CORRUPTION AND FIRMS

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Abstract. We estimate the causal real economic effects of a randomized anti-corruption crackdown on local governments in Brazil over the period 2003-2014. After anti-corruption audits, municipalities experience an increase in economic activity concentrated in sectors most dependent on government relationships. These effects spill over to nearby municipalities and are larger when the audits are covered by the media. Back-of-the-envelope estimates suggest that $1 away from corruption generates more than $3 in local value added. Using administrative matched employer-employee and firm-level datasets and novel face-to-face firm surveys we argue that corruption mostly acts as a barrier to entry, and by introducing costs and distortions on local government-dependent firms. The political misallocation of resources across firms plays a seemingly secondary role, indicating that at the local level most rents are captured by politicians and public officials rather than firms.

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1. Introduction

Corruption is at the center of the international policy debate, as epitomized by World Bank Group President Jim Yong Kim's 2013 statement defining it as "public enemy number one." Starting in the late 1990s, a number of resources and a tremendous amount of effort have been devoted to anti-corruption policies, ranging from international transparency initiatives and regulations to numerous national anti-corruption programs. One of the main goals of these policies is to spur private sector development, yet their effectiveness remains a controversial topic, as some argue such policies may backfire.[1]

In this paper, we ask: how does a national anti-corruption crackdown affect local economic activity? Answering this question is not only important from a policy perspective, but can allow us to shed light on the mechanisms linking government corruption –commonly defined as the (illegal) abuse of public office for private gain– to firm growth and resource allocation.

Although an active body of theoretical and empirical literature has investigated the interplay between corruption and economic development, establishing causality has proven extremely difficult due to the equilibrium nature of corruption.[2] As a result, both cross-country and firm-level studies typically suffer from endogeneity issues that make it difficult to pin down specific channels. Anti-corruption initiatives also tend to present similar challenges as they are often anticipated, and at times manipulated, by the corrupt agents they target in the first place.

We circumvent these obstacles by focusing on the unique context of Brazil, where the federal government randomly audited local public spending to crackdown on corruption in local governments from 2003 to 2014.[3] These audits are randomized across municipalities and have been shown to have a long-lasting impact in reducing corruption (Avis et al., 2018). By exploiting exogenous spatial and time variation, we can identify the causal impact of the program on various outcomes at the level of the local economy and the firm.

[1]See, for example, When corruption is good for the economy (Fortune, 2014), Why corruption is a messy business (Financial Times, 2014), and Where do you turn when the anti-corruption crusaders are dirty? (The New York Times, 2019).

[2]For extensive reviews of the literature, see Bardhan (1997), Jain (2001), Svensson (2005), Hanna et al. (2011), Olken and Pande (2012), Banerjee et al. (2012), Rose-Ackerman and Palifka (2016), and Fisman and Golden (2017).

[3]Audits are one of the most common tools governments adopt to monitor and punish corrupt activities. Supreme Audit Institutions (SAIs), i.e., specific agencies dedicated to anti-corruption audits, are present in almost every country in the world (http://www.intosai.org/about-us/organisation/membership-list.html).

As a first step toward understanding how corruption affects local government-firm relationships, we create and gather data from various sources. To construct our primary outcome measures at the municipality and firm level, we use administrative matched employer-employee data for the entire Brazilian formal sector over the period 2002-2017. We also rely on a number of confidential datasets (such as the administrative censuses of retail and service sector firms), and publicly available ones (such as data on campaign contributions and politicians as well as surveys of informality and data on public procurement contracts). Additionally, we collect new qualitative evidence on *how* corruption affects firm activity by means of a novel corruption-focused face-to-face survey of firms doing business with local governments.

In the first part of the paper, we estimate the impact of the anti-corruption crackdown on the local economy. In a staggered difference-in-difference framework, 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 showing that a range of local economic, demographic, and political characteristics cannot predict audits, and we illustrate parallel trends in the outcome variables prior to the audit. After an anti-corruption crackdown, treatment municipalities experience higher levels of economic activity relative to control municipalities. On average, we observe a 0.9% increase in the number of firms operating in treated municipalities in the three years after the audit.

Importantly, consistent with anti-corruption activity primarily affecting the economic sectors that most rely on government relationships, our effects are larger and fully concentrated in those sectors. We adopt two primary definitions: (i) sectors that are highly represented in government procurement, and (ii) sectors that are exposed to corruption as identified in the audit reports. We show that while the number of firms in government-dependent sectors increases by 1.4%-1.5%, there is a zero effect elsewhere, with the difference across sectors being statistically significant.

To confirm that audits do increase real economic activity, we show that other aggregate measures coming from distinct administrative sources are positively affected by the audits: total sales by local firms increase by about 6%, and the total volume of lending and deposits in local banks rise by about 3%.[4] Using government surveys of informality, we also perform several tests to show that a shift from the informal to the

[4]Sales data, and other firms' financials, are only available for a sample of medium and large firms in retail and service sectors.

formal sector is unlikely to drive our results. Additionally, we may be concerned that while we capture more economic activity, it is of lower quality. To this end, we show that the average entrant firm after anti-corruption audits is similar in size (measured by total employment), pay per employee, and ex-post growth rates compared to the average entrant firm in control municipalities. Moreover, the average entrant firm after anti-corruption audits has managers of higher ability than those in control municipalities.

Why do audits increase economic activity? The results on multiple data sources for the outcome variables, informality, and characteristics of entrants are inconsistent with a story where firms associate anti-corruption audits to a higher risk of tax audits, and react by formalizing their business, which would consequently show up as more (lower quality) firms in the economy. We also find no evidence of the federal government channeling more resources (e.g., via federal transfers) to municipalities after they are audited.

A direct intuitive mechanism is that corruption is bad for business, and the audits are effective at reducing corruption, as shown by Avis et al. (2018) who compare aggregate corruption levels in municipalities audited in the past with first-timers. Importantly, audits lead exclusively to a reduction in severe irregularity cases (i.e., corruption), while acts of mismanagement by public officials (i.e., inefficiency) are unaffected.[5] An implication of this mechanism is that less rents are extracted by corrupt agents, and therefore more public spending is directed to productive uses. We provide additional reduced-form evidence consistent with this channel. For example, the audits should be especially successful when taking place right before a municipal election, and in municipalities with better access to media; in line with these hypotheses, there are stronger effects on the local economy in these contexts. Similarly, we uncover spillovers into neighboring municipalities, with these neighbors of audited municipalities experiencing higher economic activity that suggests a deterrence effect from audits. The presence of spillovers also shows that the local reallocation of economic activity cannot account for our main results. Finally, we also find larger positive effects in areas where high amounts of corruption are uncovered, which is where the audits are potentially more effective.

