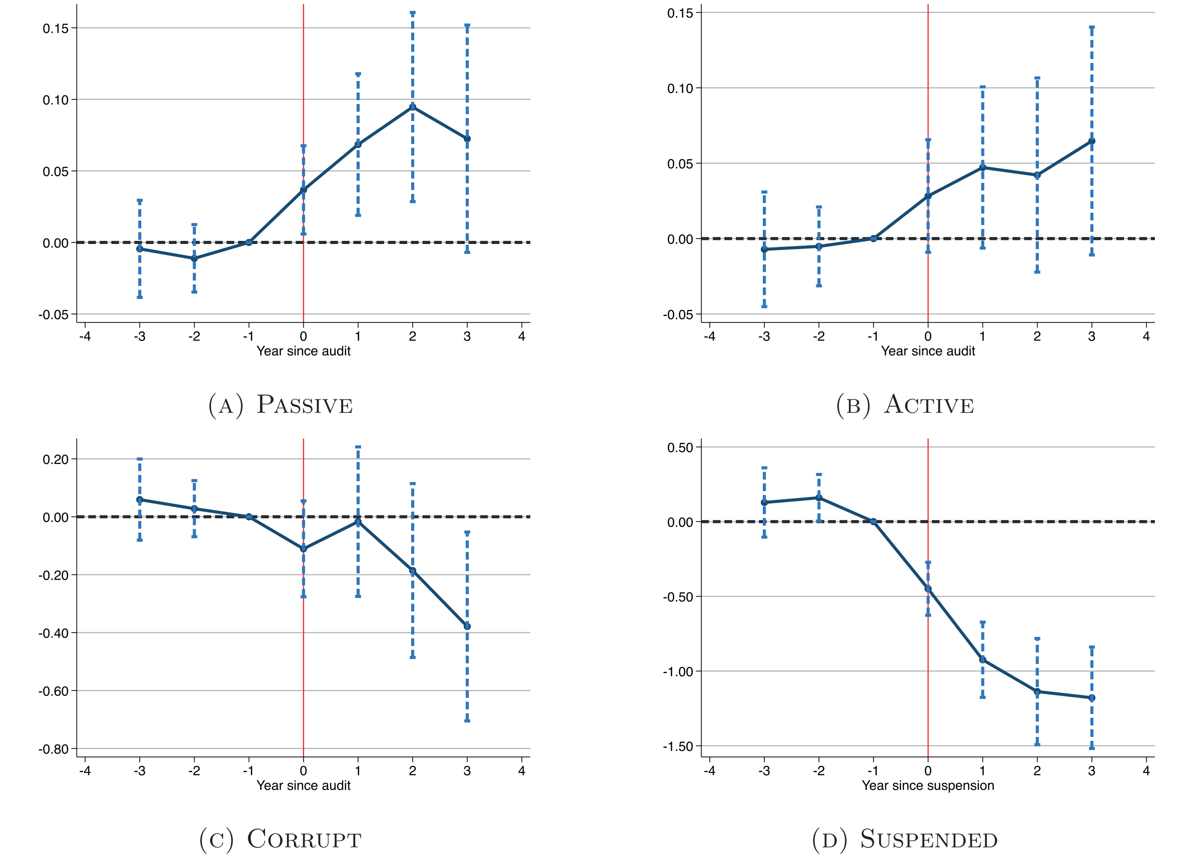
4.4. Discussion of mechanisms

In this section, we investigate the mechanisms that can rationalize both the positive effect of exposure on firm growth and its negative effect on access to procurement contracts. Our analysis is motivated by the existing literature that has shown that firms that rely more on government contracts tend to grow slower and invest less in tangible and intangible capital (Cohen and Malloy, 2016). Building on this literature, we study whether the revelation of corruption, by cutting access to government contracts, pushes firms to change their investment and business practices in order to compete for private sector's demand. In particular, 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. To investigate this mechanism, we obtain access to confidential data on firms' investment and access to credit, which allows us to analyze whether the loss of procurement contracts corresponds to a change in investment at the firm level. We further provide additional results based on worker-level data, which allow us to rule out a channel according to which firms are able to grow despite losing access to government contracts because the revelation of corruption forces a change in their internal organization. We conclude the section by presenting evidence in support of our empirical tests using original face-to-face surveys of owners of small and medium government-dependent firms, and by briefly discussing the issue of direct versus indirect effects on the audits.

4.4.1. Investment, sales, and external finance

Let us start by testing whether the revelation of corruption, by restricting access to government contracts, forces firms to change their investment and business practices in order to compete in the private sector. We do so by testing the impact of exposure on investment using data from a comprehensive survey of Brazilian manufacturing firms (PIA), akin to the US Annual Census of Manufacturers. We also explore whether firms borrow more to change their investment strategy using loan-level data from the development bank (BNDES), the primary lender for small- and medium-size manufacturing firms in Brazil which specializes in corporate loans financing fixed capital investment.

The results are reported in Table 8, where the smaller sample size for the PIA analysis reflects the fact that manufacturing firms with at least 30 employees are only a sub-



(A) Passive (B) Active

(C) Corrupt (D) Suspended

Fig. 3. Type of Corruption Exposure and Employment Growth.
Notes: This figure presents the estimation from the following specification: $Log(1+L)_{it} = \alpha_i + \alpha_t + \sum_{k=-3,\ldots,3}^{k\neq-1} \alpha_k * I\{t=k\} + \sum_{k=-3,\ldots,3}^{k\neq-1} \beta_k * I\{t=k\} * Exposed + \epsilon$, 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 C, we present the effects of the audits by type of exposure, while Panel D shows the effect of suspension. The firms in the audit regression sample (Panels A to C) are audited firms and their matched control. The firms in the suspension regression sample (Panel D) are suspended firms and their matched control. Section 4.1 details the matching method.

set of the firms in our sample. We start in column (1) by studying the effect of firm exposure to the random audit program on capital investment. The outcome variable is constructed as the monetary value of capital investment as a share of sales. We find that exposed firms experience a larger increase in capital investment in the post-exposure period. The magnitude of the coefficient indicates that exposed firms increase investment by 2 percentage points more than the control group as a share of their sales, which represents a 50% increase with respect to the average of the dependent variable. In column (2), we also document that exposed firms experience a relative increase in sales of 13%. This is important, as it indicates that the result on investment is not driven by a negative effect of exposure on sales, and is consistent with a change in growth strategy that improved firm performance. Moreover, the increase in sales after exposure further corroborates our main employment-based results that exposed firms grow after the anti-corruption audits.

