CORRUPTION AND FIRMS: EVIDENCE FROM RANDOMIZED AUDITS IN BRAZIL

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ABSTRACT. We exploit spatial variation in randomized anti-corruption audits related to government procurement contracts in Brazil to assess how corruption affects resource allocation, firm performance, and the local economy. After an anti-corruption crackdown, regions experience more entrepreneurship, improved access to finance, and higher levels of economic activity. This is inconsistent with corruption acting as "grease in the wheel." We find that two channels explain these facts: allocation of resources to less efficient firms, and distortions in government dependent firms. Using firms involved in corrupt business with the municipality, i.e. "corrupt firms," we find that the second channel is more important. Difference in difference estimation suggests that, after audits, the performance of corrupt firms improves relative to a similar set of unaffected firms. Corrupt firms invest more, increase borrowing and leverage, reallocate labor inside the firm, restructure the organizational design by increasing hierarchical layers, rely less on government contracts, and grow faster. Our findings provide novel micro-evidence on why corruption acts as an institutional failure that is detrimental to firm performance and economic growth.

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1. Introduction

A vast literature at the intersection of economics, finance, and law has debated the importance of institutions for economic growth and allocative efficiency (Acemoglu et al., 2000, Glaeser et al., 2004). Several studies have emphasized the role of financial development and institutions (Rajan and Zingales, 1996, Shleifer and Vishny, 1997, La Porta et al., 2002), courts (Djankov et al., 2003, La Porta et al., 2004), bankruptcy (Maksimovic and Phillips, 1998, Ponticelli and Alencar, 2016, Bernstein et al., 2017), as well as social capital, culture, and religion (Guiso et al., 2004, Manz et al., 2006, Campante and Yanagizawa-Drott, 2015). While the importance of these institutions is not controversial, what remains less clear are the mechanisms through which they impact economic activity. In this paper we focus on corruption as an institution. Its importance for economic growth has been of policy interest to governments, firms, entrepreneurs, and investors around the world, with the IMF estimating that corruption costs exceed 2% of world's GDP (IMF, 2016). We exploit spatial variation in randomized anti-corruption audits related to government procurement contracts and micro data to assess how corruption affects resource allocation, firm performance, and local economic activity.

IMF estimates notwithstanding, the impact of corruption on economic activity remains an area of active theoretical and empirical debate.[1] On the one hand, it is argued that corruption acts as a "sand in the wheel" and hampers economic growth, through channels such as barriers to entrepreneurship and firm investment, limited access to finance, and higher transaction costs (Shleifer and Vishny, 1993, Svensson, 2005), resulting in resource misallocation across firms (Hsieh and Klenow, 2009) and within firms (Murphy et al., 1991, Dal Bó and Rossi, 2007). On the other hand, it is argued that in presence of red-tape, corruption may act as a "grease in the wheel" of bureaucracy, reducing costs of doing business and improving performance (Leff, 1964, Huntington, 2006, Dreher and Gassebner, 2013), and potentially leading to efficient allocation as the most productive firms may have the highest willingness to pay (Lui, 1985, Shleifer and Vishny, 1994, Weaver, 2016). In this paper we take a step at resolving this argument by empirically establishing that corruption impacts economic activity, and teasing out the channels that drive this relationship.

There are two main obstacles that researchers face when empirically investigating this question. First, corruption by its inherent nature is difficult to measure. Consequently, detailed data on firm-level corruption is often unavailable and researchers have mostly relied on survey and voluntary disclosure reports which could be biased. Second, and importantly, corruption and firm and economic activity are jointly determined, making it difficult to causally identify the effects of corruption. We rely on randomized anti-corruption audits in Brazil to circumvent these challenges.[2] Two features of this setting allow us to directly

[1] See Bardhan (1997), Jain (2001), and Olken and Pande (2011) for extensive reviews of the various theories of corruption and firms.

[2] Investigative audits are one of the most common tools governments adopt to monitor and punish corrupt activities. Supreme Audit Institutions (SAIs) –specific agencies dedicated to anti-corruption audits– are

address the issues of measurement and endogeneity. One, we have access to detailed audit reports containing hard evidence on corruption cases in government procurement. And two, since these audits are randomized across municipalities, and have been shown to have long lasting effects in reducing corruption (Avis et al., 2016), they provide us with an exogenous variation in the level of corruption. Exploiting this variation, we argue we can trace the causal impact of corruption on economic activity, and the channels that drive this link.

We start constructing our data from audit reports that are available in text format. We extract detailed information on firms involved in corruption from these reports, and create firm-level measures of corruption in public procurement. Using tax identifiers, we then combine the corruption data to administrative matched employer-employee data for the entire Brazilian formal sector. We augment our data with confidential and publicly available datasets such as the manufacturing Census, the universe of federal public procurement contracts and government loans, and public procurement suspensions. Our corruption data has around 12,800 firms involved in corruption, observed over 15 years (2000-2014). Over this horizon, we follow these firms as well as another 2.3 million firms that are located in roughly 5,500 municipalities that were eligible for the anti-corruption audit program. In addition, we validate our findings from this sample by conducting a representative face-to-face survey of 115 firms doing business with local governments, sampled from 15 eligible municipalities.

We exploit the randomized nature of the audit program to estimate the causal impact of the anti-corruption crackdown on municipality and firm outcomes. Our empirical strategy first focuses on municipality outcomes. Doing so allows us to test for the different arguments linking corruption to economic activity. In particular, if "sand in the wheel" arguments are at play, we would expect lower corruption to increase economic activity and entrepreneurial opportunities. In contrast, "grease in the wheel" arguments would predict lower corruption to reduce economic activity and entrepreneurial opportunities.

At the municipality level, we compare economic outcomes in a municipality that was randomly drawn to be audited (treatment) to those in municipalities that were eligible, but were either randomly chosen to be audited later or were never audited (control). We confirm the validity of our research design by illustrating parallel trends in a plethora of observables in the years prior to the audit. After an anti-corruption crackdown, relative to control municipalities, treatment ones experience higher levels of economic activity, more entrepreneurship, and improved access to finance. In particular, we find employment in the treatment group is higher by 2.5% five years after the audit. This change is largely driven by an increase in local entrepreneurship. After the audits, there is an increase in entry of new firms by 1.8%. Additionally, audits improve access to finance, as seen in the higher volume of lending from private banks to local firms in the region (3.8%). We sharpen these tests

present in almost every country in the world. See the International Organization of Supreme Audit Institutions (INTOSAI) website for a list of 194 countries with specific agencies designed to perform audit and anti-corruption activities: http://www.intosai.org/about-us/organisation/membership-list.html.

and show that these effects are concentrated in areas that have a higher proportion of firms involved in corruption and in sectors that are highly dependent on government relationships.

Overall, our findings show that the anti-corruption crackdown has a positive net effect on the local economy – i.e., reducing corruption increases economic activity. This is inconsistent with corruption acting as "grease in the wheel" of bureaucracy and enhancing economic activity and entrepreneurial opportunities. Notably, by conducting our analysis in Brazil – an economy that is ranked among the highest on government regulation (143/144 among countries surveyed in the Global Competitiveness Index (World Economic Forum, 2015))– we tested "grease in the wheel" theories in the most generous setting. Our findings suggest that corruption limits the ability of firms to raise capital and acts as a barrier to entry for new firms.

The randomization of the audits alleviates most identification concerns. One worry is that the deterrence effect of the audit varies across municipalities over time. To deal with this, we test the robustness of our strategy to an alternative specification based on a dynamic difference-in-difference model that compares audited municipalities to a contemporaneous control group of never audited municipalities that are subject to the same audit probability. We find that results go through, and the magnitudes are slightly larger. Another worry is that there are direct spillover effects of the audits on other municipalities. Our empirical design should give conservative estimates in this case. However, we also address this concern using the alternative difference-in-difference specification, which allows us to select a control group of municipalities that are far away from any audited municipality.

Next, we use firm-level analysis to investigate two channels that may explain why corruption hampers local economic activity. First, corruption could impact economic activity because it misallocates resources across firms by favoring inefficient ones. In addition, corruption could generate distortions and costs inside firms that are dependent on government. We test for the presence of these channels by using a dynamic difference in difference specification that compares firms that are directly involved in irregular government procurement ("corrupt firms") to a control group of firms operating in the same sector, with similar characteristics, and located in eligible municipalities that were never audited. Under the first channel, anti-corruption audits negatively affect the relative performance of corrupt firms. This is because lower corruption should be associated with a reallocation of resources away from them and towards more efficient firms. In contrast, according to the second channel, anti-corruption audits positively affect the relative performance of corrupt firms, since lower corruption should imply lower costs to these firms.

Our analysis reveals presence of both of these effects, though the second channel is dominant. More specifically, while we find that corruption is related to contract misallocation, its role is secondary compared to the direct costs of corruption to "corrupt firms." In particular, relative to control group, corrupt firms improve performance after the audit. Corrupt firms

experience differentially higher employment (11%), sales (13%), and investment (38%) over the 5-year horizon.

Next, we drill down further to better understand these findings. Several results help us understand the patterns of corrupt firms. First, following audits corrupt firms obtain more loans for long-term financing (4.7%), after accounting for firm employment growth. That is, lower corruption leads firms to increase borrowing and leverage. This is consistent with the aggregate findings on bank lending, thus confirming that corruption limits firms' access to finance. Second, after audits, corrupt firms adopt a potentially more efficient internal organization of labor, as is evidenced by the churn of employees within the firm and the increase in average ability of the workforce. We provide additional supporting evidence by showing that anti-corruption audits lead firms to increase the number of hierarchical layers within the organization. These findings suggest that corruption in local environment may affect a firm's choice of organizational design. This is a new motive that connects to the literature on boundaries of the firm (Holmström and Roberts, 1998, Rajan and Wulf, 2006, Roberts, 2007, Seru, 2014). Finally, corrupt firms rely less on government procurement contracts that are allocated through discretionary procedures, suggesting that corruption limits the ability of firms to expand to new markets or products. These results are aligned with several firm-level theories of corruption, and suggest that corruption distorts resource allocation within the firm. Overall, we find corrupt firms invest more, increase borrowing and leverage, reallocate capital and labor within the organization, rely less on government contracts, and grow faster.

We conduct several additional tests to demonstrate that our findings are robust to alternative interpretations. One worry with our findings might be that the effects we find might not capture corruption frictions specific to firms doing business with the government. Instead, the findings might be driven by positive aggregate effects of the audits. To investigate this possibility, we assess other sets of local incumbent firms, and show that only government dependent firms are positively affected by the audits. In particular, we find that firms in procurement-intensive sectors and firms doing business with the federal government improve performance after the audits relative to other incumbents. Alternatively, we might worry that control firms are also directly affected by the audit, since they may do business with the audited municipality, even if located elsewhere. However, we show that the results are robust to an alternative control group that consists only of firms located in areas that are far away from any audited municipality. Another worry is that "corrupt firms" may have on-going contracts with the audited municipality, which may induce mechanical differential growth in the years after the audit. We manually collect information on the date of contract completion, and find the results also hold when focusing on firms with no on-going audited contracts. A related concern is that corrupt firms grow after the audits because of other effects related to the experience accumulated thanks to the government contract, consistent with Ferraz et al. (2015). Hence, we show that results are robust to focusing only on corrupt and control firms

that recently won a federal government contract, to account for such unobservable differences. Finally, we provide an additional validation to our empirical strategy by showing that the main results hold when comparing corrupt firms to a synthetic control group constructed using only information up to two years before the audit (Abadie and Gardeazabal, 2003).

Overall, our results uniformly support the "sand in the wheel" view and suggest costs and distortions to government dependent firms as a main channel through which corruption inhibits the performance of firms and economic activity. These findings provide novel micro-evidence on why corruption acts as an institutional failure that is detrimental to firm performance and economic growth. An immediate implication is that anti-corruption efforts can lead to higher levels of entrepreneurship and access to credit, even in the most bureaucratic economies where corruption could also act as a "grease in the wheel." Our findings have important policy implications for both governments and firms in emerging markets.

We relate to three main strands of literature. First, we contribute to the vast literature on corruption (Shleifer and Vishny, 1993, Shleifer and Vishny, 1994, Svensson, 2005, Olken and Pande, 2011). In particular, we relate to studies that focus on the role of corruption for firms and economic growth (Mauro, 1995, Kaufmann and Wei, 1999, Svensson, 2003, Fisman and Svensson, 2007, Olken and Barron, 2009, Bai et al., 2013, Sequeira and Djankov, 2014, Smith, 2016), and those studying the effectiveness of government audits in reducing corruption (Olken, 2005, Ferraz and Finan, 2008, Bobonis et al., 2013, Zamboni et al., 2013, Avis et al., 2016). Compared to previous studies, we exploit exogenous variation in corruption activity to better investigate the causal effect of corruption on local economic growth as well as mechanisms driving this link. Additionally, by using the anti-corruption audits for our identification, our study also contributes to the more recent literature that investigates the effects of anti-corruption institutions on firms (Bologna et al., 2015, Zeume, 2016, Giannetti et al., 2016, Lagaras et al., 2017).

Within this strand, our paper is closely related to two studies that also investigate the relationship between the Brazilian audit program and firms. Bologna et al. (2015) study the 2003 audits and compare aggregate economic activity in municipalities with different levels of corruption. In a contemporaneous paper, Lagaras et al. (2017) collect data similar to ours on the involvement of firms in corruption cases, and find that corrupt firms grow less in the 12 months after the audit. Our paper differs from Lagaras et al. (2017) as we focus on a long time horizon (5 years) and rely on a different counterfactual to isolate the effects. Additionally, and importantly for our purpose, we also explore the aggregate effects of audits on the local economy. Combining both aggregate and micro-level evidence allows us not only to assess the impact of corruption on various outcomes, but also to draw conclusions on the mechanisms through which corruption affects the economy.

Second, we contribute to studies that assess whether and how political connections are related to firm value (Fisman, 2001, Khwaja and Mian, 2005, Faccio, 2006, Faccio et al., 2006, Claessens et al., 2008, Goldman et al., 2008, Cooper et al., 2010, Duchin and Sosyura,

2012, Cingano and Pinotti, 2013, Schoenherr, 2015). A large part of this literature argues that politically connected firms benefit from such connections, due to favoritism. Our work adds an additional wrinkle to this argument. In particular, we show that corrupt firms benefit from lower corruption levels, suggesting that while there may be favoritism, such relationships also impose higher transaction costs and distortions on connected firms.

Third, we relate to the literature examining the causes and consequences of resource misallocation in the economy (Restuccia and Rogerson, 2008, Hsieh and Klenow, 2009, Bartelsman et al., 2009, Syverson, 2010). Our paper contributes to this work by highlighting and quantifying the role of one specific friction: corruption in public procurement.

The paper is organized as follows. In the next section, we describe the Brazilian anti-corruption program. Section 3 illustrates the data sources and a descriptive analysis. Section 4 presents the identification strategy. In Section 5 we study the impact of the anti-corruption program on the local economy, while Section 6 presents the firm-level analysis. Section 7 reports auxiliary tests and discussion. Section 8 concludes. More details on the theory, data, alternative strategies, and additional results are presented in the Appendix.

2. The Brazilian Anti-corruption Program

In May 2003, under the administration of Luis Inázio Lula da Silva, the Brazilian central government launched a large anti-corruption program aimed at fighting the rampant corruption and waste of public resources in local governments. The program consists of 39 rounds of randomized audits of municipalities' expenditures –with replacement– over the period 2003-2014, followed by anti-corruption enforcement activity such as the suspension of corrupt public officials and politicians.

The audits are conducted by the Office of the Comptroller General (Controladoria Geral da União - CGU), which is the federal agency responsible of ensuring transparency in the use of public funds. It is considered to be the main anti-corruption body in Brazil.[3] At each round, approximately 60 municipalities are randomly selected, together with a popular national lottery. The lotteries are with replacement.[4] Only municipalities below a certain population threshold are eligible for the program, and state capitals are excluded.[5] As of 2014, more than 99% of Brazil's 5,570 municipalities are eligible, and almost 2,000 have been selected at least once. The sampling procedure is stratified by state, and the implied audit probability in any given round is very low (1% within a round, and 3% within a year), as reported in Table A1.

The audit process begins immediately after the random draw, with the federal CGU office describing the details of the audit to the various CGU state offices, by means of a number

[3]In 2016, the CGU received the status of Ministry, and is now called "Ministry of Transparency, Supervision and Control." The power and responsibilities remain mostly unchanged.

[4]Hence, municipalities can be randomly selected more than once. The exception is that municipalities cannot be selected if they were selected in one of the previous three rounds.

[5]The population threshold was originally 100,000, but it was successively increased to 300,000 soon after the launch, and to 500,000 for most of the program.

of "inspection orders." The state offices are then in charge of sending a team of auditors to the selected municipalities within two weeks from the lottery date.[6] The vast majority of contracts related to federal transfers from the central government to the municipality are audited, mostly covering the previous three years.[7] The most common examples of audited public procurement contracts are retail (e.g. a firm selling stationery for a school program) and construction (e.g. construction of a hospital), as discussed later in more details.

The audit consists of an intense few weeks of field work (depending on the size of the municipality), during which auditors analyze all the documents and receipts related to each specific contract, interview local people, bureaucrats, and other relevant parties, solicit direct anonymous complaints about malfeasance, and take pictures to report the quality of the work performed. After this process, the auditors follow a detailed CGU instructions manual to write a report of the audit. These reports can be up to 300 pages long, and consist of an organized and schematic analysis of all the information gathered. Itemizing the list of irregularities found, the report aims at showing the amount of resources that were diverted, wasted or stolen, and the major reasons and parties responsible for it. Once the report is completed and cross-checked for validity, it is made public on the CGU online repository, and it is directly sent to a number of stakeholders.

The audits may have severe legal consequences, which mostly target politicians and bureaucrats, and not firms. In particular, CGU discuss the audit findings directly with the state and federal prosecutors, and with the Ministries whose transfers have displayed irregularities. These agencies then analyze the irregularities and decide on follow-up anti-corruption activity. Politicians- and bureaucrats- specific punishments, such as the temporary or lifelong suspension from the public post, and the loss of the mandate or impeachment of the mayor by the local council, are the most common consequences of the CGU audits. Additionally, audits uncovering large cases of corruption can lead to special police crackdowns.[8] Prosecutions and suspensions of firms are extremely rare.[9]

[6]On average 10 to 15 auditors and a supervisor are sent to the municipality, but this number varies according to its area and population, other municipal characteristics, and the extent of the inspection orders. The number and scope of inspection orders primarily depends on the amount of federal resources going to the municipality, and the number of public projects running. To reduce the likelihood of the auditors engaging in corruption, they are selected based on competitive public examinations, receive an attractive compensation, and are subject to extensive regular training.

[7]All federal transfers tend to be audited for smaller municipalities, while often just a subset of them can be audited for larger ones. The details of each inspection order and the sectors that are audited can change over time, under the discretion of the CGU central office. Most audited transfers are linked to national health and education policies. A typical example are transfers related to Bolsa Família, one of the largest national social welfare programs in the world.

[8]CGU estimates that more than $3,000$ public officials have been terminated following the findings of the audits ("Internal Control, Preventing, and Fighting Corruption", CGU 2014).

[9]The context changed in 2014, with the passing of the *Clean Company Act*. This very comprehensive law, among many other things, increased the likelihood of punishment and the subsequent legal costs for firms directly involved in severe corruption cases.

2.1. **Audits Reduce Corruption.** In the same context, Avis et al. (2016) show that audits reduce future levels of corruption, mainly through the actual and perceived follow-up legal punishment of politicians.[10] This is a crucial finding, as it allows us to rely on the randomization of the audits as a source of exogenous variation in the level of corruption, and subsequently back-out the causal impact of corruption on the private sector.

We conduct further tests that corroborate this finding, which we discuss more formally in the Appendix. First, in Table A2, we replicate their analysis and confirm their finding using as dependent variables only irregularities in public procurement, which is our focus because of the direct link with firms.[11] We also show the reduction in corruption is concentrated in highly corrupt areas. Second, we show that a direct effect of the audits is that they trigger a restructuring in the public sector, leading to the termination and replacement of several public officials (Table A3), with effects again more pronounced in highly corrupt areas. This is consistent with anecdotal evidence and the discussions we had with the CGU officials, who consider the punishment of corrupt bureaucrats to be one of the primary consequences of the audit program.[12]

3. Data and Descriptive Analysis

The main dataset used in the analysis is constructed from the combination of the CGU anti-corruption reports and the administrative matched employer-employee data on the Brazilian formal sector. We then complement the analysis with a variety of other data sources. The datasets are described below, and further details can be found in Appendix A.3.

3.1. **Information Content of the Anti-corruption Audit Reports.** Starting from the digital copies of the CGU audit reports, we manually construct a unique dataset on all public procurement irregularities for the period 2003-2014. We cover all 39 audit rounds and 1,881 different municipalities randomly selected to be audited.[13]

We focus on cases of irregular public procurement, namely instances of corruption that involve firms. The most common cases involve procedural irregularities, where procurement regulations are violated or documents are missing. More severe examples include the extortion of bribes by local procurement officials, or kickbacks to politicians. Other common categories are those related to cases of fraud, such as ex-post forging of procurement contracts, and

[10]Given that corruption is measured only conditional on being audited, this analysis compares corruption levels in municipalities audited for a second time (or more), to those of a municipality audited for the first time. Corruption is measured by the number of severe irregularities listed in the audit reports.

[11]The model is the following:

$$Y_{mrs} = \alpha_r + \alpha_s + \beta \times PastAudit_m + \delta \times X_m + \epsilon_{mrs} \tag{2.1}$$

where m is municipality, r is round, and s is state. $PastAudit_m$ is an indicator variable equal to 1 if the municipality has been previously audited. X_m contains a set of controls measured in year 2000, as well as the total amount audited.

[12]These results find additional support in a seminal paper by Ferraz and Finan (2008), who find evidence for electoral punishment of corrupt politicians after the audit's findings are disclosed to the public.

[13]For municipalities audited multiple times, we only consider the first audit, in chronological order.

over-invoicing, in which there is evidence of public goods or services bought for above market price values. [14]

We read and code each irregularity manually, collecting rich information on each case, and constructing a final dataset at the irregularity-firm level. Audited contracts that show no irregularity are not reported by the auditors, and hence are not observed. For each case in the report, we record the amount of the contract, the tax identifiers and the names of the firms involved (both winners and losers of public procurement bids, for example), the extent of their involvement (e.g. bad performance), the precise date of contract award and completion, as well as additional information that are discussed in details in later sections and in Appendix A.1.[15]

We label all firms involved in irregular public procurement contracts as "corrupt firms," regardless of the extent of their involvement. There is a high degree of heterogeneity. Some firms pay bribes, while others have no part in the act of corruption (e.g. the losing bidder in an irregular tender). Similarly, some firms perform well while others provide low quality work (e.g. an unfinished construction project). We discuss the importance of this heterogeneity for the interpretation of our findings in Section 6.

3.1.1. *Descriptive Analysis.* Figure 1 illustrates the *time* variation of the anti-corruption program. We can see from Panel A that the intensity of the program was higher in the first three years, with the number of audited municipalities decreasing over time and reaching a minimum in the final three years of the randomized program (2012 to 2014). Panel B plots the total amount of federal transfers (in millions of USD) to audited municipalities over time, showing spikes of around 1.5 billions USD in 2003, 2009, and 2010. Panel C displays the number of corruption cases uncovered by CGU auditors. Likely due to the increase in the size threshold for municipalities to be audited, we see peaks in the middle of the audit period, with approximately 3,000 corruption cases per year uncovered between 2005 and 2009. A similar pattern is shown in Panel D, which plots the number of firms involved in corruption cases over time. Detailed summary statistics on the audit program are reported in Table 1, by year (Panel A), lottery round (Panel B), audit (Panel C), and corruption case (Panel D).