[5] Avis et al. (2018) focus on identifying *why* the audits reduce corruption, showing that it is largely because the audits lead to legal actions (such as court convictions of corrupt politicians), which raise both actual and perceived costs of engaging in corruption. They find almost no evidence for alternative stories, such as a mechanical displacement effect of corruption to other sectors and types of expenses, a change in the composition of politicians, or other electoral costs.

Within this framework in which audits cause a shift from a higher to a lower level of corruption, our findings would then provide some of the first causal evidence against long-standing "efficient corruption" views, which state that corruption may be beneficial for firms that operate in second-best contexts plagued by red tape and unnecessary regulations (Leff, 1964; Huntington, 2006). Instead, the fact that anti-corruption efforts increase local economic activity suggests that theories of corruption hampering economic growth are at play (Murphy et al., 1991; Shleifer and Vishny, 1993; Svensson, 2005).

In the second part of the paper, we aim to inform firm-level theories of corruption. Specifically, key in the literature is understanding whether corruption rents are captured by public officials and politicians or by firms, which has vastly different policy implications. We start by estimating the impact of the anti-corruption program across firms within audited municipalities. In a context where the local government is captured by rent-seeking firms, we would expect an anti-corruption crackdown to have a differential negative effect on incumbent government-dependent firms. However, we find that local incumbents in government-dependent sectors grow more, relative to other local firms and to firms in control municipalities, after the audit. We also find their sales per employee increase, which implies these firms experience improved performance and are not just growing in response to higher competition. These results are at odds with rent-seeking theories according to which local firms in key sectors capture the local government.

We enrich our analysis using extremely granular data on the political connections of both firms and their managers. We do find some evidence of political misallocation of resources across firms. Specifically, campaign donors and firms whose managers are elected politicians, political candidates, or registered party members experience sizable employment losses after the audits. However, this set of firms represents less than 1% of the local economy, casting doubt on this channel playing a major role in our context. Consistent with these findings, Colonnelli et al. (2019) find that the vast majority of firms involved in irregularities with the government are not the active perpetrators of corruption and in fact benefit from the anti-corruption crackdown; on the other hand, the small set of firms actively involved in the corruption suffers after the audits take place.[6] All in all, our firm-level results are consistent with a large

[6]Colonnelli et al. (2019) identify firms involved in corruption as those that appear in the audit reports to be associated with an irregularity. However, most such firms are not located in the audited municipality, thus making a direct comparison with our empirical strategy and local economy results difficult. Indeed, Colonnelli et al. (2019) focus exclusively on the set of firms located in *never-audited*

literature on the individual benefits that politically connected firms accrue through corruption, but mainly highlight that most firms at the local level suffer from the rent-seeking activity of public officials and politicians.

We conclude by providing additional qualitative evidence to understand how corruption affects firms, through a new survey we administered to a representative sample of 115 owners of small and medium government-dependent firms. Essentially, all firms consider corruption an impediment to business entry and growth, and yet the vast majority of firms still consider doing business with the government to be very competitive and indicate "unofficial payments" to public officials as a necessary cost to compete. Firms indicate a primary role played by interactions with the mayor and the local procurement official, rather than corruption driven by other firms. More survey questions point to corruption as both a direct "bribe" cost as well as a source of inefficiency within firms (e.g., due to investment distortions).

Our paper relates to three main strands of literature. First, we contribute to the vast literature on corruption. In particular, we relate to studies that focus on the role of corruption for firms and economic development, and those studying the effectiveness of government audits in reducing corruption.[7] Compared to previous studies, we exploit exogenous variation in anti-corruption activity to estimate its impact on local economic activity and firm dynamics, and by exploiting extremely detailed micro-data to investigate mechanisms.

We also contribute to studies that assess the importance of political connections to firms.[8] Our evidence that politically connected firms suffer after an anti-corruption crackdown is consistent with these studies. In fact, a large body of work, such as the seminal work by Fisman (2001), argues that politically connected firms benefit from corruption. This paper adds an additional wrinkle to this argument, highlighting that

municipalities, but doing business in audited ones, so as to eliminate confounding factors driven by the impact of the audits on the local economy that we document in this paper.

[7]Examples of the former are Wade (1985), Shleifer and Vishny (1993), Shleifer and Vishny (1994), Mauro (1995), Bliss and Di Tella (1997), Kaufmann and Wei (1999), Svensson (2003), Fisman and Svensson (2007), Olken (2007), Olken and Barron (2009), Cole and Tran (2011), Sequeira and Djankov (2014), Weaver (2016), Smith (2016), and Bai et al. (2017). Anti-corruption audits in Brazil are the focus of Ferraz and Finan (2008), Bologna and Ross (2015), Lichand et al. (2016), Zamboni and Litschig (2018), and Avis et al. (2018). Studies on related anti-corruption initiatives in other countries include Bobonis et al. (2016), Zeume (2017), Giannetti et al. (2017), and Chen and Kung (2018), among others.

[8]Examples include 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), Akey (2015), Fisman and Wang (2015), Schoenherr (2018), Akcigit et al. (2018), and González and Prem (2019).

corruption is an extra cost of doing business for the vast majority of government-dependent firms interacting with local governments, where politicians and public officials are in positions of power. A key difference compared to ours is that most other studies in this literature restrict attention to the set of politically connected firms, and especially large, often publicly listed ones connected to top politicians, which are likely to capture massive rents thanks to their control over the political and regulatory environment. In this sense, our study is instead closer to Svensson (2003) and Fisman and Svensson (2007), who use survey evidence to uncover large costs of corruption to small and medium firms.

Finally, 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, 2011; Hsieh and Klenow, 2014). Our paper emphasizes the role of one important but arguably understudied friction, corruption, in the interactions between governments and firms.

This paper is organized as follows. In Section 2 we illustrate the institutional context. Section 3 describes the primary data sources. Section 4 presents the identification strategy and the results on the local economy. In Section 5 we study the impact of the anti-corruption program on firm dynamics to discuss firm-level theories of corruption. Section 6 concludes.