Finally, in column (3), we study the impact of exposure on firm borrowing. We match loan-level data with our firm-level dataset using the unique tax identifiers. We find that exposed firms experience a significant increase in the number of loans obtained from BNDES. The magnitude of the coefficient indicates that exposed firms have about 0.24 more loans from BNDES in any given year in the post-exposure period relative to the control group, which represents an increase of 59% with respect to the mean of the dependent variable. This positive effect is consistent with an increase in credit demand to finance long-term investments.

Overall, while the sample size is too limited to distinguish across different firms based on their involvement in the corruption, this evidence helps rationalize our findings on the positive effects of exposure on the majority of audited firms. In particular, it is consistent with such effects being at least in part driven by a shift of exposed firms' growth strategy away from a reliance on government contracts.

4.4.2. Worker-level evidence and the internal organization of firms

Next, we leverage the granularity of our administrative dataset at the individual level to characterize the extent and direction of the impact of the CGU anti-corruption program on the labor market outcomes of employees of

Table 7
Type of Corruption Exposure, Exit, and Public Procurement.

	(1)	(2)	(3)	(4)	(5)	(6)
	Audited			Suspended		
	Exit	Federal Procurement		Exit	Federal Procurement	
		Any Contract	Ln Amount		Any Contract	Ln Amount
Post × 1(Exposed)	−0.003	−0.013	−0.145	0.180***	−0.447***	−5.148***
	(0.008)	(0.009)	(0.100)	(0.017)	(0.052)	(0.606)
Post × 1(Exposed) × Active	0.009	−0.010	−0.074			
	(0.011)	(0.012)	(0.143)			
Post × 1(Exposed) × Corrupt	0.023	−0.060*	−0.760*			
	(0.028)	(0.033)	(0.443)			
Post × Active	−0.005	−0.002	−0.064			
	(0.007)	(0.006)	(0.066)			
Post × Corrupt	0.001	0.019	0.235			
	(0.018)	(0.017)	(0.246)			
Post	0.046***	0.015**	0.176**	0.104***	0.406***	4.650***
	(0.007)	(0.006)	(0.079)	(0.020)	(0.047)	(0.571)
Observations	16,986	15,284	15,284	2090	1326	1326
R-squared	0.245	0.612	0.642	0.230	0.569	0.598
Mean Dep. Variable	0.000	0.050	0.640	0.000	0.370	4.460
Firm 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 heterogeneous effects on public procurement contracts for the audited and suspended sample. The sample consists of audited firms and their matched control firms (columns 1 to 3) and suspended firms and their controls (columns 4 to 6). Section 4.1 details the matching method. In columns (1) and (4), the dependent variable is a dummy for exiting. In columns (2) and (5), the dependent variable is a dummy for having at least one contract from federal procurement, while in columns (3) and (6) is the logarithm of total federal procurement amount plus one. Standard errors clustered at firm level reported in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8
Investment, Sales, and Government Loans.

	(1) Investment	(2) Ln(Sales)	(3) # of Loans
Post × 1(Exposed)	0.020**	0.130*	0.237**
	(0.010)	(0.070)	(0.100)
Post	−0.017*	−0.047	−0.186**
	(0.010)	(0.055)	(0.083)
Observations	1520	1520	16,510
R^2	0.014	0.120	0.451
Mean Dep. Variable	0.041	15.48	0.402
Firm Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES

Notes: This table presents the estimation of the effect of the audit on investment, sales, and access to government funding. The sample consists of audited firms and their matched controls. Section 4.1 details the matching method. The outcomes are the capital expenditure over sales (column 1), the logarithm of sales (column 2), and the number of outstanding loans (column 3). The first two outcomes come from the Brazilian manufacturing census (PIA). Standard errors clustered at firm level reported in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

exposed firms. The employee-level analysis has two objectives. First, to the extent that corruption exposure is valued negatively on the labor market, the audits may independently influence individual outcomes in addition to the direct consequences on firms. Such analysis is particularly important in light of the scarce but growing body of empirical evidence in the literature.[12] Second, and importantly, understanding how audits affect the employees of exposed firms helps further refine our analysis of economic channels. Specifically, it is possible that audits lead firms to fire corrupt managers or other employees that were engaging in corruption for personal gain, and, therefore, restructure internally. While not inconsistent with the main mechanisms we discussed earlier, such hypothesis would add a different dimension to why firms grow after the anti-corruption crackdown.

To investigate the impact of the anti-corruption program on employees, we rely on detailed information on employee characteristics and the employer-employee structure of our dataset that allows us to incorporate labor market transitions in our analysis by following employees both over time and across firms. Specifically, we estimate a worker-level version of Eq. (1) and examine the earnings, employment, and reallocation effects of the anti-corruption program. For our analysis, we restrict our focus only on the set of employees present in treated and control firms at the time of the exposure so as to address potential con-

[12] 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 the market for directors disciplines those who are involved in mismanagement or misconduct (Srinivasan, 2005; Fich and Shivdasani, 2007; Fos and Tsoutsoura, 2014). Karpoff et al. (2008) and Karpoff et al. (2014) study the career consequences for CEOs and culpable executives when involved in financial misrepresentation or for "cooking the books." In a context related to ours in Brazil, Szerman (2020) finds that employees of disbarred firms experience a significant loss in both earnings and their probability of employment. There is also evidence of tolerance for misconduct in the labor market. For example, in the setting of financial advisors, while Egan et al. (2019) find increased turnover rates for advisors who previously engaged in misconduct, they also find that 44% of advisers who lost their jobs after misconduct find employment in the industry within a year. Similarly, Helland (2006) provides evidence of a premium placed on employees and managers with experience navigating these challenging situations.