Figure 2 shows the *geographical* variation of corruption across Brazil. In red, we show the large municipalities and state capitals that are not eligible for the program. In white, we show the set of municipalities that are eligible for the audit but that have never been selected. In shades of blue, we highlight the municipalities that are randomly audited between 2003 and

[14]Not all corruption is captured by our measures. For example, we do not consider cases not involving firms, such as one of a politician embezzling public funds that were supposed to be used for a cash transfer program to citizens. Similarly, the auditors cannot observe collusion among firms.

[15]Aggregating this data at the municipality level allows us to construct a novel measure of corruption in public procurement. This, together with the information on the specific firms, makes our data unique. In particular, these features differentiate our data from the original corruption measures constructed in the work by Ferraz and Finan (2008), Ferraz and Finan (2011), and Brollo et al. (2013). In a contemporaneous paper, Lagaras et al. (2017) construct similar measures of firm-level corruption.

2014. A darker shade means that a higher share of the audited contracts is corrupt.[16] The audit program covers all regions of Brazil, and the figure shows corruption is widespread.

Exploring the *magnitude of corruption*, we find that almost one fifth of the audited resources are affected by corruption in public procurement. Panel A of Table 1 shows that, within a year, CGU audits uncover on average 2,247 irregular cases involving 1,470 firms, and these account for 18% of the federal transfers to the municipalities. Considering that CGU investigate on average 821 millions US dollars per year, these estimates are significant.

3.2. **Matched Employer-Employee Data.** The principal source of firm and worker level data is obtained from the RAIS (Relação Anual de Informações Sociais) database by the Brazilian Ministry of Labor. It is widely considered a high quality Census of the Brazilian formal labor market (Dix-Carneiro, 2014). Except for the informal sector and a subset of self-employed businesses, its coverage is almost universal. In order to have at least three years of both pre- and post- data for our analysis, we focus on RAIS for the period 2000-2014.[17]

Each individual is assigned a unique administrative worker identifier called PIS, which allows for tracking of the individual over time and across firms, as well as establishments of the same firm. RAIS contains information on the tax identifiers of both the firm and the establishment of the worker, their locations and industry, and several other establishment- and firm- specific variables. 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 contract details (such as temporary, short term, apprenticeship contracts). Finally, each worker is assigned an occupational category specific to its current job. These occupational codes allow us to characterize the managers and CEOs of each firm, as well as lower level occupational layers such as blue collar and white collar workers. We can therefore analyze changes in the organizational structure of the firm in great depth.

3.3. **Additional Data Sources.** We rely on a variety of complementary data sources. In particular, in the paper we use the following data with firm-level tax identifiers: *(i)* Confidential data on the census of manufacturing firms; *(ii)* Universe of federal public procurement contracts and online procurement bids, at the contract and bid level; *(iii)* Subset of state and municipal public procurement contracts (when available online); *(iv)* Confidential loan-level information on all loans from the Brazilian Development Bank (BNDES); *(v)* Municipality-level data on the universe of bank credit, and on the location of banks and branches; *(vi)*

[16]We measure corruption as the sum of all public procurement irregularities, which we then scale by the number of business establishments in the municipality, to account for the fact that large municipalities have more contracts audited.

[17]We provide more details of RAIS and the data construction in Appendix A.2.

Firms' suspensions from participating in public procurement; *(vii)* Electoral data for the 2000, 2004, 2008, and 2012 municipal elections.[18]

3.4. **Final Sample and Summary Statistics.**

3.4.1. *Municipalities.* Out of 5,570 municipalities in Brazil, 5,526 of them are eligible for the program, and 1,881 are audited between 2003-2014. We select the final sample used in the analysis by dropping 603 municipalities that change the administrative boundary or municipality code during the period 1998-2014, and additional 192 municipalities that have no formal sector data at any point during the same period. These sample restrictions aim to limit potential sources of measurement error due to redistricting, and to remove the smallest municipalities characterized by extreme levels of informality. Finally, we drop 36 municipalities that are audited in 2014, so as to have at least one year of data after the audits for each treated municipality. For municipalities audited multiple times, we only consider the first audit, in chronological order. Our final sample consists of 4,695 municipalities, 1,581 of which are randomly selected to be audited in the period 2003-2013.[19]

Table 2 displays summary statistics for this sample. The program excludes state capitals and the largest municipalities and this is reflected in the small size of the audited municipalities' private sector. Municipalities have on average (median) of 212 (49) business establishments and 2,693 (392) private sector workers. These municipalities are also poor, with an average (median) GDP per capita of USD $4,798$ $(3,192)$. For each municipality, all the statistics are computed the three years prior to the year of audit. We confirm that the randomization of the program is effective and that audited municipalities are not different than eligible but not audited ones at the time of the randomization in Table A4.[20]

3.4.2. *Firms.* In the firm-level analysis, we mostly focus on firms involved in irregular procurement contracts with the audited municipalities ("corrupt firms"). We collect $17,449$ tax identifiers of firms that appear in all available audit reports. We match $13,637$ to the RAIS administrative database (at any point in time between 2000 and 2014). After removing firms that are part of the public sector, not-for-profit organizations, and other international entities, we remain with $12,852$ of them.[21] We then focus only on firms that have at least

[18]We refer to Appendix A.3 for more details on each source of data.

[19]The main results hold when imposing no sample restrictions, and when focusing only on municipalities audited once. These findings are discussed in Section 5 and reported in Table A13.

[20]In Table A4 we regress the probability of being audited against a host of pre-existing municipal characteristics. We use as regressors variables measured as averages in the years 2001-2002, before the start of the program. The outcome variable is a dummy for whether the municipality is ever audited in the period 2003-2014. The table confirms that the randomization of the program was indeed effective. Indeed, no coefficient is statistical significant in any of the six specifications we run, except for a small and marginally significant coefficient on total credit in column 3. More importantly, all magnitudes are negligible.

[21]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.

one employee in each of the three years leading up to the audit, and the year of audit: this reduces the sample to 6,739 firms. We then drop 817 firms that appear in the 2014 audit reports, and 180 firms with more than 500 employees in the year of the audit.[22] We remain with a final analysis sample of 5,742 corrupt firms.

In Table 3 we report summary statistics on the final sample of corrupt firms using data in the three years before the audit. Firms have a mean of 32 employees and a median of 10, both larger than the population averages of 21 and 4 employees.[23] The average total monthly wages amount to USD 15,365. A non-trivial share of firms receive government-subsidized loans from BNDES (14%) and federal procurement contracts (18%). These loans and government contracts can be quite substantial, with means of USD 11,221 and USD 14,942, respectively.

Table A5 reports the distribution of firms across sectors, at a coarse 7-sector level and at the narrowest level of 5-digit industries, compared to the national distribution in Brazil. 63% and 15% of firms are in the Retail and Construction sectors (column 1), respectively, compared to 51% and 3% in the economy (column 2). Looking at the 5-digit classification, we find that many of these cases relate to the construction of buildings, as well as to retail of food products and motor vehicles. On the other hand, all other sectors (Services, Low- and High-Tech Manufacturing, Transportation/Utilities/Communication, and Agriculture/Mining) are under-represented. An analogous story holds if we look at employment shares as well (columns 3 and 4). This distribution is not surprising, and it reflects also the higher prevalence of these sectors in public procurement more generally. We use the distribution of these shares to characterize Retail and Construction as "government dependent" sectors in the paper.

Finally, we find that many corrupt firms are located *outside* of the audited municipality.[24] As illustrated in Figure A1, only 25% of firms are same-municipality firms. However, proximity does matter. Using the Brazilian classification of geographies into municipalities, micro-regions, meso-regions, and states (with each of the latter fully containing multiple of the former), we find that only 9% of firms are from a different state, and most firms are located nearby the municipality.

4. Empirical Design

In this section we describe how the randomization of anti-corruption audits allows us to address the endogeneity of corruption. The key identification concern is that unobserved confounding factors may be correlated with both corruption and economic outcomes. For example, poor economic conditions may lead to corruption, thus preventing us from causally estimating whether corruption hampers local economic growth. Similarly, firms with promising growth prospects may be more likely to pay bribes, thus biasing estimates of the effect of

[22]Main results are robust to these sample restrictions.

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

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

corruption on firm performance. As argued in Section 2.1, audits provide exogenous variation in corruption levels across municipalities. In this section we discuss the empirical strategy and related identifying assumptions at both the municipality-level and the firm-level.

4.1. **Empirical Strategy at Municipality Level.** The design of the anti-corruption program lends itself to a municipality-level event study estimation method. Indeed, both the timing of the audit and the municipality selected are random. Moreover, the CGU guidelines clearly state what is the set of municipalities that are eligible to be audited at each round. We therefore create a set of treated and never treated municipalities at different points in time. Then, within the same calendar years, we compare the outcomes of municipalities that are audited in year t to those audited in year $t + \mu$ and the never treated municipalities.

4.1.1. *Estimating Equations.* We estimate both non-parametric and parametric event study models. We first estimate a fully dynamic specification that allows us to capture the dynamics of real economic outcomes relative to the year of audit. The basic non-parametric even study specification is the following:

$$y_{mt} = \alpha_m + \alpha_t + \sum_{k=-3}^{k=-2} \mu_k + \sum_{k=0}^{k=5} \mu_k + \epsilon_{mt} \tag{4.1}$$

where m and t stand for municipality and year, respectively, and $\{\mu_k\}$ capture the relative event time indicators. That is, μ_k is an indicator variable taking value 1 if it is year k relative to the audit year. These indicator variables are always 0 for municipalities that are never audited. We choose a window of 9 years around the event. As is typical in event study frameworks, we make the normalization $\mu_{-1} = 0$, so that all coefficients represent differences in outcomes relative to the year before the audit. The specification includes municipality fixed effects (α_m) and year fixed effects (α_t), which absorb fixed differences across municipalities and across years. ϵ_{mt} are standard errors clustered at the level of the municipality (Bertrand et al., 2004).

The parametric specification allows us to analyze the statistical significance and magnitude of the estimates. We estimate the following specification:

$$y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt} \tag{4.2}$$

where m and t stand for municipality and year, respectively, and $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. The parameter of interest is β, which measures the change in the outcome variables of the audited municipalities compared

to the yet-to-be audited and eligible but never audited municipalities, conditional on the set of municipality and year fixed effects.[25]

4.1.2. *Identifying Assumptions.* The interpretation of β (or the $\{\mu_k\}$ indicators of equation 4.1) as the causal impact of the anti-corruption audits requires the identifying assumption that the timing of the audit is uncorrelated with municipal economic outcomes, conditional on the set of municipality and year fixed effects. For example, an audit that is preceded by a reduction in corruption and a change in economic activity would violate the identifying assumption.

However, the design of the program limits these concerns, as the audits are randomized across the pool of eligible municipalities, as also illustrated in Table A4. Additionally, we can directly assess this assumption in the data by analyzing the dynamics in the μ_k coefficients of equation 4.1, as we illustrate in Section 5. For our research design to be valid, audited and control municipalities should follow parallel trends in the years prior to the audit, which implies that the pre-period μ_k indicators should not be statistically different from zero.

An additional concern is that different municipalities have different actual and perceived audit probabilities. This may happen for two reasons. First, as shown in Table A1, municipalities in different states are subject to differences in actual audit probability. This is a possible concern if, for example, municipalities in small states (where the audit probability in a year can be as high as 43%) react differently to the differential audit threat, in the pre-period, compared to municipalities in large states (where the audit probability in a year can be as low as 1%). Second, the perceived probability and salience of the audit, and hence its deterrence effect, may change over time. This could be because municipalities learn both about the details and the consequences of the audit. Under this scenario, municipalities audited at later points in time may not represent a valid counterfactual for municipalities audited today.

We address both concerns by re-estimating a different model based on a dynamic matching difference in difference estimator, where we compare audited municipalities to a contemporaneous control group of never audited municipalities subject to the same audit probability. The matching on audit probabilities deals directly with the first concern, while the use of a contemporaneous control group of only never audited municipalities deals with the second. We discuss this alternative strategy in details in Appendix A.1.

Another potential concern is the presence of direct spillover effects of the audits on control municipalities. For example, the anti-corruption crackdown in a municipality may increase the threat of monitoring and punishment of corruption in a neighboring municipality, thus

[25]We study heterogeneous effects based on differential pre-existing characteristics using the following interacted specification:

$$y_{mt} = \alpha_m + \alpha_t + \beta_1 \times PostAudit_{mt} + \beta_2 \times Heter_m \times PostAudit_{mt} + \epsilon_{mt} \tag{4.3}$$

where $Heter_m$ is a characteristic of the municipality measured in the one or three years before the audit.

contaminating our control group and likely biasing our estimates *downwards*.[26] We attempt to deal with these concerns by using the alternative dynamic difference in difference matching strategy mentioned above, and by selecting control groups of never audited municipalities that are distant from any audited municipality around the audit year. This makes the control group less likely to be subject to direct spillover effects.

We discuss these and additional tests in more details in Sections 5 and 7.2.

4.2. **Empirical Strategy at Firm Level.** We are interested in estimating the impact of anti-corruption audits on "corrupt firms," namely firms who are involved in irregular public procurement with the audited municipalities. Compared to the municipality-level one, this analysis presents one important challenge: while municipalities are randomly audited, corrupt firms are not. Therefore, we identify a contemporaneous control group of firms using matching estimators.[27]

There are three main challenges to selecting counterfactual corrupt firms. First, irregular public procurement contracts are only observable in audited municipalities. Second, data on *municipal* procurement contracts is mostly unavailable.[28] Third, corrupt firms represent a selected group of firms. Indeed, as discussed in Section 3.4, corrupt firms are larger, concentrated in specific sectors, and experience positive growth before the audit.

4.2.1. *Identifying Counterfactual Corrupt Firms.* We approach the above issues through a matching strategy that assigns to each corrupt firm a control firm that is in the same sector, and has the same observable characteristics in *each* of the three years leading up to the audit. Moreover, we select controls to be located in never-audited municipalities, to limit the spillover effects of the audits on local firms.[29] The richness of the administrative data we can

[26]This would be a violation of the stable unit treatment value assumption (SUTVA)'s requirement that the treatment status of any one unit must not affect the outcomes of any other unit (Rubin, 1978, Manski, 2013, Clarke, 2017).

[27]An event study approach similar to the one used for the regional analysis is problematic in this context, for two main reasons. One, there is no set of "never treated" firms that represent a valid counterfactual for corrupt firms, as corrupt firms are not randomly drawn. As a result, an event study model can be estimated off the corrupt firms only, which leads to the second empirical challenge. In such a case, the audit treatment will be correlated with the outcomes of interest (employment, firm exit), thus violating the primary identifying assumption. Intuitively, a firm must be alive to be treated at a later point in time, thus generating a mechanically higher survival rate for control firms. Similarly, corrupt firms are on a positive growth path in the years prior to the audit, because the majority of them won a local government contract. These issues make the corrupt firms revealed later an invalid counterfactual. An additional concern is that the higher growth rate up to the audit is also mechanically linked to the timing of the audit, since the latter often coincides with the completion date of the contract. Additional empirical issues that arise in event study models with no contemporaneous control group are discussed in Borusyak and Jaravel (2016).

[28]Public procurement in Brazil takes place at three levels: *federal*, *state*, and *municipal*. We observe the universe of federal procurement contracts, while only a small subset of the latter two, which we obtained by manually searching for the list of contracts on the municipalities' and states' websites. This data is available only for a few states and municipalities, and mostly for the years after 2010.

[29]80% of the firms in our final sample are single-establishment. For the remaining 20% we define the location of a firm as the location of the firm's headquarter. Results are robust to the exclusion of multi-establishment firms.

match on, together with the randomness of the audit, helps alleviate several empirical issues. In Section 4.2.3 we discuss the identifying assumptions and how we address various concerns.

Our matching strategy follows a "Coarsened Exact Matching" procedure (Iacus et al., 2012) that consists of three rounds of sequentially less restrictive matching requirements. We implement the matched sampling procedure separately for each audit year. In each audit year, a firm is sampled at most once as a control firm, but firms can be sampled multiple times across years.[30] In each of the three years prior to the audit, we match each corrupt firm to control firms on sector and on the distributions of employment, payroll, hirings, firings, number of plants, and municipality size. We also match on whether the firm has a manager, whether it won or bid for a federal procurement contract, and whether it obtained a government loan, in any of the three years prior to the audit.[31] When multiple potential matches are found for a given firm, we select the best matched firm from the comparison group with the closest propensity score, where the score is computed based on a linear probability model that includes employment levels in the three years before the audit.[32]

4.2.2. *Estimating Equations.* After constructing a contemporaneous control group of firms, we focus on a window of [-3,5] years around the audit, and estimate the following difference-in-difference model:

$$y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt} \tag{4.4}$$

where y_{jt} represents the main outcome variable of interest for firm j in year t. α_j captures firm fixed effects that control for average differences between the treatment and the control group. α_t are calendar year fixed effects controlling for average differences across years. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The coefficient of interest is β, which captures the effect of the anti-corruption audits on corrupt firms, relative to the control firms, and leveraging variation within the same firm and within the same year. ϵ_{jkt} are standard errors clustered at the level of the audited municipality.[33]

[30] In the analysis, we consider firms sampled in different audit years as different firms, so as to allow for a finer set of firm fixed effects.

[31] Round 1 (15% matched) is the most restrictive matching that uses all the information, and matches on *ventiles* of the distributions. Round 2 (22% matched) matches on *deciles* of the distributions. Round 3 (63% matched) only matches on sector and on deciles of the employment distribution in the three years before the audit. We test the robustness of our results to subsamples based on different matching rounds, and results are similar across rounds.

[32] Results are robust to the inclusion of *all* controls in the estimation.

[33] We study heterogeneous effects based on differential pre-existing characteristics using the following interacted specification:

$$y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta 1 \times PostAudit_{jt} \times Audited_j + \beta 2 \times PostAudit_{jt} \times Heter_j + \beta 3 \times PostAudit_{jt} \times Audited_j \times Heter_j + \epsilon_{jt} \tag{4.5}$$

where $Heter_j$ is a characteristic of the firm measured in the one or three years before the audit.

We also estimate the dynamic version of the model at the firm-level:

$$\begin{aligned} y_{jkt} =& \alpha_j + \alpha_t + \sum_{k=-3}^{k=-2} \delta_k \times \mathbb{1}(Event_k) + \sum_{k=0}^{k=5} \delta_k \times \mathbb{1}(Event_k) \\ &+ \sum_{k=-3}^{k=-2} \beta_k \times \mathbb{1}(Event_k) \times Audited_j + \sum_{k=0}^{k=5} \beta_k \times \mathbb{1}(Event_k) \times Audited_j + \epsilon_{jkt} \end{aligned} \tag{4.6}$$

where $\mathbb{1}(Event_k)$ is an indicator function taking value 1 if it is year k relative to the audit year. We include all leads and lags around the audit time in the model. We also normalize the coefficient in $k = -1$ to 0 ($\beta_{-1} = 0$), so that we compare outcomes of treated and control firms relative to the year before the audit.

4.2.3. *Identifying Assumptions.* The main identifying assumption is that, conditional on firm and year fixed effects, and the set of firm characteristics used for the matching, the audit treatment is uncorrelated with firm-level unobservables affecting subsequent firm performance. Unlike the municipality-level analysis, we cannot directly test this assumption by visually assessing parallel trends in the years prior to the audit, since we directly match on pre-audit characteristics. We therefore discuss in Section 6 the robustness of the results to an alternative strategy based on a synthetic control method that only matches on data up to 2 years before the audit, which allows us to use the year before the audit as a validation period (Abadie and Gardeazabal, 2003, Abadie et al., 2010).[34]

There are several concerns we must address. A primary worry is that while we observe (the universe of) *federal* government contracts, we cannot observe contracts between firms and *municipal* governments, and therefore we cannot match on them. That is, in any given year we do not know whether control firms have procurement contracts with municipalities. This information is only available conditional on the audit taking place, and for the set of firms involved in irregular contracts with the audited municipality. Consider the case in which the audit report identifies a firm with an on-going contract with the municipality, namely a firm that still needs to complete the contract.[35] We may be worried that changes in firm outcomes after the audit are a reflection of this, as for example government contracts require more labor and generate firm growth (Ferraz et al., 2015). However, this mechanical effect is unlikely to account for our findings, as most contracts are completed immediately, as in the case of retail goods (which consist of 63% of our sample). We also conduct a direct test for

[34]While the synthetic control method allows for an optimal use of pre-characteristics of all firms in the economy, we rely on the coarsened exact matching (CEM) procedure discussed above for the primary analysis, because of its flexibility. We discuss the synthetic control strategy in more details in Appendix A.2.

[35]We discussed with the CGU officials and auditors what happens in such cases, that is whether the contract becomes invalid or whether the firm is allowed to complete it. Their answer was that most of these contracts are completed when they are audited, especially considering that the majority of them relate to purchase of goods and services. In cases in which the contract is still active (e.g. a long-term construction project), they evaluate case-by-case, depending on severity of the irregularity, potential legal actions, and the extent of firm's involvement.

this channel, by excluding contracts that were awarded in the year of the audit, i.e. contracts with the highest likelihood of being on-going at the time of the audit.

A related issue is that Ferraz et al. (2015) show that *federal* government contracts lead to firm growth even after the contract ends. While we focus on smaller *municipal* contracts, similar mechanisms may take place. This means that the effects we find could be driven by other mechanisms, rather than the audit. We therefore use data on federal government contracts to show that our results hold also when restricting the sample to corrupt and control firms both having on-going federal government contracts at the time of the audit.

An additional consideration is that control firms may also be directly affected by the audit. This is an issue akin to the spillover effects discussed earlier for municipalities. While we select control firms to be located in never-audited municipalities, they may still engage in business with the audited municipality, which we cannot observe, as mentioned above. We may expect these spillover effects to bias our results *downwards*, as the same forces affecting corrupt firms may affect control firms. Other considerations may justify an opposite bias. For instance, if we capture competitors of the corrupt firms, the bias is harder to sign. We therefore show that the results are robust to a different control group of firms that are distant from the audited municipality. That is, for each corrupt firm doing business with audited municipality A, we select as control a firm that is located in a *different state* than municipality A. This limits the direct spillovers of the audit, as it is unlikely that municipal procurement is conducted by firms located in a different state, as discussed in Section 3 and shown in Figure A1.

We discuss the above and additional tests in more details in Sections 6 and 7.3.

5. The Impact of Anti-corruption Audits on the Local Economy

The first part of our analysis investigates the impact of anti-corruption audits on the local economy. This (macro) analysis allows us to test for positive ("grease the wheel") versus negative ("sand the wheel") theories of corruption. "Grease the wheel" theories posit that, conditional on the presence of red-tape, corruption may benefit firms, allowing them to overcome inefficient regulations and improve performance (Leff, 1964, Huntington, 2006). Hence, if "grease the wheel" theories are at play, we should expect lower corruption to *reduce* economic activity. That is, firms would find it harder and more costly to conduct their business without corruption. On the other hand, lower corruption should *increase* economic activity according to "sand the wheel" theories. That is, resources may be allocated to more efficient firms, and firms may face lower costs and distortions when corruption levels are lower.

5.1. **The Impact of Audits on Real Economic Outcomes.** We begin by studying the impact of the anti-corruption audits on real economic outcomes, which we measure using the natural log of total employment and number of business establishments.[36]

We first explore the dynamics of the effects around the audit by estimating equation 4.1. Figure 3 displays the point estimates of the non-parametric event study over the window of [-3,5] years around the audit, where we normalize the coefficient in the year prior to the audit. The figures provide a visual test for our identification strategy. In a difference-in-difference framework, we should observe no differential pre-trends in the pre-period between audited and control municipalities, which implies that the pre-period point estimates should not be statistically different from zero. This is what we observe for both employment and establishments in Figure 3. The figures also illustrate the positive impact of the audits on the real economy. After the audit, we see an increase in employment and number of establishments. The effects take place immediately and are persistent over time.

We interpret the magnitude of the findings by estimating equation 4.2. We report the results of this parametric event study in Table 4. As already discussed above, we see that the audits have a positive and strongly significant impact on both measures of economic activity. Columns 1 and 2 show an increase of around 2.6% in both employment and establishments in the municipality in the five years following the audit. The magnitudes of the results provide support for a non-negligible positive impact of the anti-corruption audits on real economic activity at the local level.