2. The CGU Anti-corruption Program

In May 2003, under the administration of Luis Inácio 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 consisted 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 and is considered to be the main anti-corruption body in Brazil. At each round, approximately 60 municipalities are randomly selected, with replacement.[9] As of 2014, more than 99% of Brazil's 5,570 municipalities

[9]The randomization is linked to the draw of a popular national lottery. The implied audit probability in any given round, which is constant within a state, is therefore quite low (1% within a round, and 3% within a year). Additionally, there is a small exception to the random draw with replacement, as municipalities cannot be selected if they were selected in one of the previous three rounds.

are eligible, and 1,881 have been selected at least once. Only municipalities below a certain population threshold are eligible for the program, and state capitals are excluded.[10]

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 of inspection orders. The state offices are then in charge of sending a team of auditors to the selected municipalities within days of the lottery. The audits investigate how the federal transfers from the central government to the municipality are spent, focusing mostly on the previous three years.[11]

The investigation consists of an intense few weeks of field work, during which auditors analyze all the relevant documents and receipts related to the spending of federal funds, interview local people, bureaucrats, and other relevant parties, solicit direct anonymous complaints about malfeasance, and take pictures to report the quality of public service delivery. 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, consist of an organized analysis of all the information gathered, and are publicly available.

The audits may have severe legal consequences, which mostly target corrupt politicians and officials. 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 enforcement activity. The temporary or lifelong suspension of officials from the public post and the impeachment of the mayor by the local office or the mayor's loss of mandate are the most common consequences of the CGU audits.

In Figure 1 we illustrate the variation of the program over time using administrative data from the CGU. We can see from Panel A that the intensity of the program was higher in the first three years, and significantly lower in the final three years. Panel B plots the total amount of audited federal transfers to municipalities (in millions of USD), showing spikes of around USD 1.5 billions in 2004, 2009, and 2010. Panels C and D display the total number of mismanagement and corruption irregularities, respectively, following the CGU split between minor and more severe irregularities and

[10]The population threshold was originally $100,000$, but it was successively increased to $300,000$ soon after the launch, and then rose to $500,000$ for the remaining years of the program.

[11]All federal transfers tend to be audited for smaller municipalities, while often just a subset of the transfers can be audited for larger municipalities. The details of each inspection order and the sectors that are audited can change over time, at the CGU central office's discretion.

the language of Avis et al. (2018).[12] Figure 2 shows the wide geographical variation of audits and corruption across Brazil.

3. Main Data Sources

The main outcome measures used in the analysis come from administrative matched employer-employee data, which we complement with confidential firm-level government censuses, as well as several other confidential and public datasets.

3.1. **Matched Employer-Employee Data.** The principal source of firm- and worker-level data is obtained from the Brazilian Ministry of Labor's RAIS (Relação Anual de Informações Sociais) database. It is widely considered a high-quality census of the formal labor market (Menezes-Filho et al., 2008, Dix-Carneiro, 2014, Helpman et al., 2017). Except for the informal sector and a subset of self-employed individuals, its coverage is almost universal. Data quality and coverage improved dramatically in 2002 when individual tax identifiers and additional variables were incorporated into RAIS. We therefore focus on the years 2002-2017, which is also the period for which the primary datasets discussed below are also available.

Each individual is assigned a unique administrative worker identifier, 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 on several other establishment- and firm-specific variables. We use these variables to aggregate the measures at the establishment-, firm-, and municipality-level in the analysis. Similar to other employer-employee matched data, we observe wages and hiring and firing dates, but also gender, nationality, age, and education, as well as data on hours worked, reason of hiring and firing, and contract details. Finally, each worker is assigned an occupational category specific to her or his current job, which allows us to identify managers and to distinguish the role of individuals within both public and private organizations.

Our analysis focuses on the full set of 5,526 Brazilian municipalities that are eligible for the program, thus excluding only the 44 (larger) ineligible municipalities (i.e., state capitals and those with population above the program's threshold). Of these, 1,881 municipalities are audited at least once during the period 2003-2014. Table 1 displays summary statistics for this sample (Panel B), as well as for all municipalities eligible for the program (Panel A). Eligible municipalities have an average (median) of 251

[12]The CGU started collecting data on the specific types of corruption in 2006.

(43) business establishments and 239 (41) firms and 2816 (324) private sector workers who have a wage of BRL 461 (418) per month. The average (median) firm has 16 (3) employees. As displayed in Panel B, the distribution of audited municipalities (i.e., excluding those eligible but never audited) is extremely similar.

3.2. **Firm-level Data on Retail and Service Sectors.** The primary advantage of RAIS is its nearly complete coverage of private sector firms. However, RAIS lacks balance sheet information that are especially important to construct measures of aggregate economic activity. We therefore rely on two unique yearly datasets collected by the Brazilian Institute of Statistics (IBGE), which is the primary data collection government agency in Brazil. We have access to the two datasets, the Annual Survey of Retail (PAC (Pesquisa Anual de Comércio)) and the Annual Survey of Service (PAS (Pesquisa Anual de Serviços)), for the period 2002-2014.

These data are generated by PAC and PAS from surveys aimed at monitoring firm performance in the retail and service sectors, respectively, which together cover approximately 68% of all firms in the economy.[13] Both PAC and PAS surveys are constructed using two strata: the first stratum (*estrato amostrado*) includes a nationally representative sample of single-establishment, single-state firms with less than 20 employees; the second stratum (*estrato certo*) consists of all other firms (i.e., firms with at least 20 employees or those with establishments in more than one state), which are sampled with probability one. To ensure our data are representative at the municipality level in the analysis, we use only firms in the *estrato certo*. The advantage of using these data is that they contain financial information on firm performance, such as sales and investment.[14]

3.3. **Other Datasets.** We rely on a variety of complementary data sources, which we introduce in different parts of the paper and summarize below.

Surveys of Informality. We obtain information on informality from the National Household Survey (PNAD (Pesquisa Nacional por Amostra de Domicílios)), which is an annual survey first made available in 2002 by IBGE. The primary variables we use to measure informality are the total number of workers without a formal contract and all unpaid workers. Unfortunately, this is the only source of panel data on informality for our analysis period, and it is only available at the state-year level. Therefore,

[13]Using the CNAE 2.0 classification, PAC covers Section G, while PAS covers Sections E, H, I, J, L, M, N, R, S.

[14]Unfortunately, we cannot match these data to RAIS, as both sources are based on confidential agreements that do not allow for the disclosure of the tax identifiers.

we impute informality measures at the municipality-year level using both population shares from IBGE annual surveys and informality shares from the 2000 Decennial Census.[15]

Elections, Political Connections, and Campaign Contributions. Electoral data on municipal and federal elections are obtained from the Tribunal Superior Eleitoral (TSE). TSE also provides detailed data on both individual and corporate campaign contributions over the same period as well as individual-level data on both elected and unelected political candidates. We use various measures based on these data, which we describe in more depth in Colonnelli et al. (2018), to explore effects across the electoral cycles and to identify politically connected firms.

Public Procurement. We construct measures of sectoral government dependence using contract-level data from federal public procurement. These data are obtained from the Ministry of Planning, Budget, and Management (Ministério do Planejamento, Orçamento e Gestão - MP). We have access to the universe of federal contracts from 2000 to 2014.