Table 9
The Impact of Audits on Workers.

	(1) Stay {0,1}	(2) Employed {0,1}	(3) Pay Unconditional	(4) Pay Conditional
Panel A: All Workers				
Post × 1(Exposed)	−0.030	0.009	0.075	0.016*
	(0.039)	(0.008)	(0.056)	(0.009)
Post	−0.114***	0.020***	0.149***	0.004
	(0.034)	(0.005)	(0.032)	(0.006)
Observations	913,850	913,850	913,850	819,724
R^2	0.483	0.307	0.430	0.916
Mean Dep. Variable	0.81	0.89	5.85	6.56
Panel B: Managers				
Post × 1(Exposed)	−0.018	−0.005	0.012	0.033**
	(0.030)	(0.009)	(0.077)	(0.014)
Post	−0.066 ***	0.039***	0.288***	−0.014
	(0.024)	(0.007)	(0.056)	(0.011)
Observations	54,530	54,530	54,530	50,391
R^2	0.468	0.322	0.440	0.935
Mean Dep. Variable	0.87	0.95	7.09	7.45
Panel C: Non-Managers				
Post × 1(Exposed)	−0.031	0.010	0.079	0.015
	(0.040)	(0.008)	(0.058)	(0.010)
Post	−0.117***	0.019***	0.141***	0.004
	(0.036)	(0.005)	(0.032)	(0.006)
Observations	854,280	854,280	854,280	764,564
R^2	0.484	0.307	0.422	0.906
Mean Dep. Variable	0.81	0.89	5.77	6.49
Employee Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Difference M-NM	0.013	−0.020	−0.075	0.024
p-value difference Panel B vs C	0.616	0.076	0.319	0.127

Notes: This table presents the estimation of the effect of audits on the worker-level outcomes. The sample consists of workers from audited firms and their matched controls that consists of workers who were in the firm at the time of the audit. Section 4.1 details the matching method. Stay is an indicator variables that is equal to one if the individual is employed at the same firm that he was employed at $t = -1$. Employed is an indicator variable that is equal to one if the individual is employed at any firm during the year. Pay unconditional is the logarithm of one plus the wage, while pay conditional is the logarithm of the wage. We also present the p-value for the difference in the Post × 1(Exposed) coefficient between panel B and C. Standard errors are clustered at firm level reported in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

cerns related to compositional bias arising from firm-level entry and exit of employees with heterogeneous characteristics over time. We focus on four primary labor market outcomes related to employment and earnings. First, we consider potential reallocation effects by creating an indicator variable that is equal to one if an individual remains employed at the treated and control firms at the end of each year, and zero otherwise. The second outcome is an indicator variable that captures the employment status of each worker in each year by taking the value of one if an individual is employed at the end of the year, and zero otherwise. Third, we consider the average monthly wage of each worker, independently of whether the individual is employed or not after the year of exposure, thus imputing zeros for unemployed individuals. Our fourth 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 9. Our specification includes employee fixed effects to capture time-invariant individual heterogeneity, and year fixed effects to account for nationwide time trends. We start by presenting results for all workers in Panel A. In Panels B and C, we split workers into those employed at the time of the exposure in managerial and non-managerial positions, respectively, given that the previous literature has strongly highlighted the presence of large differences in the labor market outcomes of the two groups. In fact, within the small and medium-sized firms in our sample, contracts with the government are typically handled by managers and employees at the top of the organizational layers.[13]

By and large, our coefficient estimates indicate that there are no differential labor market effects between treated and control employees in the post-exposure period. Specifically, the anti-corruption program is not associated with significant employment effects, both in terms of remaining employed at the exposed firm, or being employed

[13] Moreover, in our context, 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. 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.

at any firm at the end of the year. The lack of a differential effect of exposure on the probability of "staying" with the firm documented in column (1) suggests that the difference in post-exposure growth in employment might be mostly due to differential hiring of new employees rather than differential retention of existing employees.

When focusing on the wage component, our coefficient estimates on unconditional pay are statistically insignificant, whereas the estimates on conditional pay are marginally significant at the 10% level. In fact, when we decompose the effect on conditional pay into employees in managerial and non-managerial positions, we observe that the positive effect is largely concentrated on managers, implying that incumbent managers do not experience any reputational costs, and potentially capture part of the increase in sales in the post-audit period. In Table A5 in the Appendix, we repeat the analysis by including heterogeneity by the type of exposure, and continue to find no significant labor market effects for incumbent employees. Notice though that the coefficient estimate in column (1) of Panel B for corrupt firms provides some evidence that exposure by the audit program is associated with a lower likelihood for incumbent managers to remain employed in highly corrupt firms. However, despite experiencing a higher probability of separation, managers of corrupt firms do not appear to be punished by labor markets.

Overall, our findings indicate that exposure by the anti-corruption program did not significantly affect the employability or the compensation of managers and other workers employed by exposed firms, and therefore that the audits did not have a meaningful impact on the internal organization of these firms. This is consistent with the survey evidence we discuss next, where we see that while firms report corruption affecting several of their operational practices, they do not report corruption to have a strong impact on their internal organization choices.