In Section 7 we illustrate additional tests to address various empirical concerns mentioned earlier, and we discuss alternative interpretation of our main findings. In Appendix Table A13, we also report several robustness checks. In particular, we show that results hold when: (i) we exclude the first two years of the program, when eligibility requirements were changing (columns 1 and 2); (ii) we exclude the last 5 years of the program, so as to have a balanced set of coefficients for the pre- and post- period (columns 3 and 4); (iii) we estimate the event study specification 4.2 using only audited municipalities (columns 5 and 6); we do not drop any municipality from the estimation (columns 7 and 8); we focus only on municipalities audited once (columns 9 and 10).

5.2. **The Real Effects of Corruption?** We argue that a main channel behind the positive effects of audits on the local economy is that of lower corruption levels. This allows us to reject "grease the wheel" theories on the positive effects of corruption, according to which corruption is beneficial to business. This is consistent with Section 2.1, where we argue that audits reduce corruption, as shown by Avis et al. (2016) and in further tests we perform in Appendix Tables A2 and A3.

[36]In the RAIS database, we can differentiate firms and establishments. In this section, we focus on establishments, as they can always be assigned to a specific municipality, while firms can span multiple municipalities. In practice, most firms are single-establishments firms, and results are almost identical using firms instead of establishments.

We further probe the direct mechanism of reduced corruption by showing that the results are stronger in cases where corruption frictions are likely higher. First, we split by the median the sample of audited municipalities into High Corruption and Low Corruption ones. We do so based on the number of irregularities in public procurement that the audits uncover, scaled by the number of establishments in the municipality to account for the fact that larger municipalities have more contracts audited and thus mechanically more irregularities. Hence, we interpret this as a measure of corruption intensity in the municipality. If audits affect economic outcomes through reduced corruption, we would expect the effects to be more pronounced in highly corrupt areas. We then estimate specification 4.2, augmented by the heterogeneity indicator for High Corruption (HC) municipalities. The results are presented in Table 5, columns 1 and 2. Consistent with our hypothesis, we find that highly-corrupt municipalities experience relatively higher employment and number of establishments after the anti-corruption audits, as shown by the positive and significant interaction terms in the table.

Second, we would expect the impact of the audits to have heterogeneous effects on different sectors. In particular, corruption frictions are likely more severe in sectors that depend more on government services and that involve more frequent interactions with the public officials. Motivated by our earlier discussion on the distribution of corruption cases across firms, we define the Retail and Construction sectors together to be "government dependent." Columns 3 and 4 of Table 5 report the heterogeneous effects for these sectors, estimating the interacted version of equation 4.2. Similarly to the case of highly corrupt areas, we find the effects to be concentrated in government dependent sectors.

In sum, these results point towards a direct impact of the audits on economic activity through their impact on reduced corruption levels, thus providing evidence against "grease the wheel" theories of corruption. A possible direct mechanism behind these effects is that a higher share of municipal resources are now channeled to firms' productive activities, rather than embezzled by corrupt agents. This is naturally difficult to observe in the data. However, studying the changes in municipal budget may shed some light on this channel. We report an analysis of expenditures and revenues in the municipality following the audit in Table A7, where we estimate equation 4.2. Interestingly, we find that while expenditures stay constant (columns 1, 2, and 3), we find a positive and significant impact of the audits on municipal revenues (columns 4, 5, and 6). This may simply be a response to the higher levels of economic activity we observe after the audit, but at least it indicates that the higher economic activity represents a real increase in economic output.

In Section 6, using a firm-level analysis, we test and discuss two specific channels to explain *how* audits affect firms.

5.3. **Entrepreneurship, Employment Creation, and Access to Finance.** We study additional effects of the audits on local economic outcomes, so as to better understand the role of corruption as a barrier to entrepreneurship and access to finance. Together with

the main effects on employment and business establishments, these findings imply a positive effect of the audits on the local economy.

In Table 6, we estimate equation 4.2 to explore entrepreneurial activity, which we measure using entry and exit of establishments in the municipality.[37] We find that the audits increase economic activity through significantly higher levels of firm creation (1.8%) rather than lower rates of firm exit, consistent with the idea that corruption limits local entrepreneurship.

We investigate the contribution of new firm creation to local economic growth by separately estimating, in Table 6, the effect of the audits on employment from new firms (column 3) and employment from incumbent firms alive at the time of the audits (column 4). We find that employment in new firms is more responsive than employment in incumbent firms (4.7% and statistically significant at one percent compared to a statistically insignificant 2.1% increase), indicating the importance of local entrepreneurship for economic growth at the local level. In Table A8 we also decompose the new employment creation into employment coming from different geographical areas (for example, neighboring municipalities or different states) and other sources, and find that most employment creation comes from unemployment and the informal sector.[38] This result allows us to partially account for general equilibrium effects that may come from a reallocation of economic activity from firms in other municipalities, suggesting a net positive effect of the audits on the economy.

Higher levels of entrepreneurial activity and new employment creation likely depend on the ability of local firms and entrepreneurs to access credit. We study this in the data, and find that anti-corruption audits lead to an increase in credit availability to firms in the municipality. Estimating specification 4.2, we show in column 5 of Table 6 that the audits lead to a 3.8% increase in lending from private banks to local firms. Unfortunately, we do not have access to loan application rates, and therefore we cannot disentangle whether this is driven by a demand or supply channel. This result suggests that one mechanism through which corruption may affect firms is by limiting their ability to raise capital. As further evidence to this channel, we also show that the positive effects of the audits on local economic activity are especially pronounced in areas with ex-ante higher availability of finance. We do so by splitting municipalities into two groups, high access to finance vs low access to finance, depending respectively on whether they are above or below median in terms of per capita bank branches (or per capita lending volume) in the year before the audit. We then estimate the interacted version of the event study model 4.2, and report the results in Table A9. Consistent with our hypothesis, the increase in economic activity is concentrated in areas with better opportunities to access finance.

[37]Entry corresponds to a new establishment, namely one that appears for the first time in the formal sector in the given year. Exit corresponds to a dead establishment, namely one that does not appear anymore in any of the following years. All variables are measured in logarithmic terms.

[38]In the data we cannot distinguish between informality and unemployment.

6. How Does Corruption Affect Firms?

The analysis in Section 5 concludes that the audits positively affect the local economy, as reflected in increased economic activity, higher levels of entrepreneurship and new employment creation, and more availability of credit. We also argue that the effects are directly related to a reduction in corruption as a result of the audits, and that we can therefore reject "grease the wheel" theories. In this section we aim to test specific channels about *how* corruption negatively affects firms and the local economy.

We explore two channels. As a first channel, we hypothesize that corruption is detrimental to the local economy because it imposes higher costs on firms. In this case corruption acts as a tax, for example because firms are extorted bribes by the corrupt public officials to obtain government contracts. As a second channel, we posit that corruption hampers local economic growth because it distorts the allocation of government contracts towards inefficient firms. In this case corruption acts as a subsidy, for example because inefficient politically connected firms obtain government contracts based on favoritism, rather than efficiency. We provide a simple theoretical model to formalize this intuition in Appendix A.1.

To test for these channels, we focus on the set of firms for which the corruption distortions are certainly present. We can identify them in the data as the firms who are directly involved in irregular cases of local procurement, namely "corrupt firms." We assume the anti-corruption audits alleviate the corruption distortions, thus generating opposing predictions for corrupt firms. On the one hand, if corrupt firms are subject to a corruption tax, their performance should improve after the audit. For example, these firms can now obtain a larger share of the profits from public procurement contracts. Similarly, audits may generate an overall higher demand for these government dependent firms, as more funds are now channeled to actual municipal spending rather than embezzled by the corrupt officials. On the other hand, if corrupt firms are subject to a corruption subsidy, their performance should suffer after the audit. For example, government contracts may now be reallocated from inefficient politically favored firms towards more efficient ones.

6.1. **Visual Analysis of the Raw Data.**

Figure 4 displays the raw data on employment levels for the corrupt firms (black line), as well as the matched control firms (gray), and all firms in the economy (green), around the year of the audit.[39]

There are two main takeaways from the figure. First, as previously discussed, we observe large differences in average size and pre-trends of the corrupt firms compared to all other firms in the economy. The size and pre-audit growth differences can be gauged at by looking

[39]We create the raw data on all firms in Brazil by keeping, for each audit year, all firms alive in that year and in the previous three years, and having no more than 500 employees. This ensures we apply similar sample restrictions to those that we apply to the corrupt firms analysis sample.

at the gap between the black line for corrupt firms, and the solid green line for all firms.[40] This highlights the importance of selecting a control group of similar firms.

Second, we see a differential increase in the average size of corrupt firms after the audit. The matched control group of firms (gray line) closely resembles the path of the corrupt firms in the pre-period, by construction. This figure is the most transparent visual representation of our main result: anti-corruption audits improve the performance of "corrupt firms." In light of these findings, we argue that a primary channel through which corruption operates is an increase in costs to firms involved in business with the government, rather than a misallocation of resources towards inefficient firms.

6.2. **Corruption as a Cost of Doing Business.** We study the magnitudes of the results by estimating equation 4.4. We report the estimation results in Table 7. Following the audit, we observe an increase in employment (column 1) for corrupt firms in the magnitudes of around 11% (column 1) in the five years after the audit.[41] These findings indicate that corruption primarily affects the local economy by imposing costs on government dependent firms. Corrupt firms represent the set of firms that are likely most affected by such corruption frictions, and we find a strong and positive impact of the audits on their performance. In section 7 we also discuss the impact of the audits on other sets of firms, and show that our results can be applied to a broader set of government dependent firms.

Figure 5 reports the point estimates from specification 4.6, together with 95% confidence intervals. As we already discussed by examining the raw data, we can see that the trends of corrupt and control firms are parallel in the years leading up to the audit. After the audit, the positive effects take place immediately, and they are persistent over time.

We also explore the extent to which our results can be explained by compositional changes in the sample, primarily death of firms after the audit. That is, we may observe that the firms with the worst growth prospects exit our sample because of the audits, and this may in turn bias our coefficient on employment. In column 2 of Table 7, we therefore estimate the baseline specification using as dependent variable an indicator for firm death, i.e. the firm disappears from the formal sector. We find that corrupt firms are around 1.5% *less* likely to exit, following the audit. In Appendix Table A16, column 1, we also re-estimate specification 4.4 focusing only on corrupt firms (and respective controls) that are alive during the entire estimation window of [-3,5] years around the audit. Using log employment, the results hold for this subsample as well, and the magnitudes are almost identical. These results suggest that firm exit does not play a role in driving our main findings.

Finally, higher levels of employment may not necessarily mean better firm performance and positive outcomes for the main stakeholders. For instance, it may be the case that, after the audits, firms need to employ more labor to maintain the same level of sales. We therefore

[40]The average increase in size for all groups after the audit is partly due to survivorship bias, combined with the fact that we require firms to be active in all three years before the audit.

[41]We define employment as the total number of full time employees who worked at the firm at any point in time in the given year.

directly explore the impact of audits on sales in column 3 of Table 7, where we focus on the set of firms that we are able to match to the manufacturing census (PIA).[42] Consistent with the positive impact of audits on corrupt firms, we find that they experience an increase in sales of around 13%, relative to control firms. In column 4 we further explore the impact of the audits on investment, which we measure as total capital expenditures over sales. We observe that corrupt firms increase investment by around 39% following the audit (5 percentage points relative to a mean of 12.9). This large positive effect suggests that corruption can severely deter firm's investment, consistent with a story in which a corrupt environment generates uncertainty that affects firm strategic decisions.

In Section 7 we illustrate additional tests to address various empirical concerns mentioned earlier. We perform several additional robustness checks, which we report in the Appendix. In Table A17, we show that the results are robust to different empirical specifications. In columns (1) and (2) we add age of the firm fixed effects. In columns (3) and (4) we add municipality-year and sector-year fixed effects. In columns (5) and (6) we use as dependent variables both levels and log of employment level, each winsorized at 5% of the empirical distribution. In columns (7) and (8) we weight the regressions by the level of employment the year before the audit. In columns (9) and (10) we report the baseline results using an unrestricted window around the audit. Table A18 shows that results hold both for multi-plant (columns 1 and 2) and single-plant firms (columns 3 and 4), and that they are slightly larger for the former. Finally, Table A19 reports robustness of the results to the three matching rounds, showing that the results are similar independently of the quality of the control group matching.

6.3. **Heterogeneous Effects.** The detailed data we collect from the audit reports allow us to disentangle the effects related to different types of corruption cases. Studying the heterogeneous effects that depend on the firm's involvement in the irregularity case can help validate our findings and further sharpen our tests. On the one hand, for example, our conceptual framework would predict that inefficient firms should *not* benefit from the audits. Hence, we create a proxy for inefficient firms, which we capture by means of an indicator for corrupt firms who performed poorly. These are mostly firms that are directly involved in the corruption case and are awarded the government contract but do not complete the work, or complete it in an unsatisfactory manner (e.g. using lower quality materials in a construction project). We call these "Quality" irregularities. On the other hand, there are cases of firms that are passively involved in the corruption case. For example, some firms may appear in the audit report as bidding losers in a procurement auction. These firms are likely suffering from the presence of corruption costs, which we can test in the data. We call

[42]As the PIA database only includes a small sample of firms in the economy, we are only able to match a subset of corrupt (and control) firms. For the subset of corrupt firms that we match, we therefore create a control group of firms within the PIA database, using the same matching strategy described in Section 4.2.

these "Passive Involvement" irregularities. All other irregularities that are nor "Quality" nor "Passive Involvement" are referred to as "Corruption" irregularities.

We study such heterogeneous effects in Table 8, where we estimate the interacted version of equation 4.4. The interaction variables of interest are indicators for whether it is a case of (i) "Corruption" (columns 1 and 4), (ii) "Quality" (columns 2 and 5), (iii) "Passive Involvement" (columns 3 and 6). The results confirm that inefficient firms do not benefit from the anti-corruption program, as it is shown by their relatively lower employment levels and higher likelihood of death. On the other hand, passively involved firms experience positive effects following the audit. The latter finding indicates that our results may extend to a broader set of government dependent firms.

6.4. **Within-Firm Distortions.** The results so far indicate that corruption represents a severe friction to the performance of firms doing business with the local government. In this section we attempt to shed light on the specific distortions through which corruption negatively affects firms. Since the direct costs of corruption –*bribes*– cannot be directly observed over time, we focus on firm's resource allocation decisions, and using loan-level, worker-level, and contract-level micro data we suggest that they change in response to the anti-corruption crackdown.

6.4.1. *Access to Finance.* We first investigate the impact of the anti-corruption program on firms' borrowing decisions. Firms doing business with the government may invest less when facing the uncertainty of a corrupt environment, which is consistent with the earlier result that firms invest significantly more following the audits. Alternatively, a firm's stakeholders may put a price on the risk associated to doing business in a corrupt and opaque environment and, in the case of banks, they may contract lending or increase interest rates. Since the audits potentially reduce firms' and banks' exposure to corruption, as previously discussed, we may expect them to change firms' ability and willingness to raise capital.[43]

We use loan-level data from the Brazilian Development Bank (BNDES) to study whether audits affect borrowing and leverage at the firm level. The BNDES represents the major lender of Brazilian companies. Table 9 illustrates the impact of the audits on firm access to BNDES loans, using three main dependent variables that capture both extensive and intensive margins. In column 1 we look at the *number* of BNDES loans, scaled by size of the firm, and find a considerable increase after the audits (0.242 relative to a pre-period mean of 0.143). In column 2, we instead look at the (log of) average *amount* per loan borrowed from the BNDES, also scaled by size of the firm, finding a sizable 47.8% increase. In Table 9, column 3, we also explore whether the audit changes the *interest rate* on government loans. We find a negative but statistically insignificant effect in this case. Overall, the analysis using loan-level data shows that firms borrow more and increase leverage after the anti-corruption

[43]Unfortunately, we do not have data on loan application rates, and as a result we are not able to disentangle demand and supply effects.

audits, suggesting that firms' financial decisions may be severely affected by the presence of corruption.

6.4.2. *Labor Allocation Within the Firm.* We then take a within-firm approach, based on the idea that a corrupt and highly bureaucratic environment may lead firms to operate differently than they would in a frictions-less context. If the audit alleviates these frictions, we may expect the firm to restructure its internal organization after the anti-corruption crackdown. This implies that the composition of the workforce may be different after the audits, for instance due to changes in the presence of less productive politically connected employees. Looking *inside the firm* may therefore reveal whether these mechanisms are at play.

We study the within-firm reallocation of labor by looking at the impact of the audit on wages (monthly payroll per employee), employee churn (i.e. sum of firings and hirings), organizational structure, and average employee ability. Table 10 reports these findings, which are obtained from the estimation of equation 4.4. We find that employees' wages increase by 2% after audits (column 1), and that the firm growth is accompanied by an increase in employee churn of 18% (column 2). In addition, the organizational structure of the firm becomes more decentralized, as is evident by the positive coefficient on "Number of Layers."[44]

These findings are consistent with a story of internal restructuring following the audits. While data on worker-level productivity is not available, we argue that this internal restructuring is productivity-enhancing. First, if we consider payroll per employee to be a good proxy for quality of the worker, we find that this measure of quality improves (column 1). We then construct a second measure of worker ability, based on the Mincer residuals of a regression of an individual's private sector earnings on a rich set of fixed effects such as age, gender, and job occupation. We construct this measure for each individual in the economy, and then average it within firms, to construct measure of average quality of the workforce by year-firm. As we can see in column 4 of Table 10, this measure also shows a positive coefficient. Hence, these results suggest the audits may lead to an improved allocation of labor within the firm.

6.4.3. *Doing Business with the Government.* Another important effect of the audits may be that of making it easier for firms to expand to new markets and products. This could be due to lower transaction costs and easier access to competitive auctions, for example. Our contract-level data on federal public procurement allow us to analyze how firms may change their sources of business activity.

We estimating equation 4.4 using a variety of public procurement outcome variables. The results of this estimation are illustrated in Table 11. We find that the audits lead to a *net decrease* in firm's reliance on federal government contracts as a source of business. As

[44]We construct the variable "Number of Layers" by summing the indicators for whether the firm has at least one employee in each of the four possible layers, namely CEO, Manager, White Collar, Blue Collar. We refer to Appendix A.2 for more details on how we construct these categories from the occupation-level data. This approach follows Caliendo et al. (2012).

shown in column 1, firms decrease the total number of federal contracts by 57% after the audits. On the intensive margin, we also see a decrease, conditional on winning, in the average size of the contract by around 6.6%. In columns 3 and 4 we split the analysis into *preferential* versus *competitive* government contracts, respectively, measured as indicators for whether the firm wins any such contracts.[45] The results indicate a substitution of activity away from discretionary contracts towards competitive ones. A speculative interpretation of this finding is that the corrupt bureaucrats were over-utilizing discretionary procurement procedures, where rent extraction is easier, to the detriment of competitive ones, which would have potentially benefited the firms instead.[46] Finally, looking at the online auctions for off-the-shelf goods, we can look at the bidding behavior as well. In column 5 we observe a 10% increased participation in online procurement auctions of firms, measured by total number of bids in procurement auction. This increased participation also translates into higher win rates, as shown by the positive coefficient in column 6.

7. Additional Analysis and Survey Evidence

In this section we discuss several auxiliary tests to shed more light on the validity and interpretation of our regional level and firm level findings.

7.1. **The Effect of Audits on Other Firms.**

Exploring the impact of audits on other firms represents a useful test to sharpen our results, as corruption frictions may affect a broader group of government dependent firms. Hence, we focus on the impact of audits on the following sets of firms: (i) local establishments, (ii) local establishments in the retail or construction sectors, (iii) local establishments in the same 5-digit industries of corrupt firms, (iv) local establishments belonging to firms that participated in federal public procurement before the audit.[47]

According to the economic channels discussed earlier, an anti-corruption crackdown should differentially benefit "government dependent" (GD) firms, namely (ii), (iii), and (iv) above, relative to local firms (i). Subsequently, in Appendix Table A10, we estimate a dynamic

[45]We classify as *preferential* the "Convite" and "Tomada de Precos" tender mechanisms, which essentially either involve a restricted invitation of firms to bid, or require special conditions for firms to participate in the auction. On the other hand, we classify as *competitive* the online auctions for off-the-shelf goods ("Pregao") and the largest open invitation tender mechanisms ("Concorrencia").

[46]We do not observe the universe of *local* contracts with the government. It may therefore be the case that there is a substitution from federal to local government contracts as well. We are currently in the process of collecting a subset of municipal level contracts to analyze this phenomenon in more details.

[47]Unfortunately, we cannot observe firms affected by corruption frictions that are not captured by the audits. Additionally, as discussed earlier, we do not observe the universe of firms doing business with Brazilian municipalities, which represent a likely set of firms affected by government corruption.

difference-in-difference model where we exploit random time variation given by the anti-corruption audits, and cross-sectional variation given by the *type* of local firm. We also compare each set of firms to corrupt ones.[48]

While the results vary depending on outcomes and the definition of GD firm, they are broadly consistent with our hypothesis: audits lead to growth of government dependent firms relative to other local firms, as all types of the former outperform the latter. Interestingly, corrupt firms outperform most other sets of firms, but not all.[49] These results suggest that the positive effects on government dependent firms are not driven by an aggregate positive impact of the audits on all existing firms in the local economy.

While the above analysis is purely suggestive, as different firm types are likely different under various dimensions, it provides further evidence that anti-corruption audits primarily affect the local economy by reducing frictions that distort the operations of government dependent firms.

7.2. **Additional Tests at Municipality Level.** As discussed extensively in Section 4.1, the main identifying assumption in the regional analysis is that the timing of audits is uncorrelated with municipal economic outcomes, conditional on municipality and year fixed effects. Figure 3 provides a visual validation of this assumption.

There are some remaining concerns, as discussed in more details in Section 4.1.2. Municipalities may be subject to different audit probabilities, and the perceived probability and salience of audits may change over time. We address both concerns by estimating a dynamic matching difference in difference model, where we compare audited municipalities to a contemporaneous control group of never audited municipalities subject to the same audit probability. In Table A11, we confirm that audits have a positive effect on economic activity, and the magnitudes (around 6%) are larger than our event study estimates, consistent with a deterrence effect of audits on control municipalities.[50]

[48]The specification is the following:

$$y_{imt} = \alpha_m + \alpha_t + \delta \times PostAudit_{mt} + \beta_1 \times CorruptFirm_i \times PostAudit_{mt} + \beta_2 \times FirmType_i \times PostAudit_{mt} + \epsilon_{imt} \tag{7.1}$$

where i, m, and t are establishment, municipality, and year, respectively. y_{imt} are the outcome variables of interest, namely log of employment and an indicator for firm exit. $FirmType_i$ is an indicator variable equal to 1 depending on the type of GD firm, and $CorruptFirm_i = 1$ if the local establishment belongs to a corrupt firm audited in municipality m. $PostAudit_{mt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The coefficients of interest are β_1 and β_2, which capture the effect of the anti-corruption audits on corrupt establishments and GD establishments, respectively, relative to other local incumbent establishments, conditional on municipality and year fixed effects α_m and α_t. ϵ_{imt} are standard errors clustered at the level of the audited municipality. To be consistent with the corrupt firm analysis, we restrict attention to firms alive in the audited municipality in each of the three years leading up to the audit.

[49]In particular, corrupt establishments are less likely to exit than establishments in retail and construction, and are less likely to exit and grow more than establishments in their same 5-digit industries. On the other hand, we find that establishments belonging to firms that participated in federal public procurement do slightly better than corrupt ones.