Loans and Banking Sector. Municipal-level data on the total amount of local loans to both businesses and individuals, and on the total amount of deposits in local banks by both business and individuals, are obtained from the Brazilian Central Bank (Banco Central do Brasil - BCB), through their ESTBAN dataset, for the period 2002-2017.[16]

Other Datasets. We use administrative data on audits and irregularities from the CGU. Several datasets come directly from IBGE, such as municipality-level data on GDP and other measures used in this paper, including those in the Decennial Census. Data on federal transfers come from the National Treasury's FIMBRA dataset, while data on block grants to municipalities come from the CGU. We obtain information on the media coverage of each audit through a manual search across all national and local newspapers available online in historical archives. Finally, we collect data on

[15]We can validate the imputations in year 2010, when the new wave of the Decennial Census allows for precise measurement of informality at the municipality level. We find that our two imputation strategies lead to a correlation of the informality measures we create with the "true" ones of 0.71 and 0.89, respectively.

[16]Specifically, the main measure of total loans is the log of variable 160, while the main measure of deposits is the log of the sum of variables 410, 411, and 412.

satellite lights directly from the Defense Meteorological Satellite Program, and we then construct a municipality-year panel for the 2002-2013 period.[17]

4. 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. In this section, we first outline the empirical strategy and then show that anti-corruption audits positively affect various municipal-level economic outcomes. We then discuss the mechanisms and economic interpretation of our findings.

4.1. **Empirical Design.**

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 public officials to seek bribes from local firms, thus preventing us from causally estimating whether corruption affects the local economy. This has arguably been the main limitation to the empirical work on corruption and economic development.

The context we study is unique in this regard, as the anti-corruption audits are randomized across municipalities over time. The design of the program therefore lends itself to a municipality-level event-study estimation method. After removing the few municipalities that are ineligible for the program, we have a set of treated (at different points in time) and never-treated municipalities. Then, at the same point in time t, we can compare the outcomes of municipalities that are audited at time t to those audited at time $t + \mu$ ($\mu > 0$) and the never-treated municipalities.

Since the median number of audit rounds in a year is three, we aggregate the main outcome data at the quarter level whenever possible (e.g., when using data from RAIS), or at the year level whenever that is the frequency of the data (e.g., when using the firm censuses). Moreover, since most data sources begin in 2002 and end in 2017, our main estimation window covers the period of [-4,12] quarters (or [-1,3] years) around the audit.[18]

[17]We follow recent techniques to deal with continuity and comparability problems in raw satellite data (Liu et al., 2012; Abrahams et al., 2018). In particular, we: (i) create buffers around municipalities to decrease the censoring problem; (ii) perform a two-step deblurring filtering following the approach by Abrahams et al. (2018); (iii) follow the intercalibration step by Wu et al. (2013) to increase comparability across-time; (iv) conduct a geometric correction following Zhao et al. (2014) and Tsouvala (2015) to deal with shifted rasters.

[18]As discussed later, the results are robust to multiple other estimation windows, including an unconstrained window around the event.

4.1.1. *Estimating Equations.* We estimate both non-parametric and parametric event study models. The former allows us to capture the dynamics of real economic outcomes relative to the time of audit, and we estimate it as follows:

$$y_{mt} = \alpha_m + \alpha_t + \sum_{k=-4}^{k=-1} \mu_k + \sum_{k=1}^{k=12} \mu_k + \epsilon_{mt} \tag{4.1}$$

where m and t stand for municipality and quarter, respectively, and $\{\mu_k\}$ captures the relative event-time indicators.[19] That is, μ_k is an indicator variable taking value 1 if it is quarter k relative to the audit quarter. These indicator variables are always 0 for municipalities that are never audited. 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 quarter before the audit. The specification includes municipality fixed effects (α_m) and quarter fixed effects (α_t), which absorb fixed differences across space and time. The latter are especially important since the scope of the audits may change over time, for example due to fluctuations in the CGU budget. ϵ_{mt} are standard errors clustered at the level of the municipality (Bertrand et al., 2004).

A parametric specification, on the other hand, allows us to better analyze the statistical significance and magnitude of the estimates. We estimate the following model:

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

where again m and t stand for municipality and quarter, respectively, and $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarters 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 quarter fixed effects.[20]

[19] By quarter, unless otherwise specified, we mean the specific year-quarter, so that we have 48 quarters in the estimation period 2002-2017.

[20] When studying heterogeneous effects based on differential preexisting characteristics, we estimate 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 pre-audit, unless otherwise specified.

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 time fixed effects. For example, an audit that is anticipated by either corrupt officials and politicians, or by firms, 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 by means of a public lottery, and previous work has strongly established the validity of this randomization (Ferraz and Finan, 2008; Litschig and Zamboni, 2008; Ferraz and Finan, 2011). We also test for this in the data in Table 2, where we regress an indicator for whether the municipality is audited on a host of local economic, demographic, and political characteristics, and state fixed effects. For example, we show in column 1 audited municipalities are similar based on observable characteristics to never-audited municipalities: none of the covariates we use in the regression, which are measured at the beginning of the sample and cover economic, demographic, and political measures, are statistically significant, and the magnitudes of each coefficient are small. All covariates are standardized by their mean and standard deviation. In columns 2-5, we repeat a similar exercise for different audit cohorts, adding covariates measured the year before the audit, and find analogous results; at most one coefficient per audit cohort is statistically significant, and magnitudes are small. Overall, the patterns in the data strongly indicate that the randomization of the audits was successful.

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 next. For our research design to be valid, audited and control municipalities should follow parallel trends in the period prior to the audit, which implies that the pre-period μ_k indicators should not be statistically different from zero.

4.2. **Main Results.** We study the impact of anti-corruption audits on the local economy using the total number of private sector business establishments and the total number of private sector firms as primary outcomes, which we express in logarithmic terms.[21]

We first explore the dynamics in the raw data for our main outcome variables. Panels A and D of Figure 3 display the raw data over the window of [-4,12] quarters

[21]Unless otherwise specified, before taking the logs, the raw variables are winsorized at the 0.5% of the empirical distribution to reduce the impact of outliers.

around the audit, where we de-mean the variables by their average value over the four quarters leading up to the audit. The figures provide a visual test for our identification strategy, as we observe parallel trends in the pre-period for both our measures of business establishments and firms. The raw data also illustrate the positive impact of the audits on the real economy. We explicitly plot the difference between the treated and control outcomes in Panels B and E, which highlight the widening gap in quarters after the audit takes place. We then identify these dynamics more precisely by plotting the point estimates obtained from the estimation of equation 4.1, as in Panels C and F of Figure 3. The figure shows that pre-trends are parallel in our preferred baseline event study specification as well, with the positive effect of the audits materializing gradually over the 12 quarters after the audit.