4.4.3. A new survey of corruption and firm strategy

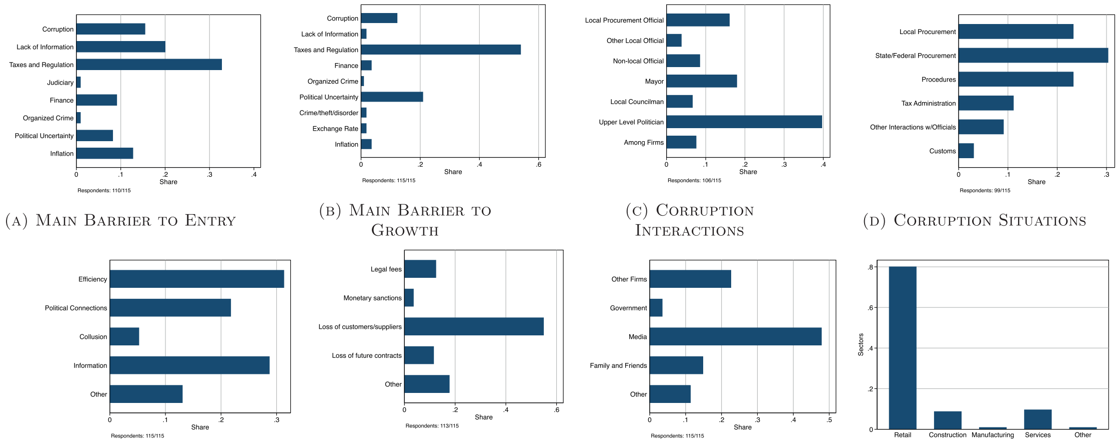
The evidence so far relies on rich administrative data at the firm level. However, administrative data on firms are typically limited in several dimensions, such as the ability to observe changes in management and operational practices. These issues are even more severe when the focus is on small and medium private firms, and where a key mechanism of interest is a shift away from corrupt practices in government interactions to competing for private demand.

We therefore provide richer, largely descriptive evidence to complement our analysis using original surveys of owners of small and medium government-dependent firms representative of our context. Through these surveys, we aim to unpack even more the link between corruption and firm strategy, which we argue is a central driver of our empirical findings. In-depth surveys are typically used as either stand-alone or corroborating evidence in contexts where administrative data alone are not sufficient to identify all economic channels at play, such as in the context of investors' (Gompers et al., 2016; 2020) and CFOs' (Graham and Harvey, 2001) decision-making, with face-to-face surveys considered to be the ideal format when feasible (Bloom and Van Reenen, 2007).

We administered the surveys face-to-face, focusing on owners and top managers of small and medium government-dependent firms. For budget constraints and due to the size of Brazil, we chose to focus on a specific geographical area that is representative of our study sample. Specifically, we restricted our attention to municipalities around the city of Nova Lima, in the Brazil's southeastern state of Minas Gerais, meeting the CGU eligibility criteria for the anti-corruption audits. We further restricted the focus to firms with up to 30 employees that had sold goods or services to local governments in the previous year. We obtain this information from the list of government providers recently made available through the "transparency portals" of the selected municipalities. After applying these restrictions, we randomly sampled 175 firms, and were able to survey 115 of them, for a response rate of approximately 66%, which is extremely high for firm-level studies (Bloom and Van Reenen, 2010). The surveys were conducted by a local research manager, who disclosed the purely academic non-partisan goal of the research was to understand the role of corruption in public procurement and other government-firm relationships. Participation was voluntary and no incentives were provided. We summarize the main findings from these surveys in Fig. 4 and Table 10, and, for brevity, we only discuss some of the most interesting findings in the paper.

First and foremost, firms consider corruption to be a major cost of doing business, ranking behind only "taxes and regulations" as the primary barrier to both entry in a new market as well as firm growth and expansion (Fig. 4, Panels A and B). Looking at Panel A of Table 10, of the 115 firms, 112 state that corruption affects business operations, and two-thirds of them believe their growth rate would increase dramatically (by more than 10%) in a world without corruption. Digging deeper into the specific ways through which corruption impacts firm activity, we find that corruption seems to be a friction to investment and innovation (82%), to decisions regarding cash holdings and the allocation of financial resources within the firm (79%), to choices to expand to new markets and products (77%), and to bid for public procurement contracts (68%). These findings are consistent with our results on investment and access to finance, and more broadly with the presence of various distortions highlighted by the academic literature when thinking of corruption as a tax (Fisman and Svensson, 2007). We find weaker evidence about corruption as a friction to the internal organization of firms, with 50% of respondents saying that corruption affects hiring and firing activity and employee selection, and only 29% saying it affects organizational structure, delegation of power, and allocation of jobs and tasks. About half (54%) of the companies interviewed report monitoring corruption within the firm, even though only 24% of them have a structured system in place to do so.

Second, the uncertainty around corruption plays a rather important role, which is reflected in the reluctance or inability of more than half of the firms to respond to questions about corruption's prevalence and about the size of "unofficial payments" (i.e., bribes). Only 21% of firms say they know ex-ante how much they must pay in bribes to public officials, with the typical bribe being around 6% of



(A) Main Barrier to Entry (B) Main Barrier to Growth (C) Corruption Interactions (D) Corruption Situations

(E) Winning Factors (F) Perceived Corruption Risks (G) Market Information (H) Sector of Respondents

Fig. 4. Firm-Level Survey Responses.
Notes: This figure reports the shares of responses from our face-to-face firm-level survey. 115 firms from Brazil's southeastern state of Minas Gerais are sampled among the pool of those doing business with 15 municipalities that were eligible for the randomized anti-corruption program. Panel A asks: "What is the main barrier to entry in a market?." Panel B asks: "What is the main barrier to firm growth and expansion?." Panel C asks: "At what level does corruption most commonly take place in your sector?." Panel D asks: "In what situation does corruption most commonly take place in your sector?." Panel E asks: "In your view, what are the most important factors to win a government contract?." Panel F asks: "What type of costs would you be afraid of incurring, in the hypothetical case your firm were involved in a corruption irregularity?." Panel G asks: "What information do you rely on to find out the main issues related to accessing a new market?." Panel H reports the sector of the firms. All respondents are provided with a list of options to choose from.

the transaction value (even though only 15 firms decided to answer this latter question). Corruption is perceived as pervasive, with firms suspecting it affects approximately half of government contracts and half the firms in their sector. These statistics are reported at the top of Table 10, Panel B. These findings complement our evidence on increased investment rates after the audits: as suggested by the seminal work by Shleifer and Vishny (1993), the presence of corruption might hinder investment due to the uncertainty it entails for firm operations. A caveat with this interpretation is that it is difficult to pin down the specific reasons why firms are unwilling to respond to sensitive survey questions like ours.