[50]We further investigate the deterrence effect of the audits by estimating the interacted version of equation 4.2, where we interact the treatment indicator with a continuous variable representing the ex-ante implied audit probability of the municipality in the previous three years. We report the results in Table A12. The

Additionally, we address the concern of direct spillover effects of the audits on control municipalities, as discussed in Section 4.1.2. Using the alternative difference in difference strategy, we restrict attention to a control group of municipalities that are far away from audited ones. Our results are robust to this specification, as reported in Table A15. In columns 1 and 2, we restrict attention to control municipalities in a different micro-region than any audited municipality in a window of [-1,1] years around the audit, while columns 3-6 are more conservative and focus on window of [-2,2] and [-3,3] years. We find the coefficients on both employment and establishments are positive and slightly larger, consistent with a downward bias due to direct spillover effects.

In terms of interpretation, in addition to reducing corruption, audits may have other consequences, which in turn account for the increased local economic activity. First, audits may increase the perceived risks for firms to remain informal, and thus generate a positive effect on economic outcomes due to formalization. We believe this channel is unlikely to explain the full effects, as the program does not target tax evasion, firms are not directly involved in the auditing process, and anecdotal evidence suggests there are no consequences for informal firms regarding their tax status. Additionally, the analysis on employment creation (Table A8) indicates that a small part of the effect comes form employment in existing firms located in nearby areas. Second, using the baseline event study specification 4.2, we study whether the increased economic activity is due to a change in the allocation of funds by the central government. In Table A7 we show that municipalities do not experience a change in federal transfers following the audits (column 7, 8, and 9). Then, in Table A14, we look at procurement contracts awarded by federal agencies, which may respond to the audits by channeling more or less resources to firms located in audited municipalities.[51] We do not find an impact of the audits on these margins neither, suggesting it is unlikely our findings are driven by a positive reaction by the federal government to the audits.

There are other possible interpretations for our findings, though the dynamics and persistence of the effects suggest it is unlikely these stories can account for the entirety of our findings. One possibility is that firms are uncertain about the *risks* of the audits, and this uncertainty vanishes after the audit takes place. Considering the small average implied audit probabilities (around 3% per year), it seems unlikely this type of uncertainty would play a major role for firms. Additionally, this uncertainty would take time to vanish, as firms slowly learn about the effects of the audits. Yet, a significant share of the effects we observe takes place in the first year. Finally, we observe in the data that there is hard punishment of corruption following the audit (e.g. firing and suspensions of public officials), which is inconsistent with this interpretation. A similar reasoning rules out a channel through which *firms*

negative coefficient on the interaction term says that places with higher ex-ante probability of audits –where deterrence effects are likely higher– experience a lower increase in economic activity after the audit. This suggests that the deterrence effect of the audit, while difficult to isolate, may also play an important role.

[51]We use three dependent variables: total volume of federal public procurement contracts awarded to local firms (column 1), total volume of federal contracts awarded based on competitive procedures (column 2), and total volume of federal contracts awarded based on discretionary procedures (column 3).

learn about corruption thanks to the audit, and decide to locate in a corrupt municipality because they consider it attractive from a business perspective.

7.3. **Additional Tests at Firm Level.** As discussed in Section 4.2.3, we cannot visually test the main identifying assumption of parallel pre-trends, because in our main empirical strategy we select the control group by matching on pre-trends. Matching on pre-trends is needed to create a valid counterfactual. This is because the audit coincides with the corrupt firm having a recent municipal government contract that generates growth (Ferraz et al., 2015), and hence corrupt firms are on a strongly positive growth path in the years leading up to the audit. Therefore, in Figure A3, we show that the positive results of the audits on corrupt firms hold also when constructing a synthetic control group of firms by matching only up to 2 years before the audit (Abadie and Gardeazabal, 2003, Abadie et al., 2010). As can be seen from the figure, this allows us to use the year before the audit as a validation period. Figure A3 shows that even though the matching stops at year -2, the effects only materialize after the audit, thus alleviating concerns of mechanical effects due to our matching estimator.[52]

We then address other concerns discussed earlier in Section 4.2.3. In Table A16, columns 2 and 3, we show that the results are robust to excluding firms with potentially on-going municipal contracts. The magnitudes of our findings for both log employment and exit probability are, if anything, slightly larger. Similarly, Table A16, columns 4 and 5, shows that the results are robust to focusing only on retail sector firms, for which contracts are completed immediately. Table A16, columns 6 and 7, reports robustness to only focusing on firms with a recent ongoing federal government contract. This test also aims to show that the impact of the audits is unrelated to corrupt firms potentially having more experience with government contracts relative to their control. The results hold for log employment, with a similar magnitude, even though they are insignificant for survivorship. Finally, we show that results hold even when considering a different control group of firms that are located far away from the audited municipality, to limit direct spillover effects of the audits. We report the results in Table A20, columns 1 and 2. We find that the effects are still positive and significant, and the magnitudes similar, when we re-estimate equation 4.4 using this different control group.

7.4. **A Survey of Government Dependent Firms in Brazil.** We provide additional qualitative evidence in support of our paper using firm-level surveys. In the summer of 2017, we conducted 115 face-to-face surveys with owners or managers of firms located in Brazil's southeastern state of Minas Gerais. We sampled firms from the pool of those doing business with 15 municipalities that were eligible for the randomized anti-corruption program. Firms and municipalities have similar characteristics to those in our analysis sample.

[52]We discuss the details of the synthetic control strategy in Appendix A.2. For the analysis in the paper we rely on the CEM strategy due to its flexibility in analyzing multiple dependent variables and heterogeneous effects.

We report basic summary statistics on the survey responses in Table A21. In the first panel, "Public Procurement," we ask a range of questions regarding the importance of government contracts for firm performance and other basic business information. The second panel, "Corruption I," includes questions on how corruption affects firm operations, resource allocation, and growth. The last panel, "Corruption II," asks various questions about bribes, corruption perceptions, and the importance of government anti-corruption activity. We report additional descriptive analysis in Figures A4 and A5.

While purely suggestive, this qualitative analysis helps motivate our study. First, it highlights the importance of corruption for firm activity, as a staggering 97% of firms say corruption affects the way they operate. Second, it sheds light on how corruption and bribery take place and on the pervasiveness of corruption in local public procurement. Finally, it provides clarifications on information levels among firms and suggests a benchmark for the magnitudes of our firm-level findings.

8. Conclusion

We exploit spatial variation in randomized anti-corruption audits related to government procurement contracts in Brazil to assess how corruption affects resource allocation, firm performance, and the local economy. After an anti-corruption crackdown, regions experience more entrepreneurship, improved access to finance, and higher levels of economic activity. Using firms involved in corrupt business with the municipality, we find that two channels explain these facts: allocation of resources to less efficient firms, and distortions in government dependent firms. The second channel dominates, as after the audits government dependent firms grow and reallocate resources within the organization.

Several studies have suggested corruption is detrimental to economic growth (Mauro, 1995, Kaufmann and Wei, 1999), and others have measured significant corruption costs to firms (Svensson, 2003, Olken and Barron, 2009, Sequeira and Djankov, 2014). However, our paper is among the first to provide causal estimates of the role of corruption for firms and the local economy. Importantly, despite our setting being the most favorable to them, our results are inconsistent with "grease the wheel" theories of corruption (Leff, 1964, Huntington, 2006, Dreher and Gassebner, 2013). Indeed, Brazil is ranked at the bottom of the world distribution in various measures of bureaucratic inefficiency and red tape (143/144 among countries surveyed in the Global Competitiveness Index (World Economic Forum, 2015)). Subsequently, our findings likely apply to other contexts where the positive effects of corruption have less bite; in particular, they can be valuable for most emerging economies characterized by a decentralized political system and imperfect anti-corruption enforcement.

Additionally, our paper has implications for various firm-level theories of corruption. We find that corruption mostly operate as a tax on firms, and that misallocation of resources towards inefficient firms exists but plays a secondary role in this context. This finding adds an additional wrinkle to the vast literature on political connections, suggesting that while

there may be favoritism, such relationships also impose higher transaction costs on connected firms. In particular, we shed light on various within-firm distortions associated with corruption, largely unexplored in the academic literature (Dal Bó and Rossi, 2007, Smith, 2016). Our findings emphasize the importance of corruption for various strategic choices by the firm, including investment and capital structure, as well as decisions to access new markets and invest in new products. A novel related empirical finding is that corruption in local environment seems to affect firms' choices of organizational design. This is a new motive that connects to the literature on boundaries of the firm (Holmström and Roberts, 1998, Rajan and Wulf, 2006, Roberts, 2007, Seru, 2014). Finally, many studies in the growing literature on misallocation hypothesize the presence of government-related distortions to firms (Hsieh and Klenow, 2009). We provide rich micro evidence for one specific friction: corruption in public procurement.

To conclude, this paper has immediate policy implications for both governments' and firms' anti-corruption policy around the world. For governments, it shows that it is important to consider the role of the private sector when designing policies aimed at reducing corruption in the public sector, as the spillover effects on the former can be significant. For firms, our findings highlight various nuances about how corruption affects firm performance and how firms operate in corrupt environments, and can be informative for the design of anti-bribery laws such as the US Foreign Corrupt Practices Act and the UK Bribery Act. Further research exploring other forms of corruption and how corruption affects large multi-national firms can help paint a more comprehensive picture needed to evaluate these and other international efforts to combat corruption.

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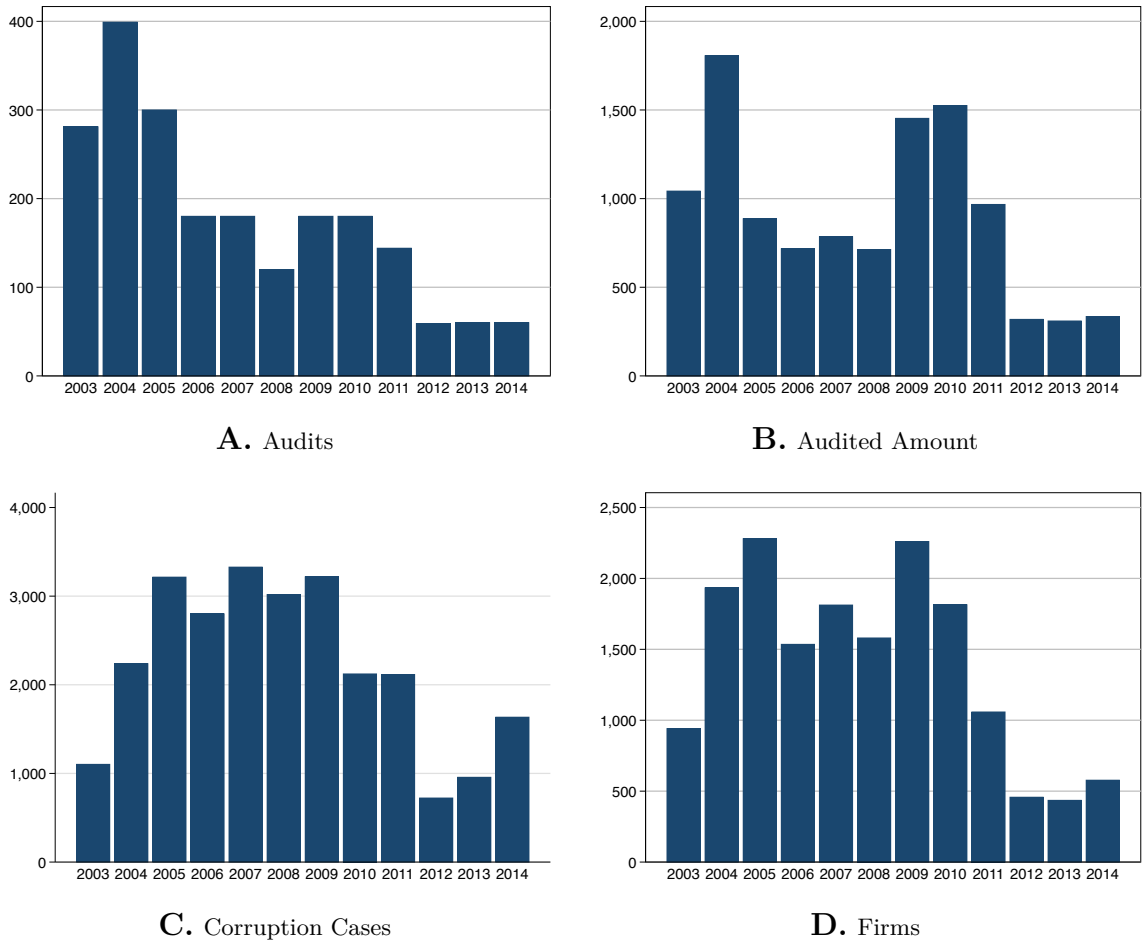
FIGURE 1. **The Anti-Corruption Program Over Time**

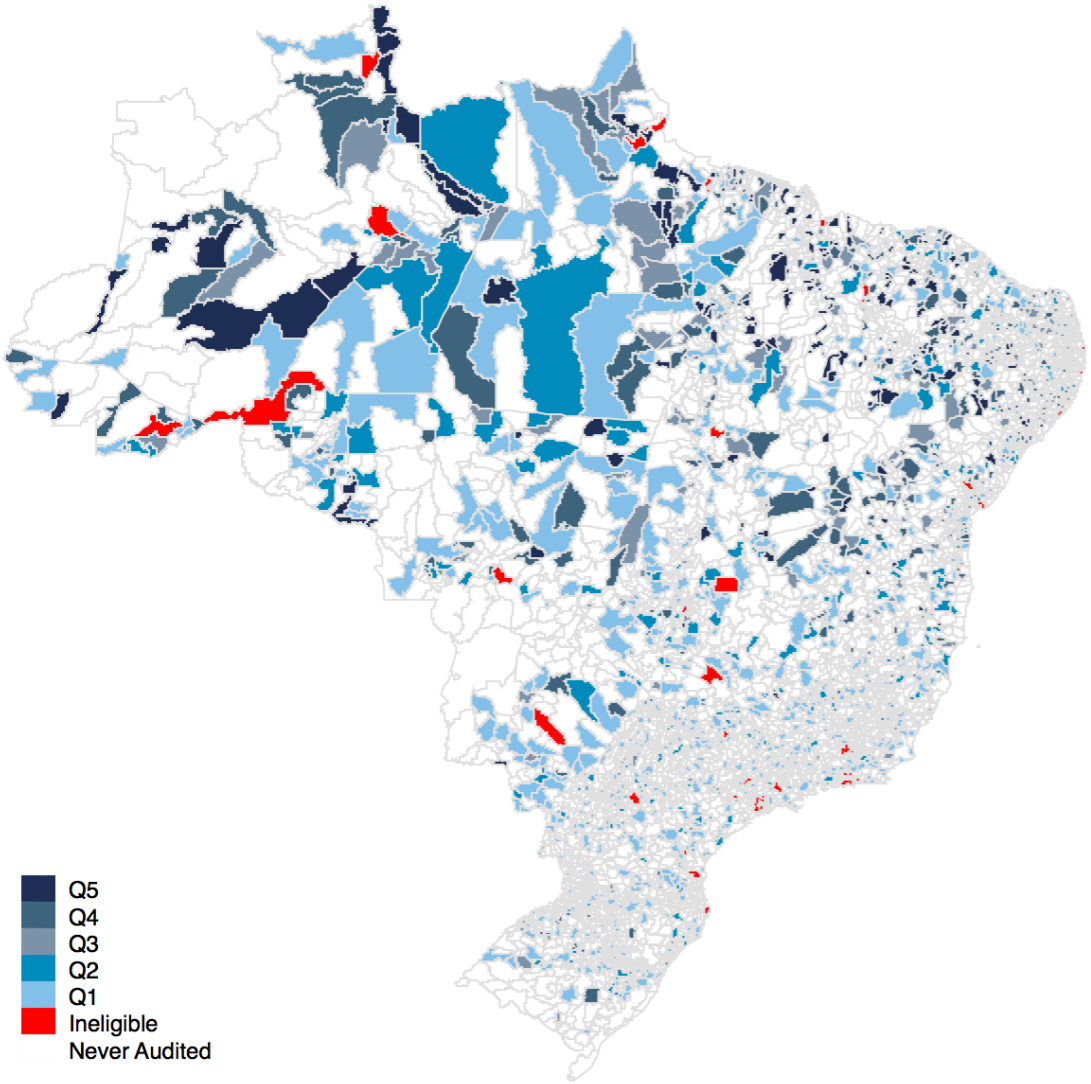


A. Audits **B.** Audited Amount

C. Corruption Cases **D.** Firms

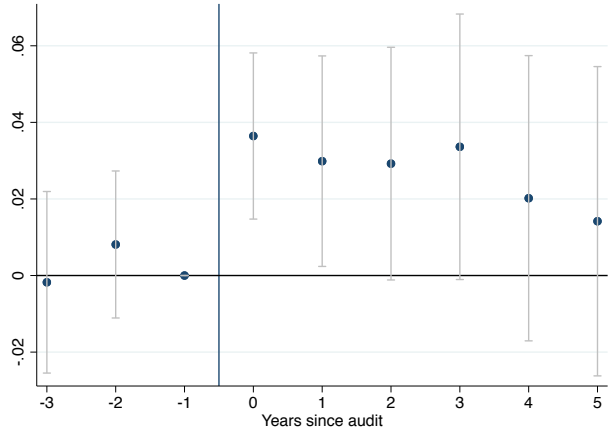
Notes: This figure illustrates the yearly variation in the anti-corruption program, using the data we extracted from the CGU anti-corruption audit reports. Panel A shows the total number of audits. Panel B shows the total amount of municipal resources audited, in real US dollars. Panel C shows the total number of irregularity cases in public procurement. Panel D shows the total number of firms involved in these irregularities.

FIGURE 2. **Corruption Across Brazilian Municipalities**

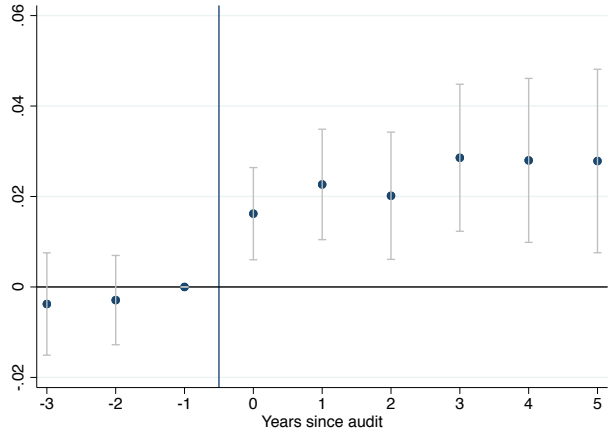


Notes: This figure shows a map of Brazilian municipalities and the spatial variation of audits and corruption intensity. In red we show the municipalities that are ineligible for the program, as discussed in Section 2. In white, we show the municipalities that are eligible for the program but have not been selected between 2003-2014. In shades of blue, we highlight the municipalities that have been audited as part of the CGU program between 2003-2014. A darker shade means that a higher percentage of the audited resources were found to be affected by irregularities in public procurement.

FIGURE 3. **Audits and Local Economy Outcomes: Point Estimates**



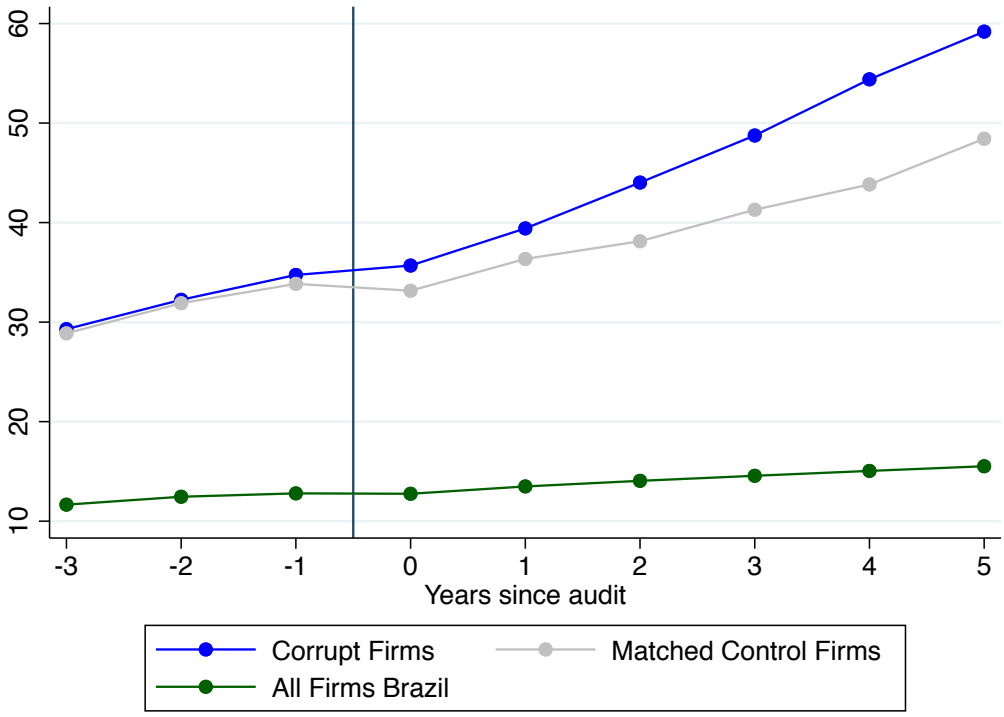
A. Ln(Employment)



B. Ln(Establishments)

Notes: This figure reports the dynamic coefficients obtained from the estimation of equation 4.1 together with 95% confidence intervals. The specification is $y_{mt} = \alpha_m + \alpha_t + \sum_{k=-3}^{k=-2} \mu_k + \sum_{k=0}^{k=5} \mu_k + \epsilon_{mt}$, and is discussed in Section 4.1. The sample covers the window [-3,+5] around the audit year. Panel A reports the coefficients on *Ln(Employment)* (log of the total number of private sector employees in the municipality). Panel A reports the coefficients on *Ln(Establishments)* (log of the total number of private sector establishments in the municipality).

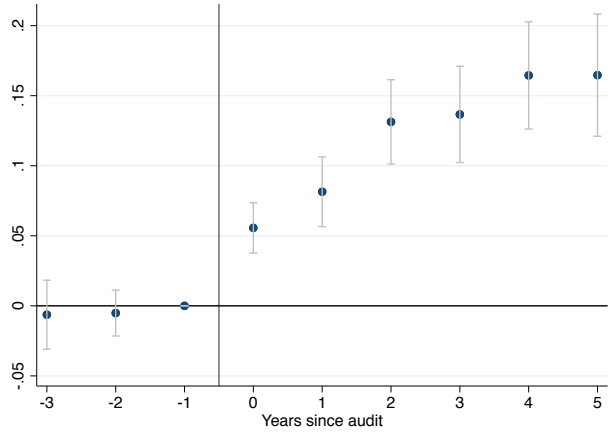
FIGURE 4. **Corrupt and Control Firms: Raw Data**



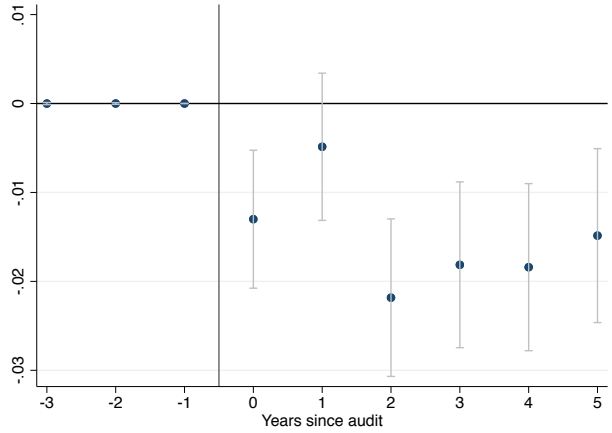
A. Employment

Notes: This figure reports the dynamics of the raw data on employment levels for the corrupt firms (blue line), the matched control firms (gray line), and all other firms in Brazil (green solid line). The first two groups are firms alive for all three years before the audit. The latter group is similarly selected to be the set of firms in the economy that have been alive for three consecutive years, in any given year. All groups are restricted to firms with less than 500 employees the year before the audit (blue and gray line) or at any point in time (green line). The sample covers the window [-3,+5] around the audit year.