We interpret the magnitude of the findings by estimating the parametric event-study equation 4.2, reported in Table 3. We see that the audits have a positive and statistically significant impact on both measures of economic activity: columns 1 and 2 show an increase of around 0.9% in response to the audit. The magnitudes and dynamics for both establishments and firms are extremely similar, as in practice we see that most firms in these local economies are single-establishment firms; these similarities also indicate that the increase in economic activity is primarily driven by new single-establishment firms rather than a reallocation of activity across municipalities by large, multi-establishment ones.

4.2.1. *Government-Dependent Sectors.* Given that the goal of the anti-corruption program is to discipline corrupt public officials and politicians in their use of public funds, a key implication of our analysis is that sectors characterized by more government interactions should be more affected. We therefore enrich the analysis by showing how the audits impact different sectors of the local economy. The importance of differential exposure to corrupt officials across sectors and firms is lucidly illustrated in one of the first studies of firm-level corruption by Svensson (2003). We measure firms' exposure to corruption in local governments using the information on the four-digit industry of each firm and by proposing two definitions of "government-dependent" (GD) sectors. First, we create a measure of public procurement intensity, as this is a direct measure of the extent of interactions between firms and the government. Using data on the universe of federal procurement contracts over the period 2000-2014, we create the distribution by sector of contracts won, scaled by the total number of establishments within each sector: we define as "GD-Procurement"

those sectors that are above the median of this distribution.[22] Second, using the data constructed in Colonnelli et al. (2019) on firms directly involved in corruption irregularities, we create a measure of direct exposure to local corruption. Namely, we identify the sectors of firms ever appearing in the audit reports as being involved (to any extent) in an irregularity, and define as "GD-Corrupt" those sectors that are above the median in terms of number of irregularities over the number of establishments.

Overall, out of a total of 622 in the data, we have 311 and 261 four-digit sectors appearing in the first and second classification, respectively, with an overlap of 185 of them, and a correlation of 0.36 (Appendix Table A1); the most common specific examples are listed in Table A2, while additional summary statistics on the size of these sectors are in Table 1. The top three examples of GD-Procurement are related to the wholesale retail of medical products and equipment, and office supplies, while in the case of GD-Corrupt, they are related to the wholesale retail of pharmaceutical products, maintenance and repair of railway vehicles, and road construction.

Consistent with the audits primarily affecting firms who strongly depend on relationships with the government, in columns 3-10 of Table 3 we find the results are stronger and fully concentrated among such firms. Depending on which GD definition we use, we observe an increase of 1.4-1.5% in the number of firms in GD sectors, compared to a zero and insignificant effect in non-GD sectors, with this difference statistically significant at different levels depending on the specification (as reported in the table). Importantly, Figure 4 provides further support to our empirical strategy, as it shows that treated and control municipalities also follow parallel trends in the dynamics of GD sectors in the period leading up to the audit.[23] Of course, these measures represent proxies of exposure to the anti-corruption policy, as ultimately all sectors in the economy may be somewhat affected. In fact, interactions with the local government are extremely common for local firms due to business regulations, licensing requirements, taxes, exemptions, and other provisions of public goods and services.[24]

[22]While data on *municipal* public procurement would be more relevant for our goals, such data are only available for a small subset of municipalities and years.

[23]We also report the raw data for the GD splits in Appendix Figure A1.

[24]While these types of government-firm interactions are each subject to different degrees of exposure to the audit investigation, notice that the officials disciplined by the audits are often responsible for a number of different tasks, which may go beyond the specific cases the auditors focus on.

4.2.2. *Robustness Checks.* We probe the robustness of our main results to various alternative specifications in Appendix Table A3. In Panel A, we show that the impact of the audits on total numbers of firms is also positive and statistically significant when we make no time restriction on the estimation window (columns 1 and 2), and when we exclude all municipalities who are audited more than once (columns 3 and 4). Additionally, given the slight changes in eligibility requirements in the first two years of the program, and the drop in program intensity in the last three years, we also show that our results hold when excluding years 2003 and 2004 (columns 5 and 6), and when excluding years 2012, 2013, and 2014 (columns 7 and 8). In Panel B of Appendix Table A3, we also show that the effects are robust to the estimation within the sample of just audited municipalities (columns 1 and 2), and to not winsorizing the dependent variables (columns 5 and 6), or winsorizing at the 5% level to drop a larger set of potential outliers (columns 7 and 8). In columns 3 and 4 of Panel B, we also show that our results are essentially unchanged when we add a control for the municipality-level implied probability of being audited in the given year-quarter. In terms of magnitudes, we find that the estimated effects of these alternative specifications are often larger than the baseline effects of Table 3.

An additional set of robustness tests refer to our definitions of government dependence (GD). We create six additional measures using both the federal procurement data and the corruption measures. However, reliable *value* measures (e.g., size of the contract) are only available for the former. Therefore, the first three additional measures we create are analogous to the main GD-Procurement measure; however, we do not scale by number of firms in the sector (GD-Procurement-NS), and we use total value rather than number of contracts (GD-Procurement-Value-S and GD-Procurement-Value-NS, for the scaled and unscaled version, respectively). The fourth measure is the baseline measure of GD-Corrupt, but without scaling by the number of firms (GD-Corrupt-NS). The fifth (scaled by number of establishments in the sector) and sixth (unscaled) versions are created using the sectoral distribution in terms of number of unique *firms* involved in corruption cases (rather than number of irregularities). We show the correlation matrix across all eight GD measures in Appendix Table A1. Consistent with our main findings, Table A4 shows that all types of GD sectors are positively affected by the audits, while we do not find any effect on other sectors, and the dynamic point estimates are consistent with the validity of our empirical design (Appendix Figures A2 and A3).

4.3. **Do Audits Increase Economic Activity?** While we find that audits lead to an increase in firm activity, it is important to know whether these firms generate real economic value. For example, if firms are on average of lower quality, then it is difficult to argue that anti-corruption audits have positive local effects. Similarly, in a context like Brazil, where informality rates are high, we may be concerned that more formal firms may simply capture a reallocation from the informal to the formal sector. We attempt to address these issues by relying on multiple datasets from different sources, and by re-estimating the main specification using different dependent variables.