A third finding is that firms report corruption to mostly involve politicians and other public officials, rather than other firms, and that public procurement is the primary area where corruption happens, although firms also highlight its pervasiveness throughout several other encounters with public officials, such as for procedures to obtain licenses, permits, and authorizations, and for tax administration purposes (Fig. 4, Panels C and D). We can interpret these findings as suggestive that the sizeable firm growth we observe following the audits might be driven by a more general reduction in interactions with the government, not just a move away from public procurement specifically.

Despite the issues raised by firms, doing business with the government is still considered a rather competitive market, with firm efficiency—rather than political connections and collusion—seen as the main determinant to obtain a government contract (Fig. 4, Panel E). Relatedly, as shown in Table 10, Panel B, 75% of firms report this market to be competitive, and a staggering 56% deem unofficial payments to public officials a necessary cost to compete. Such statistics are consistent with a world in which firms doing business with the government are not necessarily all inefficient politically connected firms, even when considering that several of them pay bribes, perhaps because that is the way "business is done" in this context. The firm-level data we collect from the audit reports, which highlight how most of the corruption cases involve wrongdoing initiated by the politicians and public officials, rather than the firm itself, seem consistent with these findings.

A related question we ask, central to our study, is: "In the hypothetical scenario in which you lose access to public procurement contracts, would you be able to maintain the same level of sales with only private sector contracts?" We find that 83% of firms indicate they would, which is both consistent with our empirical results, but that is also puzzling to the extent that a question remains for future work about why firms enter in possibly damaging business relationships with the government in the first place.

Finally, we see in Table 10 that almost all firms consider initiatives to punish corrupt officials necessary to improve the business environment, even though they believe the government has mostly been unsuccessful in this endeavor and lament difficulties in reporting corruption to higher levels of government when local officials commit irregularities.

4.4.4. Direct versus indirect effects of the audits

We conclude our discussion of mechanisms by outlining how our effects relate to other studies of the local effects of the CGU anti-corruption audits on Brazilian municipalities. Indeed, previous work has shown that audits affect political turnover (Ferraz and Finan, 2008) and local levels of firm activity, entrepreneurship, public procurement, sales, and investment (Bologna and Ross, 2015; Colonnelli and Prem, 2021), among other outcomes. All of these can

Table 10
Firm-Level Survey Responses.

Question	Share	Responses	Don't Know
Panel A: Corruption and Firm Strategy			
Does the presence of corruption affect your business operations or those of firms in your sector?	0.97	115	0
Does the presence of corruption affect investment and innovation?	0.82	115	0
Does the presence of corruption affect cash holdings and allocation of financial resources within the firm?	0.79	115	0
Does the presence of corruption affect decisions to expand to new markets and products?	0.77	115	0
Does the presence of corruption affect bidding strategy for public procurement contracts?	0.68	115	0
Does the presence of corruption affect contracts with private sector firms?	0.53	115	0
Does the presence of corruption affect hiring and firing activity and employee selection?	0.50	115	0
Does the presence of corruption affect organizational structure, delegation of power, and allocation of jobs and tasks?	0.29	115	0
In the absence of corruption, do you think your firm would be able to grow more than 10%?	0.65	113	2
Do you monitor corruption among your workers and within your business establishments?	0.54	115	0
Is there a structured system in place to monitor corruption?	0.24	115	0
Panel B: Corruption and Public Procurement			
Do firms in your industry know in advance the precise amount necessary for extra unofficial payments to public officials?	0.21	115	0
When firms in your industry do business with the government, what percent of the contract value would typically need to be paid in additional or unofficial payments/gifts, in order to secure the contract?	5.79	14	101
What do you think is the percentage of firms doing public procurement in your sector who directly witnessed or were affected by a case of corruption?	53.28	47	68
What do you think is the percentage of local public procurement contracts affected by corruption?	47.35	57	58
Would you be able to compete for public procurement contracts without making unofficial payments to public officials?	0.56	115	0
Is the market for public procurement contracts in your industry competitive?	0.75	115	0
In the hypothetical scenario in which you lose access to public procurement contracts, would you be able to maintain the same level of sales with only private sector contracts?	0.83	115	0
Do you consider anti-corruption initiatives aimed at punishing corrupt politicians and public officials to be important to improve the business environment?	0.96	115	0
Do you think the current anti-corruption initiatives by the Brazil's government are successful?	0.23	115	0
If a public official acts in an irregular manner (e.g. asking for a bribe), can firms in your industry successfully contact a superior official or office to receive a fair treatment (i.e. no bribe/unofficial payment)?	0.50	115	0

Notes: This table reports the shares of responses from the face-to-face firm-level survey. 115 firms from Brazil's southeastern state of Minas Gerais are sampled among the pool of those doing business with 15 municipalities that were eligible for the randomized anti-corruption program. When not otherwise specified, the column "Share" indicates the share of "Yes" to each question. The column "Responses" indicates the number of responses, while "Don't Know" represent the remaining number of firms who opt not to respond to that specific question.

be considered as *indirect* effects of the audits, in the sense that any firm-level outcome one observes for firms located inside the audited municipality is likely confounded by the increased transparency at the local level brought about by the audit. Yet, as discussed earlier in the paper, our main focus and primary contribution is to identify the *direct* effects of exposure in the anti-corruption program on firm-level outcomes. It is therefore important to show that the effects we uncover are not entirely driven by the local indirect effects of the audits.