FIGURE 5. **The Dynamic Impact of Audits on Corrupt Firms**



A. Ln(Employment)



B. Exit

Notes: This figure reports the dynamic coefficients obtained from the estimation of equation 4.6 together with 95% confidence intervals. The specification is $y_{jkt} = \alpha_j + \alpha_t + \sum_{k=-3}^{k=-2} \delta_k \times \mathbb{1}(Event_k) + \sum_{k=0}^{k=5} \delta_k \times \mathbb{1}(Event_k) + \sum_{k=-3}^{k=-2} \beta_k \times \mathbb{1}(Event_k) \times Audited_j + \sum_{k=0}^{k=5} \beta_k \times \mathbb{1}(Event_k) \times Audited_j + \epsilon_{jkt}$, and is discussed in Section 4.2. The sample covers the window [-3,+5] around the audit year. Panel A reports the coefficients on *Ln(Employment)* (log of the total number of private sector employees in the firm). Panel A reports the coefficients on *Ln(Establishments)* (log of the total number of private sector establishments in the firm). Panel B reports the coefficients on *Exit* (indicator for whether the firm exits the formal sector).

TABLE 1. Summary Statistics: Audits

	N	Mean	SD	p10	Median	p90
Panel A: By Year						
Rounds	12	3.25	2.09	1.00	3.00	7.00
Audits	12	178.58	104.83	60.00	180.00	300.00
Corruption Cases	12	2247.08	936.06	966.00	2257.50	3286.00
Firms	12	1470.08	726.93	473.00	1643.00	2371.00
Amount Audited (M USD)	12	821.79	374.62	323.42	830.25	1317.34
Share Irregular PP	12	0.18	0.05	0.15	0.18	0.22
Share Bad Performance	12	0.18	0.07	0.10	0.19	0.27
Panel B: By Round						
Audits	39	54.95	11.68	50.00	60.00	60.00
Corruption Cases	39	691.41	444.78	191.00	613.00	1391.00
Firms	39	452.33	228.26	134.00	473.00	754.00
Amount Audited (M USD)	39	252.86	105.62	149.04	217.81	405.98
Share Irregular PP	39	0.17	0.07	0.05	0.17	0.25
Share Bad Performance	39	0.22	0.14	0.10	0.19	0.39
Panel C: By Audit						
Corruption Cases	2,143	12.58	22.22	1.00	4.00	31.00
Firms	2,143	8.23	10.15	0.00	5.00	21.00
Amount Audited (M USD)	2,143	4.60	6.43	0.60	2.34	10.79
Share Irregular PP	2,143	0.19	0.42	0.00	0.07	0.50
Share Bad Performance	2,143	0.42	0.71	0.00	0.14	1.00
Panel D: By Case						
Firms	26,465	1.24	1.06	1.00	1.00	2.00
Irregular Amount (K USD)	26,464	33.52	93.22	0.08	2.36	80.07
Share Bad Performance	26,465	0.10	0.31	0.00	0.00	0.50
Share Passively Involved	26,465	0.07	0.24	0.00	0.00	0.00
Case Age	26,465	1.13	1.04	0.00	1.00	2.00

Notes: This table reports summary statistics for the CGU anti-corruption audit program for the period 2003-2014. The data is manually extracted fromt he CGU audit reports. Starting from data at the case-firm level, Panel A reports information at the *Year* level, Panel B at the *Round* level, Panel C at the *Audit* level, and Panel D at the *Case* level.

TABLE 2. Summary Statistics: Audited Municipalities

	Mean	SD	p10	Median	p90
N Business Establishments	212.13	502.24	6.00	49.00	529.00
Private Sector Workers	2,693.44	7,611.79	21.00	392.00	5,911.00
Avg Establishment Size	10.12	10.94	2.50	6.92	19.62
Avg Establishment Payroll	4,110.55	5,646.19	696.69	2,253.26	8,868.66
N New Business Establishments	31.92	72.75	1.00	9.00	78.00
N Exiting Business Establishments	20.04	48.81	0.00	5.00	47.00
Public Sector Workers	616.67	862.57	119.00	360.00	1,258.00
N Bank Branches	9.31	10.44	2.00	6.00	20.00
Total Credit (M USD)	21.83	55.11	0.36	6.63	50.45
Total BNDES Credit (M USD)	0.97	4.51	0.00	0.00	1.37
N Public Procurement Contracts	26.85	147.99	0.00	0.00	6.00
Percentage Construction	1.94	3.97	0.00	0.59	5.00
Percentage Retail	60.34	17.03	40.00	61.04	80.00
Percentage Manufacturing	14.46	11.98	0.00	12.12	30.00
Percentage Services	12.28	9.38	0.00	12.04	22.29
Percentage Other Sectors	12.92	13.63	0.00	9.39	27.27
GDP per capita	4,798.39	9,125.46	1,348.58	3,192.87	8,517.13

Notes: This table reports summary statistics for the municipality that are audited as part of the CGU anti-corruption program, focusing on the period 2003-2013 and on the 1,581 municipalities in the analysis sample, as discussed in Section 4.1. Summary statistics are computed using data in the window [-3, -1] before the audit. All value variables are winsorized at the 1% level of the empirical distribution and all variables are described in the text.

TABLE 3. Summary Statistics: Corrupt Firms

	Mean	SD	p10	Median	p90
Employment	32.09	66.34	2.00	10.00	79.00
Total Monthly Wages (USD)	15,365.84	51,077.06	489.17	2,901.18	32,778.79
Number of BNDES Loans	0.17	1.02	0.00	0.00	0.00
Amount of BNDES Loans (USD)	11,221.95	92,680.05	0.00	0.00	0.00
Interest rate on BNDES Loans	8.64	3.83	4.00	7.50	13.15
Federal contracts	2.53	10.57	0.00	0.00	1.00
Volume federal contracts (USD)	14,942.02	71,855.70	0.00	0.00	2,007.88
Share of firms w/ BNDES Loans	0.14	-	-	-	-
Share of firms w/ federal contracts	0.18	-	-	-	-
Managers and CEOs	1.73	5.19	0.00	0.00	5.00
White collar workers	12.71	28.31	0.00	4.00	29.00
Blue collar workers	16.67	43.13	0.00	3.00	41.00
Number of organizational layers	2.15	1.00	1.00	2.00	4.00

Notes: This table reports summary statistics for the firms that are involved in contracts that are audited as part of the program and that are deemed irregular by the CGU. This table focuses on the final sample of 5,742 firms used in the main analysis, as described in Section 4.2. The firm-level statistics come from the RAIS, the BNDES, and the federal procurement databases, and are computed using data in the window [-3, -1] before the audit. All value variables are winsorized at the 1% level of the empirical distribution and all variables are described in the text.

TABLE 4. The Impact of Audits on the Local Economy

	(1) Total Employment	(2) Total Establishments
PostAudit	0.026*	0.026***
	(0.014)	(0.007)
Observations	60,187	60,187
R-squared	0.951	0.976
Municipalities	4,695	4,695
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Avg Dep Var	5.927	4.013
SD Dep Var	2.179	1.642

Notes: This table illustrates the main effects of the audit on the local economy. The table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. *Ln(Employment)* is the log of the total number of private sector employees in the municipality. *Ln(Establishments)* is the log of the total number of private sector establishments in the municipality. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Corruption Frictions

	(1)	(2)	(3)	(4)
	Highly Corrupt Areas		Government Dependent Industries	
	Total Employment	Total Establishments	Total Employment	Total Establishments
Heter × PostAudit	0.163***	0.135***	0.156***	0.120***
	(0.027)	(0.013)	(0.019)	(0.009)
PostAudit	-0.051***	-0.038***	-0.061***	-0.043***
	(0.016)	(0.008)	(0.017)	(0.008)
Observations	60,187	60,187	120,374	120,374
R-squared	0.951	0.976	0.944	0.973
Municipalities	4,695	4,695	4,695	4,695
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Avg Dep Var	5.927	4.013	5.050	3.297
SD Dep Var	2.179	1.642	2.260	1.648

Notes: This table illustrates the heterogeneous effects of the audit on the local economy for highly corrupt areas and government dependent sectors. The table reports the coefficients obtained from the estimation of equation 4.3. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta_1 \times PostAudit_{mt} + \beta_2 \times Heter_m \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. $Heter_m$ is an indicator for *Highly Corrupt Areas* (columns 1 and 2) or *Government Dependent Industries* (columns 3 and 4). The former indicates municipalities that are audited and whose levels of corruption, measured by the total number of public procurement irregularities over number of establishments, is above the median of the empirical distribution of audited municipalities. The latter indicates retail and construction sectors. *Ln(Employment)* is the log of the total number of private sector employees in the municipality. *Ln(Establishments)* is the log of the total number of private sector establishments in the municipality. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6. Local Entrepreneurship and Bank Lending

	(1)	(2)	(3)	(4)	(5)
	Reallocation		Employment Creation		Access to Finance
	Ln New Establishments	Ln Exiting Establishments	Ln Employment New Firms	Ln Employment Incumbent Firms	Ln Lending Private Banks
PostAudit	0.018**	0.007	0.047***	0.021	0.038**
	(0.008)	(0.010)	(0.017)	(0.015)	(0.019)
Observations	60,187	60,187	60,187	60,187	41,151
R-squared	0.933	0.888	0.844	0.945	0.917
Municipalities	4,695	4,695	4,695	4,695	3,453
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	2.390	1.921	3.294	5.813	15.58
SD Dep Var	1.407	1.376	1.957	2.236	1.976

Notes: This table illustrates the effects of the audit on entrepreneurship, employment creation, and bank lending. The table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. *Ln New Establishments* is the log of the total number of private sector establishments appearing in the municipality for the first time. *Ln Exiting Establishments* is the log of the total number of private sector establishments exiting the municipality. *Ln Employment New Firms* is the log of the total number of private sector employees in new establishments. *Ln Employment Incumbent Firms* is the log of the total number of private sector employees in establishments operating in the municipality the year of the audit. *Ln Lending Private Banks* is the log of the total volume of loans from private banks to firms in the municipality (the data is available for a subset of the municipalities and years). All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7. Corruption as a Friction to Firm Performance

	(1)	(2)	(3)	(4)
	All		Manufacturing	
	Ln(Emp)	Exit	Ln(Sales)	Investment
Audited × PostAudit	0.117***	-0.015***	0.130*	0.050**
	(0.012)	(0.003)	(0.070)	(0.024)
PostAudit	-0.039***	0.046***	-0.047	-0.002
	(0.010)	(0.003)	(0.055)	(0.023)
Observations	90,655	90,655	1,520	1,520
R-squared	0.012	0.050	0.037	0.143
Firms	11,484	11,484	220	220
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Avg Dep Var.	2.389	0	14.94	0.129
SD Dep Var.	1.408	0	1.721	0.239

Notes: This table illustrates the main effects of the audit on the performance of corrupt firms. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. *Ln(Sales)* is the log of the total volume of sales by firms in the manufacturing Census (PIA). *Investment* is measured as total capital expenditures over total sales in the firm, and is also only available for firms in the manufacturing Census (PIA). The mean and standard deviations of the dependent variables are computed using data from the three years before the audit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 8. Heterogeneity by Type of Irregularity

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Emp)			Exit		
Irregularity type:	Corruption	Quality	Passive Involvement	Corruption	Quality	Passive Involvement
Type of Irregularity × Audited × PostAudit	0.005	-0.109*	0.049	-0.006	0.022*	0.001
	(0.030)	(0.063)	(0.038)	(0.006)	(0.012)	(0.007)
Audited × PostAudit	0.113***	0.123***	0.110***	-0.010*	-0.016***	-0.015***
	(0.027)	(0.012)	(0.013)	(0.005)	(0.003)	(0.003)
Type of Irregularity × PostAudit	-0.008	0.053	-0.012	-0.002	0.005	-0.000
	(0.024)	(0.043)	(0.029)	(0.005)	(0.008)	(0.006)
PostAudit	-0.033	-0.042***	-0.037***	0.047***	0.045***	0.046***
	(0.021)	(0.010)	(0.010)	(0.004)	(0.003)	(0.003)
Observations	90,655	90,655	90,655	90,655	90,655	90,655
R-squared	0.012	0.012	0.012	0.050	0.050	0.050
Firms	11,484	11,484	11,484	11,484	11,484	11,484
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.	2.326	2.642	2.591	0	0	0
SD Dep Var.	1.358	1.530	1.491	0	0	0

Notes: This table illustrates the heterogeneous effects of the audit on firm performance, depending on the type of irregularity the firm is involved in. The table reports the coefficients obtained from the estimation of equation 4.5. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta 1 \times PostAudit_{jt} \times Audited_j + \beta 2 \times PostAudit_{jt} \times Heter_j + \beta 3 \times PostAudit_{jt} \times Audited_j \times Heter_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. *Type of Irregularity* is an indicator for the type of firm involvement in the corruption case, where *Quality* captures irregularity cases with evidence of inefficient performance by the firm, *Passive Involvement* consists of cases where the firm is not directly involved in the irregular case (e.g. a losing bidder in an irregular procurement auction), and *Corruption* refers to all other cases. The sample covers the window [-3,+5] around the audit year. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. The mean and standard deviations of the dependent variables are computed using data from the three years before the audit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 9. Firm Borrowing and Leverage

	(1) N Loans	(2) Ln(Amount)	(3) Interest Rate
Audited × PostAudit	0.020***	0.047***	-0.089
	(0.003)	(0.008)	(0.202)
PostAudit	-0.008***	-0.028***	0.075
	(0.002)	(0.007)	(0.170)
Observations	90,655	90,655	9,568
R-squared	0.013	0.012	0.076
Firms	11,484	11,484	3,645
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Avg Dep Var.	0.0111	0.0540	8.572
SD Dep Var.	0.123	0.444	3.933

Notes: This table illustrates the effects of the audit on the borrowing and leverage of corrupt firms. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. *N Loans* is the total number of BNDES loans received by the firm. *Ln(Amount)* is the natural logarithm of the total value of BNDES loans obtained by the firm. *Interest Rate* is the average interest rate paid by the firm on all BNDES loans in a given year. The data come from the BNDES confidential database and is available for a subset of the municipalities and years. The mean and standard deviations of the dependent variables are computed using data from the three years before the audit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 10. The Internal Organization of the Firm

	(1) Ln(Pay/Emp)	(2) Churn	(3) N Layers	(4) Avg Ability
Audited × PostAudit	0.020***	3.943***	0.050***	0.010**
	(0.004)	(0.857)	(0.010)	(0.005)
PostAudit	0.002	-4.451***	0.006	-0.007*
	(0.003)	(0.723)	(0.009)	(0.004)
Observations	90,655	90,655	90,655	90,655
R-squared	0.250	0.018	0.013	0.001
Firms	11,484	11,484	11,484	11,484
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Avg Dep Var.	5.898	21.66	2.118	0.038
SD Dep Var.	0.443	53.88	1.002	0.443

Notes: This table illustrates the effects of the audit on the internal organization of resources and labor allocation within corrupt firms. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. *Ln(Pay/Emp)* is the log of the average monthly payroll accounted for by full time employees. *Churn* is the total number of full-time employees that are fired or hired. *N Layers* is the total number of hierarchical organizational layers in the firm. *Avg Ability* is a measure of worker ability obtained from the residuals of a Mincer regression, as described in 6. The mean and standard deviations of the dependent variables are computed using data from the three years before the audit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 11. Public Procurement

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N Contracts	Ln(Amount)	Preferential	Competitive	Bidding	Share Won
Audited × PostAudit	-0.131***	-0.066***	-0.028***	0.006*	0.008**	0.059***
	(0.025)	(0.008)	(0.003)	(0.003)	(0.004)	(0.018)
PostAudit	0.013	-0.000	0.011***	-0.008***	-0.006**	-0.060***
	(0.017)	(0.006)	(0.002)	(0.002)	(0.003)	(0.019)
Observations	90,655	90,655	90,655	90,655	74,058	5,935
R-squared	0.004	0.007	0.027	0.012	0.003	0.085
Firms	11,484	11,484	11,484	11,484	11,353	1,521
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.	0.229	0.104	0.0412	0.0515	0.0796	0.185
SD Dep Var.	2.208	0.724	0.199	0.221	0.271	0.266

Notes: This table illustrates the effects of the audit on the federal procurement activity of corrupt firms. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. *Ln(Amount)* is the natural logarithm of the total value of federal procurement contracts received by the firm. *Bidding* is the total number of unique online reverse price auctions a firm bid for using the Comprasnet system. *ShareWon* is the share of auctions won by the firm. Finally, in columns 6 and 7 we split the analysis into preferential (column 6) versus competitive (column 7) government contracts. These columns reports a dummy for having a contract of each type. We classify as preferential the "Convite" and "Tomada de Precos" tender mechanisms, which essentially either involve a small invitation of firms to bid, or require special conditions for firms to participate in the auction. On the other hand, we classify as competitive the online auctions for off-the-shelf goods ("Pregao") and the largest open invitation tender mechanisms ("Concorrencia"). *Audited* is a dummy equal to 1 for audited firms, thus indicating treatment status. The mean and standard deviations of the dependent variables are computed using data from the three years before the audit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

Appendix A.1. Conceptual Framework

We introduce corruption distortions in a standard monopolistic competition framework of heterogeneous firms. This stylized model is used to illustrate the intuition behind our empirical analysis by means of a set of testable predictions. For simplicity, we focus on the industry equilibrium in a closed economy and consider only the static one-period version of the model.

A.1. **Preferences.** For each sector $j \in \{0, 1, ..., J\}$, consumer preferences are given by:

$$U = \sum_{j=0}^{J} \beta_j log Q_j, \quad \sum_{j=0}^{J} \beta_j = 1, \beta_j \geq 0$$

Sector $j = 0$ is the numeraire, while in each of the other $j = 1, ..., J$ sectors there is a continuum of horizontally differentiated varieties. We assume Constant Elasticity of Substitution (CES) preferences (Dixit and Stiglitz, 1977), so that:

$$Q_j = \left[\int_{\omega \in \Omega} q_j(\omega)^{\frac{\sigma_j - 1}{\sigma_j}} d\omega \right]^{\frac{\sigma_j}{\sigma_j - 1}}, \quad \sigma_j > 1, j \geq 1$$

The demand for each differentiated variety within sector j is given by:

$$q_j(\omega) = (1 - \tau) A_j p_j(\omega)^{-\sigma_j}, \quad A_j = X_j P_j^{\sigma_j - 1}$$

where $X_j = \beta_j Y$ denotes the fraction of aggregate income (Y) consumers spend on goods from sector j, and P_j is the price index, with A_j thus representing an index of market demand taken as given by firms, which proportionally scales every firm's residual demand.[53]

The corruption parameter $\tau \in (-\infty, 1]$ introduces an important element of heterogeneity across firms. Some firms could have $\tau < 0$, thus receiving a subsidy on their demand. Other firms could have $\tau \in (0, 1]$, and be subject to a tax on their demand. The case of $\tau = 0$ is one with no corruption-induced heterogeneity across firms within a sector.

A.2. **Technology.** Firms in sector j produce varieties using a composite factor of production L_j, at unit cost w_j.[54] Within each industry, each firm chooses to supply a distinct horizontally differentiated variety. To produce, all firms need to incur a fixed cost f_j. In a world with corruption, firms need to pay an additional overhead cost, c_j. We can think of this cost as a bribe firms need to pay to obtain a license to operate. Importantly, we assume this cost is constant within sectors, but it can vary across sectors (for example because some government-dependent sectors , where interactions between firms and public officials are more common, are subject to higher bribes). Firms need also incur a constant marginal cost that is inversely proportional to firm productivity ψ. Hence, the total amount of input required for firms to produce q_j units of a variety is:

$$l_j = f_j + c_j + \frac{q_j}{\psi}$$

A.3. **Firm Behavior.** In equilibrium, firms maximize profits by choosing a price that is a constant mark-up over marginal cost. The first order condition (FOC) yields:

$$p_j(\psi) = \frac{\sigma_j}{\sigma_j - 1} \frac{w_j}{\psi}$$

---

[53] As a dual to Q_j, we have: $P_j = \left[\int_{\omega \in \Omega} p_j(\omega)^{1 - \sigma_j} d\omega \right]^{\frac{1}{1 - \sigma_j}}$

[54] Sector 0 is characterized by $w_0 = 1$.

Revenues are determined by the equilibrium conditions above, as follows:

$$r_j(\psi,\tau) = (1-\tau)A_j p_j(\psi)^{1-\sigma_j} = (1-\tau)A_j\left[\frac{\sigma_j-1}{\sigma_j}\right]^{\sigma_j-1} w_j^{1-\sigma_j}\psi^{\sigma_j-1}$$

As a result, firm profits in equilibrium are given by:

$$\pi_j(\psi,\tau) = \frac{r_j(\psi,\tau)}{\sigma_j} - w_j(f_j+c_j) = (1-\tau)B_j\psi^{\sigma_j-1} - w_j(f_j+c_j)$$

where $B_j = \frac{(\sigma_j-1)^{\sigma_j-1}}{\sigma_j^{\sigma_j}} w_j^{1-\sigma_j} A_j$.

In summary, there are two sources of heterogeneity across firms within a sector, namely productivity ψ and corruption distortion τ. All firms within a sector must also pay a corruption-specific overhead cost c_j.

A.4. **Firm Entry and Exit.** Firms can enter sector j by paying a sunk entry cost f_E. All potential entrants face uncertainty about both the productivity in the sector, and the corruption distortion they will face. After a firm pays f_E, it draws ψ and τ from fixed distributions $g(\psi)$ and $h(\tau)$, with respective cumulative distributions $G(\psi)$ and $H(\tau)$.

Once a firm observes its productivity and corruption draws, it decides whether to produce or to exit the sector. This decision is determined by a combination (ψ^*,τ^*) at which the firm has zero profits:

$$\pi_j(\psi^*,\tau^*) = \frac{r_j(\psi^*,\tau^*)}{\sigma_j} - w_j(f_j+c_j) = (1-\tau^*)B_j(\psi^*)^{\sigma_j-1} - w_j(f_j+c_j) = 0$$

This generates a simple productivity cutoff ψ^*, such that a firm makes positive profits if and only if:

$$\psi^* > \left[\frac{w_j(f_j+c_j)}{B_j}\right]^{\frac{1}{\sigma_j-1}} (1-\tau)^{\frac{1}{1-\sigma}}$$

From this equation, we can see that the higher the corruption tax, the higher the level of productivity needed to be profitable in the market.

The free entry condition implies that in equilibrium, the expected measure of ex-ante profits must be equal to zero when accounting for the entry cost, namely:

$$\int_{-\infty}^{1}\int_{\psi^*}^{\infty} \pi_j(\psi,\tau)dG(\psi)dH(\tau) = wf_E$$

Finally, in each period all firms face a probability δ of exogenous exit.

A.5. **Equilibrium.** We close the economy by fixing the number of workers $\bar{L}$, but allowing mobility of workers across sectors. Using wages determined by labor mobility and the productivity cutoff ψ^* determined by the zero profit condition, we obtain that aggregate income

With the zero profit condition that provides the productivity cut-off (ψ^*) and the wage determined by labor mobility, we obtain that aggregate income is equal to $Y = w\bar{L}$ and industry revenue is equal to $R_j = \beta_j Y = \beta_j w\bar{Y}$. It follows that the mass of firms in industry j is given by:

$$M_j = \frac{R_j}{\bar{r}_j} = \frac{B_j\bar{L}}{\sigma_j\left[\frac{f_{E_j}(c)}{1-G(\psi_j^*(\tau,c))} + f_j\right]}$$

where $\bar{r}_j$ is the average revenue of active firms derived from the free entry condition.

A.6. **The Audit Experiment.** We model the anti-corruption audit as a shock that eliminates the fixed overhead corruption cost c_j and the corruption tax τ, so that $c_j = \tau = 0$. That is, we assume that the audit is effective at reducing corruption, as shown in the data.