Table 4, Panel A, reports the results from the estimation of equation 4.2 using yearly outcome data from the administrative firm censuses PAC (Retail) and PAS (Services) and from the Brazilian Central Bank. These datasets are all generated by different agencies for different purposes and capture a different dimension of economic activity than just the total number of firms. In column 1 we find that, after the anti-corruption audits, total revenues generated by local firms increase by approximately 6.2%; similarly, investment rates (i.e., the share of capital expenditures over total assets) increase by a sizable 14.8%, even though this coefficient is not statistically significant (column 2).[25] On the other hand, in columns 3 and 4, we explore the impact of the audits on the two primary measures of financial development collected by the Central Bank, namely the total volume of local credit to firms and individuals and the total deposits by firms and individuals in local bank branches. In both cases, we find an increase of approximately of 2.4% and 3.2%, respectively, in the three years after the audit takes place, .

In Panel B of Table 4, we tackle more directly the issues of unobservable informality in the micro-data, which cannot be conclusively addressed for obvious measurement reasons. However, we rely on the two primary sources of information on informality in Brazil, namely the National Household Survey (PNAD) and the Decennial Census; while there is no information on output and firm activity, both include counts of informal (and formal) employment. The limitation is that PNAD is only available at the state-year level, while the only relevant Censuses for our purposes are available in 2000 and 2010. In the first case, we therefore impute the data at the municipality level as discussed in Section 3, while in the second case we modify our strategy to be a difference-in-difference with just one pre- and one post- set of observations. Table 4, Panel B shows that both informal and formal economic activity increase.

[25]Unfortunately, we do not have access to data on value added and other variables needed to construct productivity measures, which are only available for large manufacturing firms through other IBGE confidential datasets.

Importantly, the increase of informal employment suggests our results are unlikely to be driven by a shift from the informal to the formal sector, but rather that the audits lead to overall higher levels of economic activity.[26]

We conclude the characterization of the increased levels of economic activity in Table 5, where we take a different approach. We argue that a higher number of firms are associated with positive outcomes for the municipality if these firms are not of lower quality than the average new firm in the economy. To estimate whether and how the audits affect the quality of firms in the municipality, we create a repeated cross-section of all new firms in the economy, where each firm appears only in its founding year, and then estimate the following specification:

$$y_{fsmt} = \alpha_m + \alpha_{st} + \beta \times PostAudit_{mt} + \epsilon_{fsmt} \tag{4.4}$$

where f is firm, s is sector, and the rest is as in the main specification 4.2.[27] While measuring a firm's quality, y_{fsmt}, is obviously challenging, the rich information in RAIS allows us to construct various proxies for it, which we use as dependent variables. Specifically, we first look at firm-specific characteristics, namely average employment and pay per employee (columns 1 and 2, respectively), and future growth rates (three-year and five-year rates, in column 3 and 4, respectively) of new firms, which are standard proxies for firm-level performance.[28] We find that the audit does not lead to the creation of worse-performing firms in the municipality. We then move to characterizing firm quality looking at the characteristics of its employees. In columns 5 and 6, we find that firms in audited municipalities are similar in terms of

[26]In a recent paper, Bologna Pavlik and Harger (2018) also argue for the importance of accounting for informality in this context and find that audits have mixed and modest effects on GDP and satellite lights, with the latter measure declining starting several years after an audit. As discussed in Section 3, we also apply recent measurement approaches to create a panel of satellite nighttime data at the municipality-year level, together with GDP estimates. As shown in Table A5, we also find modest effects of the audits on both GDP and satellite light measures –using our main specifications, when statistically significant, these estimates are positive. Our focus on firm-level measures of economic activity is due to the fact that local GDP is only imputed to municipalities using proxies from more aggregate statistics (Instituto Brasileiro de Geografia e Estatistica, 2004), while satellite lights are only available for a smaller subset of our sample, tend to under-estimate real electricity consumption that does not show up in light output, and do not distinguish between private and public sector activity (Burlig and Preonas, 2016).

[27]The main difference is that we include sector-by-year fixed effects, as our main results show that there is a disproportionate positive effect of the audits on specific (government-dependent) sectors of the economy, which we must take into account when comparing firm characteristics across different municipalities.

[28]Growth rates (winsorized at 0.5% of the tails) are measured as $(emp_k - emp_0)/emp_0$, where emp_0 is employment at the year of entry, and emp_k is employment 3 or 5 years after entry.

the share of workers with at least high school education, and and this remains the same when focusing on managers' education only. We further estimate worker ability following the AKM procedure of Abowd et al. (1999) and find no difference in average ability across firms (column 7). However, when focusing on the managerial capital of the firms, we find that audits lead to the creation of firms with higher-ability managers on average (column 8). Finally, under the assumption that the unemployed or the individuals in the informal sector are lower-quality workers, we use as dependent variable the share of such employees in the firm (column 9), and find no effect on this margin neither. All in all, we find that the quality of new firms following an audit is similar to that of the average new firm and that, if anything, there is an increase in the average ability of managers operating in the local economy.

4.4. **Interpretation and Magnitudes.** Why do anti-corruption audits increase economic activity? There are a few potential explanations for our effects, but we find no such empirical support for them. One potential explanation is that the audit generates a behavioral response in the local informal sector. That is, informal firms may associate anti-corruption audits with a higher risk of tax audits, and thus respond by formalizing their business. This would lead to more firms being registered in our main dataset (RAIS). However, while interesting, this story is inconsistent with our previous results regarding informality and the characteristics of firms. Furthermore, discussions with CGU officials suggest this is unlikely the case, as the anti-corruption audits have no link to tax audits, and they are not used by the tax collection agency for any purpose. There is also no overlap between tax and anti-corruption auditors.

Another reason for why we see more economic activity after an audit is that the federal government may react positively, expecting positive outcomes after cracking down on corruption, and thus channeling more resources locally. Since the main source of federal support to municipalities consists of transfers and block grants from the central government, we can test for this directly using municipality-level data from the CGU and the National Treasury. In Appendix Table A6, we find no evidence that municipalities experience a higher inflow of various federal transfers (columns 1, 2, and 3), nor of block grants or their share that is disbursed (columns 4 and 5) after the audits. Similarly, we find no effect on another margin of the central government's reaction to the audits, namely the allocation of federal public procurement contracts to local firms (column 6).