There are two important reasons why we believe this is not the case. First, our identification strategy—by ensuring that both treated and control firms are not located in any municipality that was ever audited at any point in time—eliminates any direct overlapping between our estimates and those identified in previous work. Second, several of our findings are, if anything, opposite to those in other studies looking at local outcomes. A primary example relates to the difference between our findings and those in Colonnelli and Prem (2021), which is the closest to our paper. While we show that exposed firms on average grow in size and lose access to government contracts, the results in Colonnelli and Prem (2021) highlight a widespread increase in public procurement participation and an increase in economic activity that comes fully from firm entry, rather than firm growth. While there are findings that suggest some of the mechanisms at play are similar, such as the fact that a small set of politically connected firms suffer, the primary results of the two papers are fundamentally distinct.

To further alleviate these concerns, we provide additional empirical tests aimed at showing that the typical mechanisms discussed by previous work are unlikely to affect our findings. Specifically, we report two sets of analysis in the Appendix, which aim to explore the heterogeneity of our firm-level effects with respect to several local level features of the audited municipalities. Notice that exposed firms in our sample are located *outside* of audited municipalities. Thus, in the absence of any propagation of aggregate municipality-level effects along procurement links, we expect no heterogeneous effects depending on these municipality features. In Appendix Table A6, we report an analysis that shows that our effects are not systematically different for firms exposed by audits of municipalities with different levels of corruption uncovered by the audit itself, a margin all previous studies highlighted as a key driver of changes in the local economy and the local political system. We show these results are robust to several definitions of local corruption levels. Then, in Appendix Table A7, we conduct a similar group of tests, where we show that our effects are not significantly heterogeneous across municipalities where: (Panel A) the audits happen late in the electoral term, (Panel B) the mayor is in its second and final political mandate, (Panel C) the information is spread locally because of the presence of a local radio, and (Panel D) the information is spread locally because of the presence of a local newspaper. All of these margins are those that the previous political economy literature, and in particular the seminal work by Ferraz and Finan (2008) and the more recent work by Avis et al. (2018), argue to be important in explaining the effects of increased transparency on the audited municipality.

All together, our findings indicate that the firm-level effects we uncover are, at a minimum, not fully explained by the indirect effects of the audits identified by previous work on the CGU anti-corruption program. Combined with our earlier tests of mechanisms and the heterogeneous effects across firm types, our evidence points to strong *direct* effects of the audits on firm-level growth patterns, thus providing a more complete picture of the impacts the audits may have on firms and not just on the local economy.

5. Concluding remarks

Corruption practices in the assignment of procurement contracts have been documented in many countries, and especially in developing economies. The existence of such practices can have important implications for firms, as it can distort the allocation of production factors or shape firms' investment policies. Understanding how anti-corruption efforts affect firms is therefore key for our understanding of the drivers of firm growth in emerging markets.

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 to corrupt practices using randomized anti-corruption audits. We document that firms exposed by the audits lose access to procurement contracts but also grow faster in the years after exposure. We argue that, by cutting access to government contracts for exposed firms, anti-corruption campaigns might force such firms to adjust their investment and business practices in order to compete in the market for private demand. We find evidence consistent with this mechanism using detailed micro data on firms' investment and access to credit. On the other hand, we do not observe major changes in the internal organization of firms after exposure. We complement the quantitative evidence with a new survey of business owners, which provides qualitative support to our findings that anti-corruption programs affect firm growth as well as firm strategy. Finally, we show that the firm-level effects we uncover are unlikely to be driven by the aggregate local consequences of the audits documented by previous work (Ferraz and Finan, 2008; Avis et al., 2018; Colonnelli and Prem, 2021).

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 corrupt environment might affect firm behavior. Our focus only speaks to the extent to which an anti-corruption program impacts some of these margins, thus leaving a number of open questions more directly linking corruption and firm decisions. Additional surveys and experimental designs might help further unpack these and other mechanisms, due to the difficulties to test them using administrative data only. Importantly, future studies of anti-corruption initiatives should also further unpack the difference between firms that are revealed to be corrupt and all corrupt firms more broadly, which we cannot observe in our setting. 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.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jfineco.2021.12.013.

References

Akey, P., 2015. Valuing changes in political networks: evidence from campaign contributions to close congressional elections. Rev. Financ. Stud. 28 (11), 3188–3223.

Akey, P., Lewellen, S., 2017. Policy Uncertainty, Political Capital, and Firm Risk-Taking. (March 22)

Avis, E., Ferraz, C., Finan, F., 2018. Do government audits reduce corruption? Estimating the impacts of exposing corrupt politicians. J. Polit. Economy 126 (5), 1912–1964.

Baker, A., Larcker, D.F., Wang, C.C., 2021. How Much Should We Trust Staggered Difference-in-Differences Estimates? Available at SSRN 3794018.

Bardhan, P., 1997. Corruption and development: a review of issues. J. Econ. Lit. 35 (3), 1320–1346.

Bertrand, M., Bombardini, M., Fisman, R., Trebbi, F., Yegen, E., 2020. Investing in influence: investors, portfolio firms, and political giving

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? Q. J. Econ. 119 (1), 249–275.

Bloom, N., Eifert, B., Mahajan, A., McKenzie, D., Roberts, J., 2013. Does management matter? Evidence from india. Q. J. Econ. 128 (1), 1–51.

Bloom, N., Van Reenen, J., 2007. Measuring and explaining management practices across firms and countries. Q. J. Econ. 122 (4), 1351–1408.

Bloom, N., Van Reenen, J., 2010. New approaches to surveying organizations. Am. Econ. Rev. 100 (2), 105–109.

Bologna, J., Ross, A., 2015. Corruption and entrepreneurship: evidence from brazilian municipalities. Public Choice 165 (1–2), 59–77.