PROPOSITION **A.1.1.** ***[Government Dependent Firms]*** *Labor and revenue are decreasing in τ, while profits and revenue productivity are a decreasing in τ and c. This implies that for firms subject to a corruption*

tax ($\tau > 0$), labor, revenue, profits, and revenue productivity increase after the audit. For firms obtaining corruption subsidies ($\tau < 0$), after the audit labor and revenue decrease while the effect on profits and revenue productivity is ambiguous depending on the relative importance of τ and c_j. For all other firms ($\tau = 0$), after the audit, labor and revenue remain constant, while profits and revenue productivity increase.

Proof.

Firm-level predictions are immediately derived by taking the partial derivatives of labor ($l_j(\psi,\tau)$), revenue ($r_j(\psi,\tau)$), profits ($\pi_j(\psi,\tau,c)$), and revenue productivity ($\frac{r_j(\psi)}{l(\psi)}$) with respect to τ and c_j.

$$\begin{aligned}
\frac{\partial \pi_j(\psi,\tau,c)}{\partial \tau} &= -B_j \psi^{\sigma_j - 1} < 0 \\
\frac{\partial r_j(\psi,\tau)}{\partial \tau} &= -B_j \psi^{\sigma_j - 1} < 0 \\
\frac{\partial l_j(\psi,\tau)}{\partial \tau} &= -\frac{A_j p_j^{-\sigma_j}}{\psi} < 0 \\
\frac{\partial \frac{r_j(\psi)}{l(\psi)}}{\partial \tau} &= -\frac{w_j \sigma_j}{\sigma_j - 1}\left[1 - \frac{(f+c)}{l(\psi)}\right] < 0 \\
\frac{\partial \pi(\psi,\tau,c)}{\partial c} &= -w_j < 0 \\
\frac{\partial r_j(\psi,\tau)}{\partial c} &= 0 \\
\frac{\partial l_j(\psi,\tau)}{\partial c} &= 0 \\
\frac{\partial \frac{r_j(\psi)}{l(\psi)}}{\partial c} &= -\frac{w_j \sigma_j (1-\tau)}{l(\psi)(\sigma_j - 1)} < 0
\end{aligned}$$

■

PROPOSITION **A.1.2.** ***[Entry and Exit]*** *The productivity cut-off for entry, $\psi^*(\tau,c)$, is increasing in τ and c. This implies that after the audit, firms with $\tau > 0$ are more likely to enter, while for firms with $\tau < 0$ the effect is ambiguous depending on the relative importance of τ and c_j.*

Proof. We take derivatives of $\psi^*(\tau,c)$ with respect to τ and c:

$$\begin{aligned}
\frac{\partial \psi^*(\tau,c)}{\tau} &= \frac{1}{\sigma_j - 1}\left[\frac{w_j(f_j + c_j)}{B_j}\right]^{\frac{1}{\sigma_j - 1}} (1-\tau)^{\frac{\sigma_j}{1-\sigma_j}} > 0 \\
\frac{\partial \psi^*(\tau,c)}{c} &= \frac{1}{\sigma_j - 1}\frac{w_j}{B_j}\left[\frac{w_j(f_j + c_j)}{B_j}\right]^{\frac{-\sigma_j}{\sigma_j - 1}} (1-\tau)^{\frac{1}{1-\sigma_j}} > 0
\end{aligned}$$

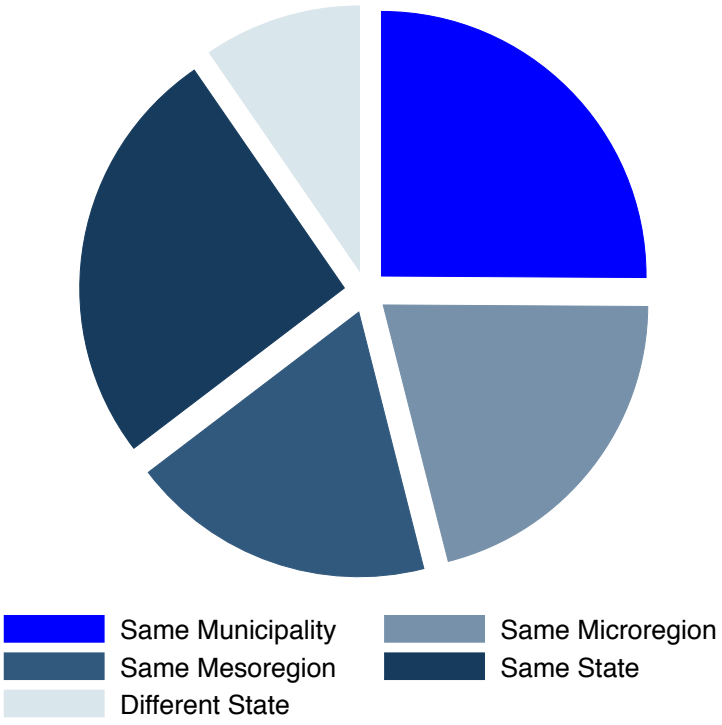
For firms with $\tau > 0$, the decrease in corruption taxes and c will lead to a decrease in $\psi^*(\tau,c)$. The probability of having a productivity level, ψ, above the entry cutoff ψ^* is measured by $1 - G(\psi^*)$. Since $G(\psi)$ is an increasing function in ψ, the reduction in ψ^* will lead to a higher probability of entry. For firms with $\tau < 0$, the decrease in corruption subsidies and c have an ambiguous effect on the productivity cutoff, depending on the relative importance of τ and c_j. If the reduction in corruption subsidies τ dominates the reduction in corruption overhead costs c_j, then $\psi^*(\tau,c)$ will increase, and subsequently the probability of entering and the probability of exiting increase for these firms (viceversa if the c_j effect dominates). ■

We visually illustrate the intuition of the model and some of the predictions in Figure A6. The three-dimensional figure has three axes: productivity(ψ), corruption tax (τ), and overhead costs (c). The dark blue plane represents the minimum productivity above which firms can profitably operate without corruption distortions τ and c. The rainbow plane shows instead the minimum productivity above which firms can

profitably operate in presence of such distortions. This implies that higher levels of distortions change the firms' performance and the compositions of firms that decide to enter the market.

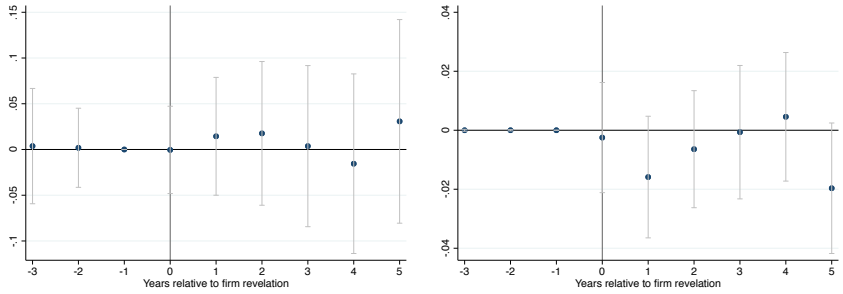
Appendix A.2. Figures and Tables

Figure A1. **Location of Firms Doing Municipal Public Procurement**



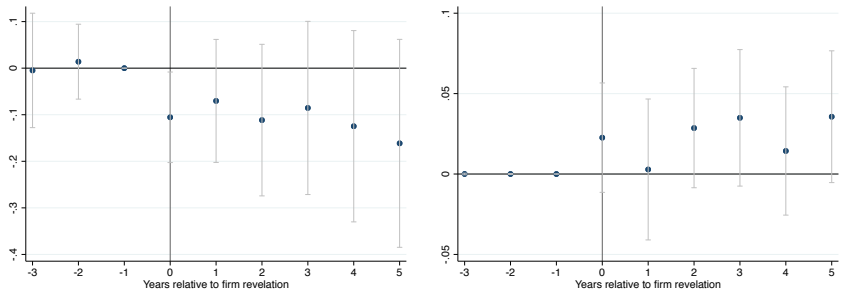
Notes: This figure illustrates the percentage of corrupt firms whose headquarter is located within certain geographical regions around the audited municipality they had contracts with. We report the shares of firms located in the same municipality, same microregion, same mesoregion, same state, and different state, the year before the audit.

FIGURE A2. **Dynamic Effect on Firms, Interaction w/Type of Irregularity**



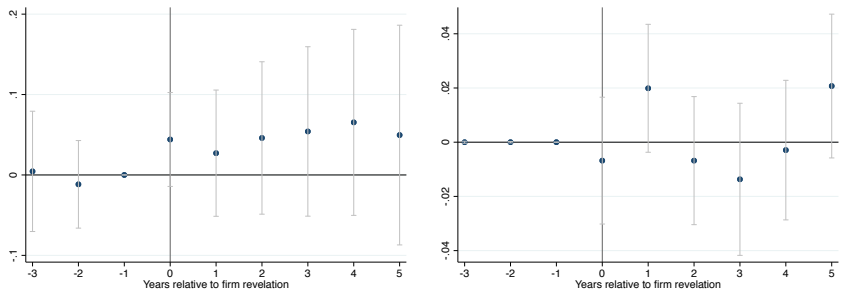
A. Ln(Emp) Corruption

B. Exit Corruption



C. Ln(Emp) Quality

D. Exit Quality

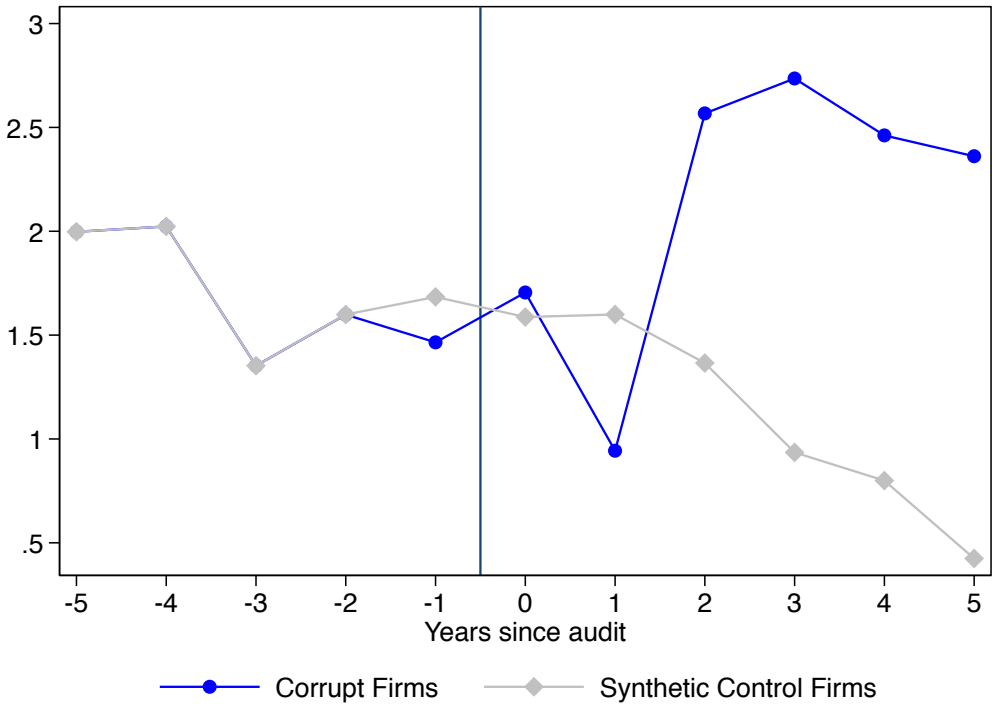


E. Ln(Emp) Passive

F. Exit Passive

Notes: This figure reports the dynamic coefficients obtained from the estimation of the interacted version of equation 4.6, together with 95% confidence intervals, as discussed in Section 4.2. *Quality* captures irregularity cases with evidence of inefficient performance by the firm, *Passive Involvement* consists of cases where the firm is not directly involved in the irregular case (e.g. a losing bidder in an irregular procurement auction), and *Corruption* refers to all other cases. The sample covers the window [-3,+5] around the audit year. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. The sample covers the window [-3,+5] around the audit year.

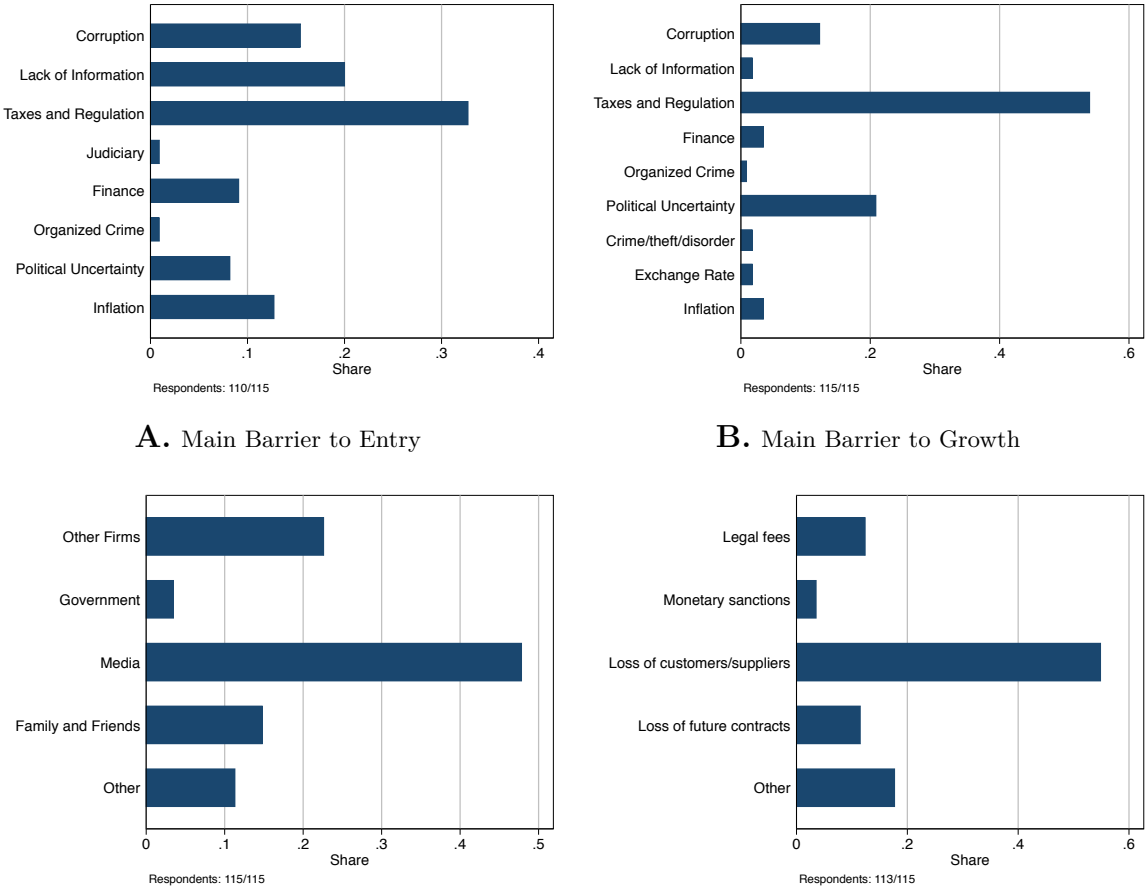
FIGURE A3. **Synthetic Controls**



A. Ln(Emp)

Notes: This figure reports the dynamics of log employment for corrupt firms (blue line) compared to a synthetic control group of firms (gray line), as discussed in Section A.2.

FIGURE A4. **Additional Survey Responses I**



A. Main Barrier to Entry

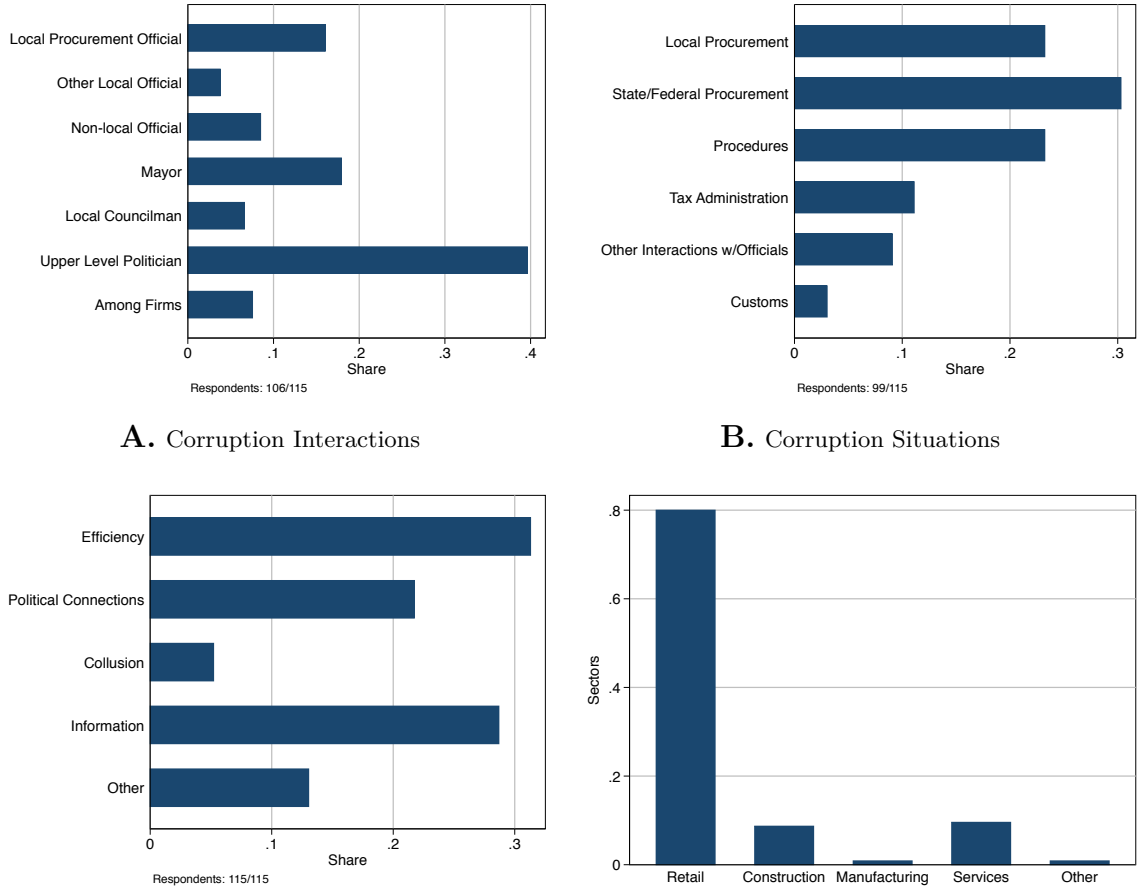
B. Main Barrier to Growth

C. Information About Markets

D. Perceived Risks of Corruption

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: "What information do you rely on to find out the main issues related to accessing a new market?". Panel D asks: "What type of costs would you be afraid of incurring, in the hypothetical case your firm were involved in a corruption irregularity?". All respondents are provided with a list of options to choose from.

FIGURE A5. **Additional Survey Responses II**



A. Corruption Interactions

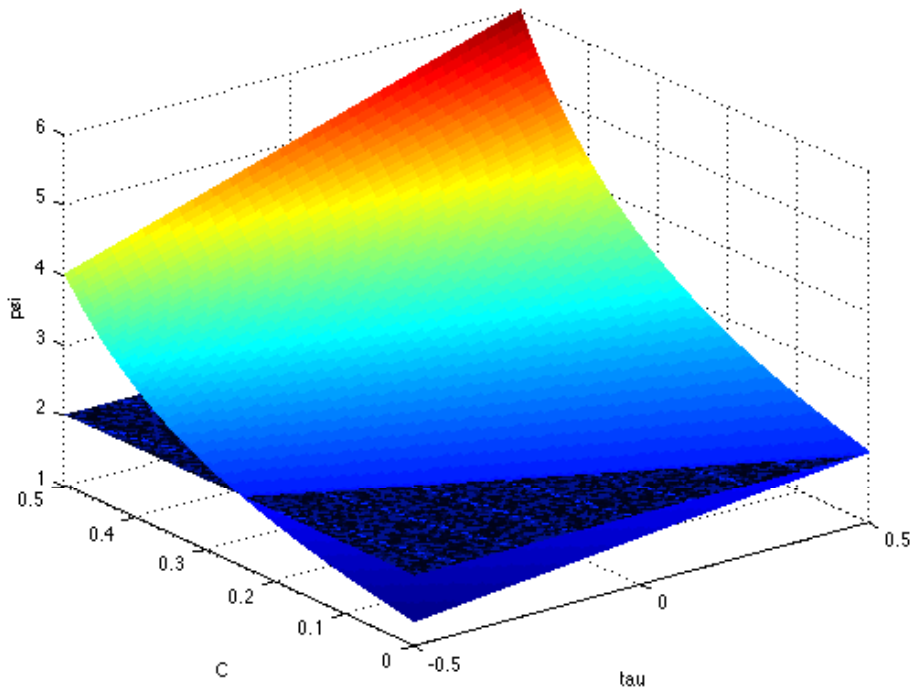
B. Corruption Situations

C. Main Factors to Win Government Contracts

D. Sector of Respondents

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: "At what level does corruption most commonly take place in your sector?". Panel B asks: "In what situation does corruption most commonly take place in your sector?". Panel C asks: "In your view, what are the most important factors to win a government contract?". Panel D reports the sector of the firms. All respondents are provided with a list of options to choose from..

FIGURE A6. **Simulated Corruption Distortions**



Notes: This figure illustrates the intuition of the model presented in Section A.1. The three-dimensional figure has three axes: productivity(ψ), corruption tax (τ), and overhead costs (c). The dark blue plane represents the minimum productivity above which firms can profitably operate without corruption distortions τ and c. The rainbow plane shows instead the minimum productivity above which firms can profitably operate in presence of such distortions. This implies that higher levels of distortions change the firms' performance and the compositions of firms that decide to enter the market.

TABLE A1. Audit Probability (%)

	N	Mean	SD	Min	p10	Median	p90	Max
Audit Probability within Round	215,514	0.99	0.55	0.00	0.47	0.93	1.45	7.14
Audit Probability within Year	66,312	3.23	2.46	0.00	0.82	2.52	6.03	42.86
Audit Probability across States	26	1.35	0.64	0.71	0.77	1.24	1.95	3.25

Notes: This table reports audit probabilities for the CGU anti-corruption audit program for the period 2003-2014. First row computes statistics based on all observations with round-level probabilities. Second row computes statistics after the aggregation of round-level probabilities at the year level. Third row computes statistics after the aggregation of round-level probabilities at the state level.