A direct potential explanation behind our effects is that corruption is bad for business, and that audits are effective at reducing corruption. That audits are effective

is precisely what Avis et al. (2018) show in the same context by comparing aggregate corruption levels among municipalities audited for the first time to those of municipalities that had also been audited in the past. Using municipality-level data from the CGU, they show corruption irregularities decrease by 8%, while cases of mismanagement remain unaffected.[29]

We can provide further reduced-form tests consistent with Avis et al. (2018), as well as with other previous studies on the political economy of the Brazil's CGU program. We do so in Table 6, where we estimate the interacted specification 4.3 using the total number of business establishments as main dependent variable. First, we may expect the effects to be stronger in contexts where more corruption was uncovered, as this can trigger higher perceived and actual threats of punishment of illicit acts. In column 1 of Table 6 we add as an heterogeneity term an indicator for whether the municipality is a "high corruption" one (that is, if the number of irregularities uncovered, scaled by municipality size, is above the median of the empirical distribution), and we find that the audits' effects are considerably larger in those areas. In column 2, consistent with both Ferraz and Finan (2008) and Avis et al. (2018), we find the audits have a stronger impact in municipalities where the audit is covered by the media, suggesting the information channel is a key one to understand the effectiveness of anti-corruption policies.[30] Finally, in column 3, we show that the effects are also larger when the audit takes place the year prior to a municipal election, which Ferraz and Finan (2008) show is associated with a higher threat of electoral punishment for the (corrupt) politicians.

An additional important channel through which audits are effective is one of deterrence, as also seen in Bobonis et al. (2016), Zamboni and Litschig (2018), and Avis et al. (2018). This is obviously impossible to fully identify in our context, as almost the entire country is subject to the audit threat after the program is announced in

[29]Importantly, Avis et al. (2018) on identifying *why* the audits reduce corruption, in a context where politicians are not perfectly informed about the likelihood or effectiveness of the audits. They show that the audits lead to an increase in legal actions, such as police crackdowns and court convictions of corrupt politicians and officials, which raise both actual and perceived costs of engaging in corruption. That is, after the audits mayors may refrain from engaging in corruption due to the legal costs they are now aware they may incur. As a result, less public funds are diverted away from productive uses. Through various tests and a structural model, Avis et al. (2018) find almost no evidence for alternative stories, such as a mechanical displacement effect of corruption to other sectors and types of expenses, a change in the composition of politicians, or other electoral costs.

[30]We manually collect information on the coverage of the CGU's specific municipal audits across both national and local newspapers by searching through online archives for mentions of the CGU audit taking place. 40% of the audits were covered by at least one news outlet according to our search.

2003. Nonetheless, we can test for the presence of spillover effects, which may happen in neighboring municipalities that are not directly subject to the audit, but may similarly perceive its threat. We provide evidence in support of this story in column 4 of Table 6, where we define "neighboring" as all municipalities in the same micro-region.[31] In particular, we study the impact of the audit of one or more municipalities, in a given micro-region, on all other municipalities that are not audited in the same micro-region. The control group consists of municipalities in micro-regions that are audited later or that are never audited.[32] We uncover the presence of spillover effects, as a nearby audit has an impact on non-audited municipalities that is similar in magnitudes to our baseline effects (i.e., a 1% increase in the total number of business establishments). This also corroborates the importance of information diffusion for the effectiveness of the audits, which we further validate by showing that these spillover effects are even larger when the given audit is covered by the media (column 5).[33] Finally, these results also indicate that we may be underestimating the average impact of the audits, and that our results are unlikely to be driven by a reallocation of economic activity from nearby areas.

Ultimately, these tests are only suggestive of the role of corruption per se, as we cannot measure corruption across all municipalities over time, and because we can only label irregularities based on the sometimes limited information from the audit reports. Yet, the findings of Avis et al. (2018) that corruption cases diminish, while mismanagement ones do not, may suggest at least a partial shift from a higher to a lower equilibrium of corruption occurs, in a within-country setting where regulations, red tape, and the efficiency of officials are arguably held fixed.[34] In this scenario, our findings provide some of the first causal evidence against long-standing "efficient corruption" views, which posit that in heavily regulated contexts like Brazil, economic activity would be higher in a corrupt equilibrium where it is easier for firms to (illegally) bypass bureaucratic processes (Leff, 1964, Huntington, 2006).[35] Instead,

[31]Brazil has 5,570 municipalities and 558 micro-regions, which the national statistical agency (IBGE) defines as the best approximation to local labor markets.

[32]We only consider a micro-region to be treated the first time one (or more) of its municipalities is randomly drawn to be audited.

[33]We define the interaction term on media coverage as the number of audited municipalities in the micro-region whose specific audits were directly covered by local or national newspapers.

[34]Most regulations are not set at the municipality level and thus do not change after the audits. Moreover, the audit program had no training or management component for local public officials.

[35]Brazil is ranked among the highest on government regulation in the Global Competitiveness Index (World Economic Forum, 2015; 143/144 countries surveyed), and in our surveys almost half the firms rank regulations (and taxes) as the biggest barrier to entry and to growth they face.

our effects seem consistent with various theories of bureaucratic and political corruption as a way for government officials and politicians to extract rents away from the private sector, and with studies highlighting the negative correlation of corruption and economic outcomes at the aggregate and firm level (Mauro, 1995; (Fisman and Svensson, 2007); Rose-Ackerman and Palifka, 2016). We further expand on this in Section 5.

We conclude with a suggestive cost-benefit and magnitudes analysis, since estimating the impact of an audit program is important for policy, especially in light of the heated debate over the need for and effectiveness of anti-corruption policies around the world. We estimate the average firm in a municipality in our sample generates $\$258,000$ of value added per year.[36] Given the average increase in the number of firms, this implies an average of $\$583,000$ extra value added in the municipality in a given year after the audit ($\$258,000 * 251.11 * 0.009$). The average municipality receives approximately $\$5,400,000$ in federal transfers in a given year. According to Avis et al. (2018), the audits lead to a reduction of $\$174,000$ in corrupt local public spending. As a result, we can estimate that one less dollar linked to corruption generates 3.35 dollars of local value added ($\$583,000/174,000$). In terms of cost-benefit of the CGU program, consider that an audit is estimated to cost $\$50,000$ (Zamboni and Litschig, 2018), which would suggest a local multiplier of about 11.7.[37] A caveat to this analysis is that it does not take into account the presence of spillovers and it is specific to this partial equilibrium context. While we caution to take these numbers with a grain of salt, they indicate a potentially non-trivial impact anti-corruption policy may have on the local economy.

5. How Does Corruption Affect Firms?

The analysis in Section 4 concludes that anti-corruption audits positively affect the local economy. Yet, these aggregate effects may mask the firm-level heterogeneity relevant to firm-level theories of corruption; such heterogeneity allow for aggregate

[36]We compute this number using the average sales of a firm in our sample in the PAC and PAS datasets, rescaled by the average firm size in our main analysis sample from RAIS, since we must take into account that the latter firms are smaller. We use sales as, unfortunately, PAC and PAS do not report value added. However, sales and value added appear to be extremely similar in the Brazilian manufacturing sector, which is where we can compare these numbers in other IBGE confidential datasets (specifically, the Annual Survey of Manufacturing, PIA).