Brogaard, J., Denes, M., Duchin, R., 2019. Political Influence and the Renegotiation of Government Contracts. Available at SSRN 2604805.

Brollo, F., Nannicini, T., Perotti, R., Tabellini, G., 2013. The political resource curse. Am. Econ. Rev. 103 (5), 1759–1796. doi:10.1257/aer.103.5.1759.

Cai, J., Szeidl, A., 2018. Interfirm relationships and business performance. Q. J. Econ. 133 (3), 1229–1282.

Carrillo, P., Donaldson, D., Pomeranz, D., Singhal, M., 2018. The Bigger the Better? Using Lotteries to Identify the Allocative Efficiency Effects of Firm Size. Technical Report, Working Paper.

Cengiz, D., Dube, A., Lindner, A., Zipperer, B., 2019. The effect of minimum wages on low-wage jobs. Q. J. Econ. 134 (3), 1405–1454.

Cheung, Y.L., Rau, P.R., Stouraitis, A., 2012. How much do firms pay as bribes and what benefits do they get? Evidence from corruption cases worldwide. Technical Report. National Bureau of Economic Research.

Cheung, Y.-L., Rau, P.R., Stouraitis, A., 2020. What determines the return to bribery? Evidence from corruption cases worldwide. Manage. Sci..
Christensen, H.B., Maffett, M.G., Rauter, T., 2020. Policeman for the World: The Impact of Extraterritorial FCPA Enforcement on Foreign Investment and Internal Controls. Available at SSRN 3349272.
Christensen, H.B., Maffett, M.G., Rauter, T., 2020. Reversing the Resource Curse: Foreign Corruption Regulation and Economic Development. Available at SSRN 3712693.
Cingano, F., Pinotti, P., 2013. Politicians at work: the private returns and social costs of political connections. J. Eur. Econ. Assoc. 11 (2), 433–465.
Claessens, S., Feijen, E., Laeven, L., 2008. Political connections and preferential access to finance: the role of campaign contributions. J. Financ. Econ. 88 (3), 554–580.
Cohen, L., Coval, J., Malloy, C., 2011. Do powerful politicians cause corporate downsizing? J. Polit. Economy 119 (6), 1015–1060.
Cohen, L., Malloy, C.J., 2016. Mini West Virginias: Corporations as Government Dependents. Working Paper.
Colby, S., 2012. Explaining the BNDES: what it is, what it does and how it works. CEBRI Artigos 8 (3), 3–31.
Cole, S., Tran, A., 2011. 14 Evidence from the firm: a new approach to understanding corruption. Int. Handb. Econ.Corruption 408 (7).
Colonnelli, E., Gallego, J.A., Prem, M., 2020. What Predicts Corruption? Available at SSRN 3330651.
Colonnelli, E., Pinho Neto, V., Teso, E., 2020. Politics at Work. Available at SSRN 3715617.
Colonnelli, E., Prem, M., 2021. Corruption and firms. Rev. Econ. Stud..
Colonnelli, E., Prem, M., Teso, E., 2020. Patronage and selection in public sector organizations. Am. Econ. Rev. 110 (10), 3071–3099.
Cooper, M.J., Gulen, H., Ovtchinnikov, A.V., 2010. Corporate political contributions and stock returns. J. Finance 65 (2), 687–724.
Dal Bó, E., Rossi, M.A., 2007. Corruption and inefficiency: theory and evidence from electric utilities. J. Public Econ. 91 (5), 939–962.
De Chaisemartin, C., d'Haultfoeuille, X., 2020. Two-way fixed effects estimators with heterogeneous treatment effects. Am. Econ. Rev. 110 (9) 2964-29.
Decarolis, F., Fisman, R., Pinotti, P., Vannutelli, S., 2020. Rules, Discretion, and Corruption in Procurement: Evidence from Italian Government Contracting. Working Paper 28209. National Bureau of Economic Research doi:10.3386/w28209. http://www.nber.org/papers/w28209
Dix-Carneiro, R., 2014. Trade liberalization and labor market dynamics. Econometrica 82 (3), 825–885. doi:10.3982/ECTA10457. https://onlinelibrary.wiley.com/doi/abs/10.3982/ECTA10457
Duchin, R., Sosyura, D., 2012. The politics of government investment. J. Financ. Econ. 106 (1), 24–48.
Dyck, I.A., Morse, A., Zingales, L., 2021. How pervasive is corporate fraud? Rev. Account. Stud.. Forthcoming
Egan, M., Matvos, G., Seru, A., 2019. The market for financial adviser misconduct. J. Polit. Economy 127 (1), 233–295.
Faccio, M., 2006. Politically connected firms. Am. Econ. Rev. 96 (1), 369–386.
Faccio, M., Masulis, R.W., McConnell, J.J., 2006. Political connections and corporate bailouts. J. Finance 61 (6), 2597–2635.
Fama, E., Jensen, M., 1983. Separation of ownership and control. J. Law Econ. 26 (2), 301–325.
Fama, E.F., 1980. Agency problems and the theory of the firm. J. Polit. Economy 88 (2), 288–307.
Ferraz, C., Finan, F., 2008. Exposing corrupt politicians: the effects of Brazil's publicly released audits on electoral outcomes. Q. J. Econ. 123 (2), 703–745.
Ferraz, C., Finan, F., Szerman, D., 2015. Procuring Firm Growth: The Effects of Government Purchases on Firm Dynamics. Technical Report. National Bureau of Economic Research.
Fich, E., Shivdasani, A., 2007. Financial fraud, director reputation, and shareholder wealth. J. Financ. Econ. 86 (2), 306–336.
Fisman, R., 2001. Estimating the value of political connections. Am. Econ. Rev. 91 (4), 1095–1102.
Fisman, R., Svensson, J., 2007. Are corruption and taxation really harmful to growth? Firm level evidence. J. Dev. Econ. 83 (1), 63–75.