TABLE A2. Audits Reduce Corruption in Public Procurement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ln Number of Firms			Ln Number of Cases			Ln Number of Firms (Corruption)		
Panel A: Baseline Specification									
Past Audit	-0.140**	-0.161**	-0.606**	-0.142*	-0.168**	-0.640**	-0.143*	-0.162**	-0.778***
	(0.069)	(0.067)	(0.248)	(0.086)	(0.084)	(0.304)	(0.082)	(0.082)	(0.295)
Observations	2,081	2,081	472	2,081	2,081	472	2,081	2,081	472
R-squared	0.395	0.421	0.710	0.399	0.421	0.706	0.395	0.411	0.700
Panel B: High Corruption Heterogeneity									
Past Audit × HC in t-1	-0.100	-0.082	-0.880***	-0.141	-0.136	-1.237***	-0.205	-0.221	-1.228***
	(0.123)	(0.122)	(0.182)	(0.151)	(0.149)	(0.225)	(0.145)	(0.146)	(0.220)
Audited in the past	-0.085	-0.115	0.021	-0.063	-0.093	0.240	-0.029	-0.039	0.097
	(0.104)	(0.103)	(0.278)	(0.125)	(0.124)	(0.343)	(0.117)	(0.118)	(0.324)
Observations	2,081	2,081	472	2,081	2,081	472	2,081	2,081	472
R-squared	0.395	0.421	0.739	0.399	0.422	0.744	0.396	0.411	0.741
Municipality FE	No	No	Yes	No	No	Yes	No	No	Yes
State FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Robust standard errors in parentheses									
*** p<0.01, ** p<0.05, * p<0.1									

Notes: This table reports the estimation results of specification 2.1, and shows that audits are effective at reducing local private sector corruption. We use three main dependent variables measured in log: Number of unique firms reported in the audit report (columns 1,2,3), Number of irregularity cases uncover by the audit (columns 4,5,6), Number of unique firms involved in cases of Corruption reported by the audit (columns 7,8,9). *Past Audit* is a dummy equal to 1 if the municipality had been previously audited. *Plant in t-1* is a dummy equal to 1 for highly corrupt areas, measured by *HC = Total Number of Irregular Cases* (weighted by number of plants in the municipality to account for the fact that larger municipalities have more contracts audited and thus mechanically more irregularities). Controls include the total amount audited in the given audit and other variables measured for all municipalities in year 2000, and in log terms: population, GDP, number of plants, number of employees, average payroll per employee, number of firms and number of contracts in competitive and discretionary procurement. All variables are described in the main text. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A3. Audits Trigger a Restructuring of the Local Bureaucracy

	(1) Firing of Bureaucrats	(2) Hiring of Bureaucrats	(3) Churn of Bureaucrats
		Panel A: All	
PostAudit	0.054***	0.035*	0.042**
	(0.016)	(0.019)	(0.017)
R-squared	0.759	0.620	0.702
		Panel B: Highly Corrupt Areas	
HC × PostAudit	0.192***	0.076**	0.126***
	(0.030)	(0.037)	(0.032)
PostAudit	-0.037*	-0.001	-0.018
	(0.019)	(0.022)	(0.020)
R-squared	0.760	0.620	0.702
Observations	60,187	60,187	60,187
Municipalities	4,695	4,695	4,695
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Avg Dep Var	3.341	3.856	4.399
SD Dep Var	1.280	1.375	1.313

Notes: This table illustrates the effect of the audits on public sector employees, and particularly on firings, hirings, and churn of local bureaucrats. he table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. We focus on public sector employees in the RAIS database. *Firings* is the log total firings, *Hirings* is the log total hirings, and *Churn* is the log of the sum of firings and hirings. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A4. Randomization of Audits

	(1) Private Sector	(2) Public Sector	(3) Credit Market	(4) Public Procurement	(5) Other	(6) All
N Business Establishments	-0.002					-0.028
	(0.020)					(0.030)
Private Sector Workers	0.005					0.008
	(0.021)					(0.025)
Avg Establishment Size	0.012					0.012
	(0.015)					(0.016)
Avg Establishment Payroll	-0.002					-0.005
	(0.016)					(0.016)
HHI	-0.009					-0.007
	(0.008)					(0.008)
Share Small Plants	0.011					0.010
	(0.011)					(0.011)
Share Medium Plants	0.012					0.011
	(0.008)					(0.008)
Share in Construction	-0.005					-0.005
	(0.007)					(0.007)
Share in Retail	-0.002					-0.002
	(0.011)					(0.011)
Share in Services	-0.002					-0.003
	(0.008)					(0.008)
Share in Manufacturing	0.004					0.004
	(0.008)					(0.008)
Public Sector Workers		0.003				-0.015
		(0.007)				(0.019)
Public Sector Payroll		0.004				0.001
		(0.010)				(0.010)
N Bank Branches			0.015			0.021
			(0.010)			(0.018)
Firms w/BNDES Loans			-0.008			-0.006
			(0.014)			(0.015)
Total Credit			-0.013*			-0.007
			(0.007)			(0.009)
Total BNDES Credit			-0.009			-0.010
			(0.009)			(0.009)
Firms w/Public Procurement				0.000		0.001
				(0.012)		(0.013)
N Public Procurement Contracts				0.002		0.002
				(0.012)		(0.012)
Share Firms in Discretionary Proc.				0.018		0.016
				(0.021)		(0.022)
Share Discretionary Contracts				-0.009		-0.012
				(0.021)		(0.021)
Population					0.010	0.011
					(0.012)	(0.018)
GDP					-0.003	0.006
					(0.012)	(0.017)
Observations	5,526	5,526	5,526	5,526	5,526	5,526
R-squared	0.037	0.036	0.036	0.036	0.036	0.038
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table illustrates the effective randomization of the audits. The sample includes all eligible municipalities. The outcome variable is an indicator for whether the municipality is ever audited in the period 2003-2014. We use as regressors variables measured as averages in the years 2001-2002, and we standardize each of them by their mean and standard deviation. Each of the five columns only includes regressors that are specific to a particular set of variables. The sixth columns includes all regressors. All variables are described in the main text. All specifications include state fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Corruption Across Industries

	Share Corrupt Sample	Share Full Sample	Share (Emp W) Corrupt Sample	Share (Emp W) Full Sample
Panel A: Sectors				
Retail	62.96	51.93	49.49	28.05
Construction	15.87	3.38	18.94	8.10
Services	8.34	24.95	7.47	27.73
Low-Tech Manufacturing	7.01	11.16	8.57	19.43
Transportation/Utilities/Communications	2.94	5.35	5.76	7.38
High-Tech Manufacturing	1.66	1.70	6.34	4.88
Agriculture/Mining	0.97	1.32	0.66	3.06
Panel B: 5-Digit Industries (top 30 only)				
Construction of buildings	10.11	1.45	7.55	3.49
Retail of food products (minimarkets)	5.17	3.32	0.47	1.12
Retail of fuel	5.00	1.38	0.90	1.12
Retail of accessories for cars	4.82	3.09	2.43	1.28
Retail and repair of cars	4.2	2.88	1.88	1.25
Wholesaling of pharmaceutical products	3.99	0.14	1.04	0.20
Retail of newspapers and stationery	3.87	1.17	0.44	0.46
Retail of food products (supermarkets)	3.70	1.16	11.79	3.32
Retail trade of raw construction materials	3.35	2.61	0.99	1.33
Wholesale of motor vehicles	2.83	0.75	3.34	0.80
Retail of electrical equipment	2.57	1.82	0.48	0.79
Retail, other	2.53	2.73	0.57	1.08
Retail of computer supplies	2.15	1.18	0.35	0.48
Civil engineering works	1.69	0.22	4.50	0.7
Retail of furniture	2.92	3.01	1.95	1.26
Retail of recreational and sports goods	1.41	2.70	0.60	0.93
Construction of highways and railways	1.22	0.09	3.83	0.66
Wholesale of health supplies	1.18	0.09	0.26	0.06
Retail of bakery	0.93	1.86	0.11	0.99
Restaurants and food catering	0.90	5.78	0.13	3.73
Local transport services	0.87	0.38	0.70	1.77
Retail of medical and orthopedic supplies	0.87	0.12	0.15	0.04
Retail of clothing and accessories	0.84	5.98	2.69	2.38
Manufacture of construction materials	0.79	0.38	0.38	0.38
Construction scrapers	0.77	0.15	0.93	0.21
Maintenance of motor vehicles	0.77	1.71	0.12	0.62
Diagnostic and therapeutic health services	0.73	0.61	0.12	0.39
Retail of meat and fish products	0.60	1.13	0.06	0.38
Hospital care	0.56	0.23	0.43	0.81
Retail of audio and video equipment	0.56	0.49	3.53	0.40

Notes: This table illustrates the industry distribution of firms involved in irregularity cases, in comparison to the national distribution. Panel A assigns firms to sectors using the 7-sector classification by Dix-Carneiro (2014), while Panel B is based on the 5-digit National Classification of Economic Activities (CNAE) by Brazilian Statistical institute (IBGE). The "Share Corrupt Sample" column is computed on the sample of firms that are ever found to be involved in an irregularity case in the CGU audits. On the other hand, the "Share Full Sample" column refers to all private sector firms in the formal economy (i.e. in the RAIS dataset), and the shares are obtained using all firms that ever appeared in the data in the period 2000 to 2014. The *Emp W* columns 3 and 4 are analogous to columns 1 and 2 with the difference that they report employment shares.

Table A6. Municipalities Before Start of Anti-corruption Program

	Average		P-value	Average		P-value
	Eligible Not Audited	Audited		Low Corruption	High Corruption	
N Business Establishments	135.79	137.81	0.791	254.30	27.00	0.000
Private Sector Workers	1,551.39	1,603.09	0.593	2,966.32	306.32	0.000
Avg Establishment Size	8.68	9.10	0.132	10.48	7.79	0.000
Avg Establishment Payroll	3,400.19	3,556.95	0.267	4,365.85	2,787.48	0.000
N New Business Establishments	21.50	22.00	0.659	39.81	5.05	0.000
N Exiting Business Establishments	12.81	13.05	0.746	23.97	2.65	0.000
Public Sector Workers	441.84	480.53	0.082	711.46	260.85	0.000
N Bank Branches	6.38	6.30	0.783	10.56	2.24	0.000
Total Credit (M USD)	8.61	8.71	0.879	16.23	1.55	0.000
Total BNDES Credit (M USD)	0.19	0.19	0.845	0.35	0.03	0.000
N Public Procurement Contracts	13.72	18.02	0.132	36.38	0.56	0.000
Share Construction	0.02	0.02	0.934	0.02	0.03	0.065
Share Retail	0.55	0.55	0.929	0.57	0.54	0.003
Share Manufacturing	0.15	0.15	0.441	0.17	0.13	0.000
Share Services	0.11	0.11	0.812	0.14	0.09	0.000
Share Other Sectors	0.14	0.14	0.769	0.12	0.16	0.000
GDP per capita	4,513.73	4,384.70	0.356	5,708.91	3,125.06	0.000
Number of Municipalities	3,647	1,881	-	917	964	-

Notes: This table reports summary stats before the CGU anti-corruption program begins, for the municipality that are eligible to be audited. The first column reports averages for the municipalities that are eligible to be audited, but that have not been selected between 2003-2014. The second column reports averages for audited municipalities. All variables are measured in 2001-2002. The 4th column report averages for audited municipalities whose intensity of corruption was below median, while the 5th columns reports averages for the highly corrupt municipalities. We categorize the audits into High Corruption and Low Corruption ones, depending on the amount of private sector irregularities they uncover, which we measure by HC = *Total Number of Irregular Cases* (weighted by number of plants in the municipality to account for the fact that larger municipalities have more contracts audited and thus mechanically more irregularities). High Corruption municipalities are those with an above median ratio of this measure. Columns 3 and 6 reported the p-value for the differences in means between variables in the preceding two columns. All value variables are winsorized at the 1% level of the empirical distribution, and all variables are described in the main text.

TABLE A7. Changes in Municipal Budget After the Audits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ln(Expenditures)			Ln(Revenues)			Ln(Federal Transfers)		
	Total	Current	Capital	Total	Current	Capital	Total	Current	Capital
Audited × PostAudit	0.006	0.003	0.003	0.013***	0.016***	-0.049	0.004	0.009	-0.102
	(0.009)	(0.010)	(0.022)	(0.005)	(0.005)	(0.091)	(0.005)	(0.005)	(0.115)
Observations	55,356	55,356	55,356	51,900	51,902	51,694	49,833	51,890	49,836
R-squared	0.816	0.801	0.611	0.963	0.960	0.365	0.905	0.891	0.354
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	16.19	16.05	13.93	16.21	16.23	11.05	15.52	15.50	3.861
SD Dep Var	0.982	0.981	1.466	0.956	0.959	4.438	0.777	0.787	5.648

Notes: This table illustrates the effects of the audit on municipal budget items. The table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. *Ln(Expenditures)* is the log of the volume of expenditures by the municipality. *Ln(Revenues)* is the log of the volume of revenues to the municipality. *Ln(Federal Transfers)* is the log of the volume of transfers from the federal government to the municipality. Each category is split into Total, Current, and Capital amounts. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A8. Geography of Employment Creation

	(1) Ln Emp Same Micro Region	(2) Ln Emp Same Meso Region	(3) Ln Emp Same State	(4) Ln Emp Different State	(5) Ln Emp From Unemp/Informal
PostAudit	-0.007	0.004	0.006	-0.004	0.021
	(0.018)	(0.019)	(0.019)	(0.019)	(0.015)
Observations	60,153	60,153	60,153	60,153	60,153
R-squared	0.904	0.893	0.886	0.881	0.934
Municipalities	4,695	4,695	4,695	4,695	4,695
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	3.474	3.098	2.801	2.059	4.609
SD Dep Var	2.148	2.108	2.062	1.894	2.043

Notes: This table illustrates the effect of the audit on employment creation. The table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. We focus on public sector employees in the RAIS database. *Ln Emp* is the total number of private sector workers in the municipality, which is split into five mutually exclusive categories depending on where the employees are coming from. The employees can come from the same micro-region, same meso-region, same state, different state, or from the unemployment/informal sector. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A9. Access to Finance

	(1)	(2)	(3)	(4)
	Branches		Total Lending	
	Total Employment	Total Establishments	Total Employment	Total Establishments
Heter × PostAudit	0.226***	0.160***	0.141***	0.089***
	(0.024)	(0.012)	(0.025)	(0.012)
PostAudit	-0.102***	-0.065***	-0.067***	-0.033***
	(0.013)	(0.007)	(0.018)	(0.009)
Observations	60,187	60,187	60,187	60,187
R-squared	0.952	0.976	0.951	0.976
Municipalities	4,695	4,695	4,695	4,695
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Avg Dep Var	5.927	4.013	5.927	4.013
SD Dep Var	2.179	1.642	2.179	1.642

Notes: This table illustrates the heterogeneous effects of the audit on the local economy for areas with high or low levels of access to finance. The table reports the coefficients obtained from the estimation of equation 4.3. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta_1 \times PostAudit_{mt} + \beta_2 \times Heter_m \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. $Heter_m$ is an indicator for *Highly Access to Finance*. The first measure (columns 1 and 2) is based on the median split depending on whether municipalities have a high or low ratio of bank branches to business establishments. The second measure (columns 3 and 4) is based on the median split depending on whether municipalities have a high or low ratio of total lending volume from private banks to business establishments. *Ln(Employment)* is the log of the total number of private sector employees in the municipality. *Ln(Establishments)* is the log of the total number of private sector establishments in the municipality. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A10. The Impact of Audits on Government Dependent Incumbent Firms

	(1)	(2)	(3)	(4)	(5)	(6)
	Government Dependent Industries		Corrupt Sectors		Federal Government Providers	
	Exit	Ln(Employment)	Exit	Ln(Employment)	Exit	Ln(Employment)
Corrupt Firm × PostAudit	-0.034***	0.083***	-0.039***	0.045***	-0.038***	0.055***
	(0.002)	(0.011)	(0.002)	(0.011)	(0.002)	(0.010)
Firm Type × PostAudit	0.006***	0.051***	0.004**	0.023***	-0.063***	0.173***
	(0.001)	(0.005)	(0.002)	(0.006)	(0.002)	(0.016)
PostAudit	0.061***	-0.056***	0.064***	-0.028***	0.065***	-0.028***
	(0.001)	(0.004)	(0.001)	(0.003)	(0.001)	(0.003)
P-value β_1 vs β_2	0.00	0.267	0.000	0.000	0.000	0.265
R-squared	0.203	0.861	0.202	0.861	0.203	0.861
Observations	1,852,499	1,852,499	1,852,499	1,852,499	1,852,499	1,852,499
Firms	236,372	236,372	236,372	236,372	236,372	236,372
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	-	1.649	-	1.649	-	1.649
SD Dep Var	-	1.194	-	1.194	-	1.194

Notes: This table illustrates the effect of the audits on local establishments operating in the municipality in each of the three years before the audit. The sample covers the window [-3,+5] around the audit year. *Corrupt Firm* is an indicator taking value 1 for establishment belonging to a corrupt firm. $FirmType_i$ is an indicator variable equal to 1 depending on the type of GD firm. *Government Dependent Industry* indicate establishments in the retail and construction sector. *Corrupt Sector* include establishments operating in a corrupt 5-digit sector, namely a sector including at least one corrupt firm in the audited municipality. *Federal Government Provider* are establishments belonging to firms that have won at least a federal government contracts in the three years prior to the audit. *P-value* β_1 *vs* β_2 is the p-value for the difference of the coefficients on *Corrupt Firm* and *Firm Type*. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A11. The Impact of Audits on the Local Economy - Dynamic Diff-in-Diff

	(1) Total Employment	(2) Total Establishments
Audited × PostAudit	0.062***	0.060***
	(0.015)	(0.007)
PostAudit	0.033***	0.017***
	(0.002)	(0.001)
Observations	4,748,630	4,748,630
R-squared	0.304	0.596
Municipality IDs	546,897	546,897
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Avg Dep Var	6.157	4.234
SD Dep Var	2.111	1.610

Notes: This table illustrates the effect of the audit on the local economy using the alternative dynamic difference in difference specification. The table reports the coefficients obtained from the estimation of equation A1. The specification is $y_{mt} = \alpha_m + \alpha_t + \delta \times PostAudit_{mt} + \beta \times PostAudit_{mt} \times Audited_m + \epsilon_{mt}$, and is discussed in Section A.1. The sample includes all municipalities audited in the period 2003-2013 and a contemporaneous control group of eligible never audited municipalities that are repeated multiple times as controls. The sample covers the window [-3,+5] around the audit year. $Audited_m$ is an indicator variable for audited municipality m, thus capturing treatment status. $PostAudit_{mt}$ is an indicator variable for both audited and control municipalities for the year of the audit and all years after. *Ln(Employment)* is the log of the total number of private sector employees in the municipality. *Ln(Establishments)* is the log of the total number of private sector establishments in the municipality. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A12. Deterrence Effect

	(1) Total Employment	(2) Total Establishments
AuditProb × PostAudit	-1.313***	-1.043***
	(0.397)	(0.201)
PostAudit	0.063***	0.055***
	(0.019)	(0.010)
Observations	60,187	60,187
R-squared	0.951	0.976
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Avg Dep Var	5.927	4.013
SD Dep Var	2.179	1.642

Notes: This table illustrates the heterogeneous effects of the audit on the local economy depending on the ex-ante differences in implied audit probability. The table reports the coefficients obtained from the estimation of equation 4.3. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta_1 \times PostAudit_{mt} + \beta_2 \times AuditProb_m t \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. $AuditProb_m t$ is a continuous variable that measures the average implied probability of the municipality using the last three years of the program (if available). *Ln(Employment)* is the log of the total number of private sector employees in the municipality. *Ln(Establishments)* is the log of the total number of private sector establishments in the municipality. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A13. Aggregate Robustness: Different Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Excluding 2003-2004 Audits		Excluding 2010-2014 Audits		Only Audited Municipalities		No Sample Restrictions		Single Audit Municipalities	
	Total Employment	Total Establishments	Total Employment	Total Establishments	Total Employment	Total Establishments	Total Employment	Total Establishments	Total Employment	Total Establishments
PostAudit	0.046***	0.036***	0.017	0.023***	0.058***	0.027***	0.040***	0.032***	0.022	0.022***
	(0.017)	(0.008)	(0.016)	(0.008)	(0.013)	(0.006)	(0.015)	(0.008)	(0.015)	(0.008)
Observations	54,878	54,878	58,343	58,343	13,677	13,677	71,422	71,422	58,335	58,335
R-squared	0.950	0.975	0.951	0.976	0.972	0.989	0.952	0.975	0.951	0.976
Municipalities	4,695	4,695	4,432	4,432	1,581	1,581	5,568	5,568	4,488	4,488
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	5.927	4.013	5.888	3.972	5.927	4.013	5.507	3.713	5.925	4.019
SD Dep Var	2.179	1.642	2.220	1.671	2.179	1.642	2.497	1.836	2.150	1.621

Notes: This table illustrates the robustness of main effects of the audit on the local economy to different estimation samples. The table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample covers the window [-3,+5] around the audit year. Columns 1 and 2 exclude municipalities audited in 2003 and 2004. Columns 3 and 4 exclude municipalities audited in 2010-2014. Columns 5 and 6 exclude never audited municipalities. Columns 7 and 8 includes the full sample of eligible municipalities where no sample restrictions are imposed. Columns 7 and 8 includes only municipalities audited once. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. *Ln(Employment)* is the log of the total number of private sector employees in the municipality. *Ln(Establishments)* is the log of the total number of private sector establishments in the municipality. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A14. Federal Public Porcurement

	(1) Total Value Government Contracts	(2) Total Value Competitive Contracts	(3) Total Value Discretionary Contracts
PostAudit	0.051	0.032	-0.019
	(0.066)	(0.071)	(0.053)
Observations	60,187	60,187	60,187
R-squared	0.675	0.630	0.538
Municipalities	4,695	4,695	4,695
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Avg Dep Var	1.944	1.338	1.292
SD Dep Var	4.496	3.901	3.592

Notes: This table illustrates the effects of the audit on government lending to local firms. The table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2013 and all eligible non-audited municipalities and covers the window [-3,+5] around the audit year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A15. Aggregate Robustness: Direct Spillovers Dynamic Diff-in-Diff

	(1)	(2)	(3)	(4)	(5)	(6)
	Window: [-1,1]		Window: [-2,2]		Window: [-3,3]	
	Total Employment	Total Establishments	Total Employment	Total Establishments	Total Employment	Total Establishments
Audited × PostAudit	0.067***	0.066***	0.065***	0.067***	0.071***	0.070***
	(0.017)	(0.008)	(0.019)	(0.010)	0.030***	0.015***
PostAudit	0.031***	0.013***	0.030***	0.015***	0.032***	0.017***
	(0.004)	(0.002)	(0.005)	(0.002)	(0.005)	(0.003)
Observations	1,729,343	1,729,343	1,135,121	1,135,121	848,612	848,612
R-squared	0.978	0.992	0.979	0.992	0.980	0.992
Municipality IDs	205,433	205,433	136,792	136,792	102,905	102,905
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	6.342	4.376	6.429	4.438	6.516	4.499
SD Dep Var	2.151	1.644	2.184	1.667	2.213	1.692

Notes: This table illustrates the effect of the audit on the local economy using the alternative dynamic difference in difference specification. The table reports the coefficients obtained from the estimation of equation A1. The specification is $y_{mt} = \alpha_m + \alpha_t + \delta \times PostAudit_{mt} + \beta \times PostAudit_{mt} \times Audited_m + \epsilon_{mt}$, and is discussed in Section A.1. The sample includes municipalities audited in the period 2003-2013 and a contemporaneous control group of eligible never audited municipalities that are repeated multiple times as controls. The sample covers the window [-3,+5] around the audit year. For each audit year, we exclude as controls municipalities that are in the same micro-region of any audited municipality in a symmetric window around the given year. We use a 3-year window in columns 1 and 2, a 5-year window in columns 3 and 4, and a 7-year window in columns 5 and 6. $Audited_m$ is an indicator variable for audited municipality m, thus capturing treatment status. $PostAudit_{mt}$ is an indicator variable for both audited and control municipalities for the year of the audit and all years after. *Ln(Employment)* is the log of the total number of private sector employees in the municipality. *Ln(Establishments)* is the log of the total number of private sector establishments in the municipality. All specifications include municipality and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A16. Firms Robustness: Compositional Changes and On-going Contracts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Balanced Panel	Excluding On-going Contracts		Retail Sector		Firms with On-going Federal Contracts	
	Ln(Emp)	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
Audited × PostAudit	0.117***	0.156***	-0.027***	0.110***	-0.015***	0.108**	-0.005
	(0.016)	(0.023)	(0.005)	(0.012)	(0.003)	(0.043)	(0.007)
PostAudit	0.052***	-0.038**	0.049***	0.110***	-0.015***	-0.007	0.028***
	(0.013)	(0.019)	(0.006)	(0.010)	(0.003)	(0.039)	(0.007)
Observations	44,352	20,476	20,476	58,157	58,157	13,021	13,021
R-squared	0.040	0.020	0.051	0.020	0.047	0.029	0.034
Firms	4,928	2,590	2,590	7,770	7,770	1,615	1,615
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.	2.686	2.308	0	2.158	0	3.219	0
SD Dep Var.	1.400	1.372	0	1.286	0	1.501	0

Notes: This table illustrates the robustness of the main effects of the audit on the performance of corrupt firms to compositional changes and on-going contracts. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. We apply various sample restrictions to the baseline specification. In Column 1 the sample is restricted to the set of firms alive for during the entire window [-3,5] around the audit. Columns 2 and 3 exclude firms (and respective controls) whose audited contract's data is the same year of the audit. Columns 4 and 5 restricts the sample to firms in the Retail sector. Columns 6 and 7 focus on firms who won a federal government contract in the three years before the audit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A17. Firms Robustness: Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Age of Firm FE		Municipality-Year and Sector-Year FEs		Winsorization at 5%		Weighting by Firm Employment		Unrestricted Window Around Audit	
	Ln(Emp)	Exit	Ln(Emp)	Exit	Emp	Ln(Emp)	Ln(Emp)	Exit	Ln(Emp)	Exit
Audited × PostAudit	0.115***	-0.015***	0.116***	-0.015***	3.368***	0.113***	0.122***	-0.015***	0.130***	-0.014***
	(0.012)	(0.003)	(0.012)	(0.003)	(0.550)	(0.012)	(0.035)	(0.003)	(0.013)	(0.002)
PostAudit	-0.062***	0.039***	-0.049*	0.033***	-1.766***	-0.032***	0.142***	0.014***	-0.026**	0.048***
	(0.010)	(0.003)	(0.029)	(0.008)	(0.428)	(0.010)	(0.024)	(0.003)	(0.012)	(0.002)
Observations	90,655	90,655	90,128	90,128	90,655	90,655	90,655	90,655	106,741	106,741
R-squared	0.879	0.199	0.897	0.293	0.020	0.010	0.023	0.027	0.009	0.051
Number of firms	11,484	11,484	11,459	11,459	11,484	11,484	11,484	11,484	11,484	
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.	2.389	-	2.388	-	30.10	2.385	2.389	-	2.389	-
SD Dep Var.	1.408	-	1.405	-	54.62	1.396	1.408	-	1.408	-

Notes: This table illustrates the robustness of the main effects of the audit on the performance of corrupt firms to alternative specifications of equation 4.4. In columns (1) and (2) we add age of the firm fixed effects. In columns (3) and (4) we add municipality-year and sector-year fixed effects. In columns (5) and (6) we use as dependent variables both levels and log of employment level, each winsorized at 5% of the empirical distribution. In columns (7) and (8) we weight the regressions by the level of employment the year before the audit. In columns (9) and (10) we report the baseline results using an unrestricted window around the audit. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A18. Firm-Level Robustness: Multi-Plant Vs Single-Plant Firms

	(1)	(2)	(3)	(4)
	Multi-Plants Firms		Single-Plant Firms	
	Ln(Emp)	Exit	Ln(Emp)	Exit
Audited × PostAudit	0.191***	-0.029***	0.104***	-0.012***
	(0.031)	(0.005)	(0.013)	(0.003)
PostAudit	-0.003	0.024***	-0.045***	0.049***
	(0.023)	(0.005)	(0.011)	(0.003)
Observations	13,353	13,353	77,302	77,302
R-squared	0.019	0.029	0.011	0.054
Number of firms	1,614	1,614	9,870	9,870
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Avg Dep Var.	3.689	-	2.176	-
SD Dep Var.	1.294	-	1.308	-

Notes: This table illustrates the robustness of the main effects of the audit on the performance of corrupt firms splitting the sample into multi-plant vs single-plant firms. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A19. Firm-Level Robustness: Matching Rounds

	(1)	(2)	(3)	(4)	(5)	(6)
	Round 1		Round 2		Round 3	
	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
Audited × PostAudit	0.099***	-0.015**	0.094***	-0.010**	0.128***	-0.016***
	(0.029)	(0.007)	(0.028)	(0.005)	(0.014)	(0.003)
PostAudit	-0.025	0.071***	-0.024	0.033***	-0.045***	0.044***
	(0.025)	(0.008)	(0.022)	(0.005)	(0.012)	(0.003)
Observations	13,191	13,191	19,465	19,465	57,999	57,999
R-squared	0.034	0.063	0.008	0.039	0.013	0.052
Number of firms	1,703	1,703	2,424	2,424	7,357	7,357
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.	1.622	0	3.176	0	2.308	0
SD Dep Var.	1.815	0	1.728	0	1	0

Notes: This table illustrates the robustness of the main effects of the audit on the performance of corrupt firms splitting the sample by matching round. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. Matching round 1 is the most restrictive, and matching round 3 is the least restrictive. The details are discussed in Section 4.2. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A20. Firm-Level Robustness to Different Control Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Different State		Same 2-Digit Sector		Same 4-Digit Sector	
	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
Audited × PostAudit	0.114***	-0.014***	0.125***	-0.016***	0.106***	-0.020***
	(0.012)	(0.003)	(0.012)	(0.003)	(0.013)	(0.003)
PostAudit	-0.050***	0.047***	-0.050***	0.047***	-0.043***	0.049***
	(0.010)	(0.003)	(0.010)	(0.003)	(0.012)	(0.003)
Observations	89,716	89,716	83,276	83,276	68,463	68,463
R-squared	0.012	0.050	0.012	0.051	0.012	0.052
Firms	11,369	11,369	10,575	10,575	8,695	8,695
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var.	2.390	0	2.324	0	2.361	0
SD Dep Var.	1.403	0	1.355	0	1.294	0

Notes: This table illustrates the robustness of the main effects of the audit on the performance of corrupt firms, by focusing on firms that are far away from any audited municipality and on firms in the same narrower sector of the corrupt firm. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{jt} = \alpha_j + \alpha_t + \delta \times PostAudit_{jt} + \beta \times PostAudit_{jt} \times Audited_j + \epsilon_{jt}$, and is discussed in Section 4.2. $Audited_j$ is an indicator variable equal to 1 for corrupt firms. $PostAudit_{jt}$ is an indicator variable for both treated and control firms for the year of the audit and all years after. The sample covers the window [-3,+5] around the audit year. Columns 1 and 2 include as controls firms located in never audited municipalities that are in a different microregion than any audited municipality, for each given year. *Ln(Emp)* is the log of the total number of full time employees in the firm. *Exit* is an indicator of firm exit. Matching round 1 is the most restrictive, and matching round 3 is the least restrictive. The details are discussed in Section 4.2. All specifications include firm and year fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.1.

Table A21. Firm-Level Survey

Question	Share	Responses	Don't Know
Public Procurement			
Do you do business with the municipality you operate in?	0.97	115	0
Do you do business with a nearby municipality?	0.30	115	0
Do you do business with other public agencies (e.g. the state, the federal government, other public entities)?	0.09	115	0
What percentage of your sales are accounted for by public procurement contracts?	7.22	105	10
What percentage of government contracts lasts less than one year?	94	114	1
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
Is the market for public procurement contracts in your industry competitive?	0.75	115	0
What percentage of your workforce is represented by full time employees?	99.5	112	3
On average, would it take more than one year to fire a full time employee?	0.95	114	1
Do you regularly do a restructuring of the organization (hiring or firing employees, re-allocating jobs and tasks, promotions, etc)?	0.44	115	0
Corruption I			
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 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
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 decisions to expand to new markets and products?	0.77	115	0
Does the presence of corruption affect cash holdings and allocation of financial resources within the firm?	0.79	115	0
In the absence of corruption, do you think your firm would be able to grow more than 10%?	0.65	112	3
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
Corruption II			
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?	53.27	47	68
What do you think is the percentage of local public procurement contracts affected by corruption?	47.35	57	58
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
Would you be able to compete for public procurement contracts without making unofficial payments to public officials?	0.30	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?	5.78	14	101
Do firms in your industry to know in advance the precise amount necessary for extra unofficial payments to public officials?	0.21	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?	0.24	114	1
Do you know CGU and its anti-corruption efforts?	0.49	115	0

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. All respondents are provided with a list of options to choose from. 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 do not respond.

Appendix A.3. Data

A.1. **Coding of CGU Audit Reports.** We extract information from the audit reports thanks to the support of a team of research assistants. The coding of the information is performed according to the following timeline.

A.1.1. *Preliminary Analysis.* We download and carefully read and analyze a random sample of 100 audit reports. Based on this extensive reading, we develop a detailed instruction manual, highlighting the specific pieces of information we want to extract.

A.1.2. *Full Data Extraction.* Four research assistant and a research supervisor are each assigned a set of audit reports to analyze.[55] All researchers are native speakers (four Brazilians and one Portuguese), have at least a university degree, they are paid a competitive hourly wage, and they obtain a performance-based bonus based on speed and quality. The job is tracked online on a leading freelancing platform. Each team member is also assigned 30 audit reports that are also assigned to other team members. This provides a double check of 150 audit reports. Incompatible entries are checked and corrected by the research supervisor.

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

- Tax identifier and firm name
- Amount associated to the irregularity (i.e. value of public procurement contract)
- Type of corruption, chosen to be one or multiple of the following: irregular procurement, diversion of public funds, mismanagement of public funds, over-invoicing
- Involvement of the firm, i.e. a dummy for whether the firm is passively involved in the irregular case.
- Whether there is a bank mentioned in the irregularity, and its name (if available)
- Indicator for whether it is a family firm (based on the company name and other information in the report)
- Indicator for whether there is a personal connection to the politician or public official (mostly family members)
- Open-ended comments about additional information and issues

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

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

A.1.3. *Double-Check of Entire Database and Extraction of Additional Information.* After a preliminary analysis of the data, we conduct a thorough cross-validation of the information collected, and a simultaneous second phase of data extraction from the audit reports. This phase is carried out by the research supervisor.

[55]Each team member receives a slightly different number of reports, as this depends on the nature of the contract and individual efficiency.

First, the research supervisor goes over the entire list of data entries and audit reports, with the objective of validating the most valuable information extracted for the cases involving formal firms (tax identifier, amount, type of corruption, firm involvement).

Second, she collects three additional pieces of information:

- The date the contract awarding and completion, when available (these are often months or years earlier than the date the audit takes place)
- An indicator variable for whether the irregularity is simply a procedural irregularity (e.g. missing application documents)
- An indicator variable for whether the firm does not perform the job at all (despite being paid for it), or performs an incomplete or low-quality job.

A.1.4. *Final Quality Check.* We then examine the data, and identify all remaining unsolved issues and potential data entry mistakes. Subsequently, together with the research supervisor, we examine the text of the audit reports again to check and, if needed, correct these issues.

A.2. **RAIS.** The principal source of firm and worker level data is obtained from the RAIS (Relação Anual de Informações Sociais) database. RAIS is an administrative dataset created in 1976 and managed by the Brazilian Ministry of Labor (Ministério do Trabalho e Emprego - MTE). It is widely considered a high quality Census of the Brazilian formal labor market (Dix-Carneiro, 2014), with comparable properties to the matched employer-employee data for France and the United States (Menezes-Filho et al., 2008). Except for the informal sector, its coverage is almost universal, in that very few categories of workers, such as a subset of self-employed individuals and elected politicians, are not required to report information in the system.[56] RAIS has information on both private sector and public sector employees. In order to have at least three years of both pre- and post- data for our analysis, we focus on RAIS for the period 2000-2014.

The database is mainly used to (a) generate statistics on the Brazilian economy by several government agencies (for example, the Brazilian Central Bank), and to (b) generate and provide information necessary to process unemployment insurance payments and other worker benefits programs. Both firms and workers have incentives to provide accurate information. On the one hand, employers are subject to severe fines if they do not regularly submit the required information. On the other hand, workers have incentive to be in RAIS since they want to be eligible to receive the various government benefits they are entitled to.

In any given year, the data in RAIS is at the worker-job level, meaning that a worker with multiple jobs, or a worker who is first fired and then hired in the same year, appears more than once in the data. Each individual is assigned a unique administrative worker identifier called PIS, which allows for tracking of the individual over time and across firms (as well as establishments of the same firm). We keep only one observation per worker per year, choosing the oldest highest-paying job of the individual, as in Menezes-Filho et al. (2008) and Helpman et al. (2017). RAIS contains information on both the firm and the establishment tax identifiers of the worker, their locations and industry, and several other plant-specific variables.[57] 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

[56]The absence of informal sector workers is a more severe weakness of the data in Brazil than in the US, given the larger share of informal firms in the former compared to the latter. Reliable estimates of the informal sector in Brazil are not available. However, discussions we had with officials at the MTE and at public procurement agencies suggest that informality is less of an issue when it comes to firms regularly doing business with the government, as it is the case in the context of this paper.

[57]At the firm level, we assign sectoral classification based on the mode sector of the firm's establishments in the given year. The municipality of a firm is instead computed as the municipality of the headquarter established (identified by the "0001" code). If there is no headquarter establishment in the given year, we assign the mode municipality across the firm's establishments in the given year.

contains individual specific data on gender, nationality, age and education, as well as data on hours worked, reason of hiring and firing/separation, and contract details (such as temporary, short term, apprenticeship contracts). Firm-level and establishment-level information is obtained by aggregating the individual-level data, *after* the above restrictions are imposed.

We compute wages using the average monthly wage of the worker over the given job spell in the given calendar year, as reported in RAIS. We express all wages in real terms and in Brazilian Reals. Wages in RAIS include all taxable income and worker payments to Brazilian social security contributions. As described in Helpman et al. (2017), and reported in the RAIS manual, the wage measure includes the following: salaries; overtime compensation for contracted extra hours; extraordinary additions, supplements and bonuses (but not participation in the employer's profits outside the employment contract such as through equity holdings in the employing firm); tips and gratuities; commissions and fees; contracted premia; hazard compensation; executive compensation; cost reimbursement components if they exceed 50 percent of the base salary and are for travel or transfers necessary for the execution of the job; payments for periods of vacation, holidays and parental leave (but not severance payments for layoffs and or indemnity payments for permanent maternal leave); vacation gratuities if they exceed 20 days of salary; piece wages; and in-kind remunerations such as room and board .

Each worker is also assigned an occupational category specific to its current job. There are more than 2,000 such categories, which follow the detailed Classificação Brasileira de Ocupações (CBO). We match the CBO classification to the International Standard Classification of Occupations (ISCO-88) using the procedure outlined in Muendler et al. (2004). This correspondence allows us to confidently categorize workers into four organizational layers, following a framework close to Caliendo et al. (2012). From bottom to top layer, these are: *Blue Collar* (skilled agricultural and fishery workers, craft and related workers, plant and machine operators and assemblers, elementary occupations); *White Collar* (professionals, technicians and associate professionals, clerks, service workers and shop and market sales workers), *Managers*, and *CEOs* (defined, following a standard ISCO-88 criterion, as the highest paid manager of a firm with at least 3 managers).[58]

A.3. **Additional Data.**

Manufacturing Census. We construct productivity, investment, profits, and sales measures using the Annual Industrial Survey (Pesquisa Industrial Anual - PIA), which is confidential data constructed and maintained by the Brazilian Statistical Institute (Instituto Brasileiro de Geografia e Estatística - IBGE). This data is available exclusively for the manufacturing and extraction sectors, and the full panel data is available only for firms with more than 30 employees.

Public Procurement Contracts. Data on public procurement is obtained from the Ministry of Planning, Budget, and Management (Ministério do Planejamento, Orçamento e Gestão - MP). We have access to the universe of federal public procurement contracts, from the late 1990s to 2015. For each contract, we have information on the company name and tax identifier of the winning firm, the code of the government agency responsible for the tender, the value of each lot auctioned off in the contract, month and year the contract was awarded, and a description of the lots. For the subset of federal procurement contracts that were auctioned off using the Brazilian eProcurement system ComprasNet (fully launched in 2005), we also observe all the bidders and bids (that is, both winners and losers, as well as all the bids, of each auction).[59]

[58]These correspond, respectively, to ISCO-88 1-digit categories 6-9 (Blue Collar), 2-5 (White Collar), and 1 (Managers and CEOs).

[59]These contracts represent almost 40% of total contracts, and allow us to analyze participation rates as well.

Loans and Banking Sector. We obtain loan-level data used 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 of Brazilian companies. BNDES provides a significant share of long-term bank lending in Brazil, and it is among the largest sources of investment in industry and infrastructure (Colby, 2012). For each loan, we have information on the loan amount, the interest rate (and type), and tax identifier of the firm receiving the loan.[60]

Municipal level data on the number and amount of *all* loans to businesses, and on the location of bank branches in Brazil are obtained from the Brazilian Central Bank (Banco Central do Brasil - BCB). This data covers both private and government banks.

Suspensions of Firms. Data on public procurement suspensions of firms come from the National Registry of Ineligible and Suspended Companies (Cadastro Nacional de Empresas Inidôneas e Suspensas - CEIS), also referred to as the "Blacklist."[61]

Other Datasets. Electoral data for the 2000, 2004, 2008, and 2012 municipal elections, and all political candidates, are obtained from the Tribunal Superior Eleitoral (TSE). All other data used in the paper come from publicly available databases from the Instituto Brasileiro de Geografia e Estatística (IBGE).

[60]We have access to BNDES data on the following types of loans: *non-automatic*, *direct and indirect*; *post-shipment exports*; *pre-shipment exports*; *automatic*. The excluded categories of loans are those pertaining to *BNDES card*, and *variable income operations*.

[61]These data sources do not cover the universe of suspended firms. In particular, the CEIS information is verified only after 2007, and comes from approximately half of the Brazilian states.

Appendix A.4. Alternative Strategies

A.1. **Municipality Level Dynamic Difference-in-Difference Model.** This section briefly describes an alternative empirical strategy to estimate the effects of the anti-corruption audits on local economic outcomes. The strategic is based on a dynamic matching difference in difference estimator, similar to the strategy discussed for corrupt firms in Section 4.2.

We make three empirical choices. First, at each audit year, we use as controls only municipalities that are eligible for the lottery but that have *never* been audited. That is, municipalities audited at a later point in time are not used as controls in earlier years, and vice-versa. This allows us to have controls that are never directly affected by the treatment, and to analyze longer time horizons. Figure 2 provides a visual illustration of the pool of treated (shades of blue) and control (white) municipalities. Second, for each audited municipality at each audit year, we select as controls only municipalities that have the same audit probability.[62][63] Matching on the audit probability is essential to ensure that the treatment is exogenous to the economic conditions of the municipality, since the sampling procedure is stratified by state and states differ widely in size. In particular, this allows us to control for any differential *deterrence effect* that could bias our findings. This could occur, for example, if municipalities in small states (where the audit probability in a year can be as high as 43%) react differently to the audit threat, in the pre-period, compared to municipalities in large states (where the audit probability in a year can be as low as 1%). Finally, for each audited municipality, we keep as controls *all* potential controls selected in the first two steps.[64]

We estimate the following difference-in-difference model, over a window of [-3,5] years around the audit:

$$y_{mt} = \alpha_m + \alpha_t + \delta \times PostAudit_{mt} + \beta \times PostAudit_{mt} \times Audited_m + \epsilon_{mt} \tag{A1}$$

where y_{mt} represents the main outcome variable of interest for municipality m in year t, such as total number of business establishments or total employment. α_m captures municipality fixed effects and α_t are calendar year fixed effects, so as to control for unchanging differences across municipalities and years. $Audited_m$ is an indicator variable for audited municipality m, thus capturing treatment status. $PostAudit_{mt}$ is an indicator variable for both audited and control municipalities for the year of the audit and all years after. Our coefficient of interest is β, which measures the differential effect of the anti-corruption audits on local economic outcomes, comparing audited municipalities to municipalities that have the same probability of being audited, controlling for the sets of fixed effects. ϵ_{mkt} are standard errors clustered at the level of the municipality.

We can estimate heterogeneity in treatment effects by augmenting specification 4.2 with interactions of the post-audit treatment effects with specific pre-audit or fixed covariates, such as levels of private sector corruption uncovered or presence of media. These specifications then naturally also include interactions between the baseline period effects and the given covariate.

[62]The exact audit probability is known ex-ante only by CGU, and it is established only a few months before the audit itself. Hence, we define the audit probabilities to be as close as possible to what the relevant agents in the municipality would expect, based on the recent audit rounds. Specifically, for each municipality at each audit year, we compute the probability as the average implied probability of the last three years, for the given municipality. By construction, these probabilities are the same within state-year. For audit years 2004 and 2005, we use the average implied probability in the last and last two years, respectively; for audit year 2003, we use the 2003 implied probability.

[63]The same audit probability requirement captures all municipalities in the same state, as well as municipalities in other states with the same probabilities.

[64]Since we have multiple treatments over time, municipalities can be repeated as controls multiple times. Clustering the standard errors at the level of the municipality accounts for the correlation in the error term that comes from repeating control municipalities multiple times. See also Dube et al. (2010) and Flaaen et al. (2016).

Additionally, to visually asses the dynamic effects of the audits, we can report the point estimates of the following dynamic version of equation 4.2:

$$y_{mkt} = \alpha_m + \alpha_t + \sum_{k=-3}^{k=5} \delta_k \times \mathbb{1}(Event_k) + \sum_{k=-3}^{k=5} \beta_k \times \mathbb{1}(Event_k) \times Audited_m + \epsilon_{mkt} \tag{A2}$$

where $k = t - a$ represents the years relative to the audit year a, and $\mathbb{1}(Event_k)$ is an indicator function taking value 1 if it is year k relative to the audit year. We also normalize the coefficient in $k = -1$ to 0 ($\beta_{-1} = 0$), so that we compare outcomes of audited and control municipalities relative to the year before the audit, and leveraging variation within the same municipality and within the same relative event year.

The main identifying assumption is that the anti-corruption audits are exogenous, conditional on all the covariates included in specification 4.4. In particular, we assume that the treated and control municipalities would have followed parallel trends in the main outcomes in the years after the audit, in the counterfactual scenario in which the audit did not take place. Conditional on audit probability, the randomness of the audit ensures these identifying assumptions are likely to be satisfied.

A.2. **Synthetic Controls.** We test the robustness of our corrupt firms empirical framework through an alternative strategy based on a synthetic control estimator (Abadie and Gardeazabal, 2003, Abadie et al., 2010).

We build a synthetic control for each corrupt using only average information in the years [-4,-2] relative to the audit. We create the synthetic control from a pool of pre-selected firms, which we select as being in the same sector and having similar employment levels three years before the audit.[65] The synthetic control is obtained weighting all firms in the control pool so as to minimize the pre-treatment differences with the treated firm. In particular, this methodology allows to flexibly control for unobserved factors that affect common trends in both the treatment and control group (Abadie et al., 2010).

While this empirical strategy is commonly used in cases of only one treated unit, we follow a strategy similar to Acemoglu et al. (2016) to extend the methodology to the case of multiple treated units. Hence, we first construct the synthetic control for each firm, and we then aggregate the individual treatment effects through a re-weighting using the quality of each match. Focusing on the main dependent variable ($Ln(Emp)$), our estimate is computed as follows:

$$\theta(t) = \frac{\sum_{i \in \text{TreatmentGroup}} \frac{Ln(Emp)_{it} - \widehat{Ln(Emp)}_{it}}{\sigma_i}}{\sum_{i \in \text{TreatmentGroup}} \frac{1}{\sigma_i}}, \tag{A3}$$

where $\widehat{Ln(Emp)}_{it} = \sum_{j \in \text{ControlGroup}} w_j^i Ln(Emp)_{jt}$, with w_i^j being the optimal weights obtained from the minimization of pre-audit differences in $Ln(Emp)$. $1/\sigma_i$ measures the goodness of fit for each match, so that better matches are given more weight in the estimation. We create the synthetic control using averages in the years [-4,-2] of the following variables that capture firm activity: log of payroll per employee, log of total payroll, total number of hirings, and total number of firings. We also use $Ln(Emp)$ in the matching by using its value in year -2 only.

We report the dynamics of the synthetic control and the treated firms in Figure A3.

[65] We require firms to be in the same employment decile in the economy within the given sector. This step is not needed, but it alleviates computational issues arising when constructing a synthetic control using the several millions firms in the Brazil's formal economy.