Fisman, R., Wang, Y., 2015. The mortality cost of political connections. Rev. Econ. Stud. 82 (4), 1346–1382.
Fos, V., Tsoutsoura, M., 2014. Shareholder democracy in play: career consequences of proxy contests. J. Financ. Econ. 114 (2), 316–340. doi:10.1016/j.jfineco.2014.07.009.
Giannetti, M., Liao, G., You, J., Yu, X., 2021. The externalities of corruption: evidence from entrepreneurial firms in china. Rev. Financ. 25 (3), 629–667.
Goldman, E., Rocholl, J., So, J., 2009. Do politically connected boards affect firm value? Rev. Financ. Stud. 22 (6), 2331–2360.
Goldman, E., Rocholl, J., So, J., 2013. Politically connected boards of directors and the allocation of procurement contracts. Rev. Financ. rfs039.
Goldman, J., Zeume, S., 2020. Who Benefits from Anti-Corruption Enforcement? Available at SSRN 3745751.
Gompers, P., Kaplan, S.N., Mukharlyamov, V., 2016. What do private equity firms say they do? J. Financ. Econ. 121 (3), 449–476.
Gompers, P.A., Gornall, W., Kaplan, S.N., Strebulaev, I.A., 2020. How do venture capitalists make decisions? J. Financ. Econ. 135 (1), 169–190.
González, F., Prem, M., 2020. Losing your dictator: firms during political transition. J. Econ. Growth 25 (2), 227–257.
Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J. Econom..
Graham, J.R., Harvey, C.R., 2001. The theory and practice of corporate finance: evidence from the field. J. Financ. Econ. 60 (2–3), 187–243.
Griffin, J., Liu, C., Shu, T., 2016. Is the Chinese Anti-Corruption Campaign Effective. University of Texas, Austin.
Hanna, R., Bishop, S., Nadel, S., Scheffler, G., Durlacher, K., 2011. The Effectiveness of Anti-Corruption Policy: What has Worked, What Hasn't, and What We Don't know - A Systematic Review. London, 978-1-907345-14-2.
Helland, E., 2006. Reputational penalties and the merits of class-action securities litigation. J. Law Econ. 49 (2), 365–395. doi:10.1086/505371.
Iacus, S.M., King, G., Porro, G., 2012. Causal inference without balance checking: coarsened exact matching. Polit. Anal. 1–24.
Jäger, S., 2019. How Substitutable Are Workers? Evidence from Worker Deaths. Working Paper.
Jaravel, X., Petkova, N., Bell, A., 2018. Team-specific capital and innovation. Am. Econ. Rev. 108 (4–5), 1034–1073.
Karpoff, J., Lee, D., Martin, G., 2017. Foreign Bribery: Incentives and Enforcement. Technical Report. University of Washington working paper.
Karpoff, J.M., Lee, D.S., Martin, G.S., 2008. The consequences to managers for cooking the books. J. Financ. Econ. 88 (88), 193–215.
Karpoff, J.M., Lee, D.S., Martin, G.S., 2014. The consequences to managers for financial misrepresentation. In: Accounting and Regulation. Springer, pp. 339–375.
Karpoff, J.M., Lee, D.S., Martin, G.S., 2017. Foreign Bribery: Incentives and Enforcement. Available at SSRN 1573222.
Khwaja, A.I., Mian, A., 2005. Do lenders favor politically connected firms? Rent provision in an emerging financial market. Q. J. Econ. 120 (4), 1371–1411.
Liu, T., Liu, Y., Ullah, B., Wei, Z., Xu, L.C., 2021. The dark side of transparency in developing countries: the link between financial reporting practices and corruption. J. Corp. Finance 66, 101829. doi:10.1016/j.jcorpfin.2020.101829.
O'Donovan, J., Wagner, H.F., Zeume, S., 2019. The value of offshore secrets: evidence from the panama papers. Rev. Financ. Stud. 32 (11), 4117–4155.
Olken, B.A., Pande, R., 2012. Corruption in developing countries. Annu. Rev. Econom. 4 (1), 479–509. doi:10.1146/annurev-economics-080511-110917.
Ponticelli, J., Alencar, L.S., 2016. Court enforcement, bank loans, and firm investment: evidence from a bankruptcy reform in Brazil. Q. J. Econ. 131 (3), 1365–1413.
Schoenherr, D., 2019. Political connections and allocative distortions. J. Finance 74 (2), 543–586.
Shleifer, A., Vishny, R.W., 1993. Corruption. Q. J. Econ. 108 (3), 599–617.
Smith, J.D., 2016. Us political corruption and firm financial policies. J. Financ. Econ. 121 (2), 350–367.
Srinivasan, S., 2005. Consequences of financial reporting failure for outside directors: evidence from accounting restatements and audit committee members. J. Account. Res. 43 (2), 291–334. doi:10.1111/j.1475-679x.2005.00172.x.
Svensson, J., 2003. Who must pay bribes and how much? Evidence from a cross section of firms. Q. J. Econ. 118 (1), 207–230.
Svensson, J., 2005. Eight questions about corruption. J. Econ. Perspect. 19 (3), 19–42.
Szerman, C., 2020. The Employee Costs of Corporate Debarment. Working Paper.
Torres, E., Zeidan, R., 2016. The life-cycle of national development banks: the experience of Brazil's BNDES. Q. Rev. Econ. Finance 62, 97–104.
Wang, T.Y., Winton, A., Yu, X., 2010. Corporate fraud and business conditions: evidence from IPOs. J. Finance 65 (6), 2255–2292. doi:10.1111/j.1540-6261.2010.01615.x.
Zamboni, Y., Litschig, S., 2018. Audit risk and rent extraction: evidence from a randomized evaluation in Brazil. J. Dev. Econ. 134, 133–149.
Zeume, S., 2017. Bribes and firm value. Rev. Financ. Stud. 30 (5), 1457–1489.