[37]While the 11.7 multiplier may seem massive, consider that the $\$50,000$ estimate only accounts for the costs of the actual audit process, but excludes the extra legal costs that may be needed for the audits to be effective, as these latter costs are difficult to determine.

economic costs to coexist with the individual benefits specific firms receive from corruption. Key to firm-level theories of corruption is understanding whether corruption rents are captured by government agents or firms. Indeed, one set of theories points to firms and their rent-seeking and regulatory-capture practices in various economic sectors which aims to extract rents at the expense of other firms. Another set of theories emphasizes corrupt officials and politicians as the key players extracting rents away from productive firms coerced into paying extra costs of doing business in the form of bribes or other operational distortions. These theories, and several nuanced versions of them, are discussed at length in the reviews by Bardhan (1997), Jain (2001), Olken and Pande (2012), Rose-Ackerman and Palifka (2016), and Fisman and Golden (2017).

In this section, we contribute to this debate in two ways. First, we study the impact of the CGU program across firms within audited municipalities to explore which firms benefit and/or suffer most from the anti-corruption crackdown. Second, we provide descriptive evidence drawn from new face-to-face surveys of owners of small and medium firms doing business with municipal governments.

5.1. **Anti-corruption Audits and Firm Dynamics.** In a context where the local government is captured by rent-seeking firms, we would expect an anti-corruption crackdown to have a differential negative effect on government-dependent firms. Specifically, these negative effects should be concentrated among local *incumbent* firms in these sectors, which may now lose preferential access to government favors or suffer from increased competitive pressure. To this end, we estimate a firm-level specification that estimates the differential impact of the audits across firms within audited municipalities.[38] The empirical model is the following:

$$
\begin{aligned}
y_{fmt} = {} & \alpha_f + \alpha_t + \gamma_1 \times Z_f \times Incumbent_f \times PostAudit_{mt} \\
& + \gamma_2 \times Incumbent_f \times PostAudit_{mt} + \gamma_3 \times Z_f \times PostAudit_{mt} \\
& + \beta \times PostAudit_{mt} + \epsilon_{fmt},
\end{aligned} \tag{5.1}
$$

[38]We mainly focus on the within-audited municipality comparison since the measures of interest, such as incumbent status, are defined relative to the timing of the audit, and thus cannot be defined in never-audited municipalities.

where f, m, and t stand for firm, municipality, and quarter, respectively, and $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarters after the audit in the audited municipality, and 0 otherwise.[39] $PostAudit_{mt}$ is always 0 for never-treated municipalities. $Incumbent_f$ is an indicator variable equal to one for those establishments alive in the year-quarter of the audit and the previous four quarters. Z_f are a set of indicator variables that are fixed within firms and that aim to capture their government-dependent or politically connected status. α_f and α_t are firm and year-quarter fixed effects, respectively. Standard errors are clustered at the municipality level to account for correlation across time and across establishments in the same municipality. We are interested in measuring the impact of the audits on firm performance, but there is no detailed balance-sheet information that is available for all firms; hence, we measure firm-level outcome variables using the log of total employment and, when possible, investment and the log of sales per employee.[40] The final sample includes approximately 3.8 million unique establishments across 5,524 eligible municipalities over the 2002-2017 period, and the estimation window covers the [-4,12] quarters around the audit.

The results are reported in Table 7, where the main parameter of interest is γ_1 (top row), which captures the differential impact of the audits on incumbent government-dependent firms relative to other firms in the municipality. Interestingly, government-dependent firms grow after the audits, as shown by the 1.3% relative increase in average employment for firms in both GD-Procurement (column 1) and GD-Corrupt (column 2) sectors. Additionally, after the audit takes place, we find that incumbent government-dependent firms grow by 1.4% relative to the average firm in control municipalities.[41] Importantly, when focusing on the PAC and PAS sample, the growth in size among this set of firms seems to be linked to increased performance as well. Government-dependent incumbent firms in GD-Procurement (GD-Corrupt) sectors

[39]More precisely, the unit of observation is an establishment, rather than a firm, since the former can always be linked to a specific municipality. As discussed earlier, most firms have a single establishment, and this choice is inconsequential for the results.

[40]The employment measures are available for all firms in the sample, as they come from the aggregation of the employee-level data in RAIS. Investment and especially sales per employee are more accurate measures of firm performance, but they are only available for the subset of firms in the administrative PAC and PAS censuses discussed in Section 3. Given our focus on the *estrato certo* (i.e., all firms sampled with probability one), when using these outcome variables we cannot distinguish between incumbents and non-incumbents, since most firms are incumbents, and we shut down the channel of entry and exit from the census solely due to random sampling.

[41]This result is obtained by summing the coefficients reported in each column of Table 7. We find these estimates to be statistically significant at the 1% level.

experience a 4.8% (3.9%) in sales per employee, and perform significantly better also relatively to the average firm in control municipalities. The latter results are inconsistent with a story according to which firms simply hire more workers to obtain the same level of output, for example as a response to increased competition. We find similar increases in investment by incumbent firms in both GD-Procurement and GD-Corrupt sectors relative to other local firms, even though there is no difference relative to firms in control municipalities.[42]

The results so far indicate that audits have an especially positive effect on incumbent firms in government-dependent sectors. We further enrich the analysis to explore more granular heterogeneous effects across firms. We aim specifically to identify firms which may be ex-ante more likely to benefit from the presence of corruption. To this end, we create multiple measures of political connections using data on the universe of political candidates (elected or not), party affiliations, and corporate donations in Brazil, as introduced in Section 3. We create the following indicator variables, which are equal to one if a firm has these characteristics at the time of the audit or at any time prior to that: (i) firm that donates to a political campaign; (ii) firm that has at least one manager who donates to a political campaign; (iii) firm that has at least one manager who is an elected politician; (iv) firm that has at least one manager who is a political candidate (elected or not); (v) firm that has at least one manager who is a registered member of a political party.

We report the findings of these additional tests in Table 8, where we estimate specification 5.1 using these firm-level indicator variables to measure Z_f. We drop the $Incumbent_f$ indicator from the analysis since all such measures require the firm to be an incumbent, and we focus on employment as a dependent variable since we are unable to match these firm-level political features to the PAC and PAS administrative census data. Compared to the analysis of government-dependent incumbents, we find that politically connected firms grow less after the anti-corruption crackdown. The magnitudes of these differential effects are large, ranging from around a 3% decline in size for campaign-donor firms to a decline of more than 30% for firms whose manager is an elected politician, arguably the strongest measure of political connection.

In sum, our firm-level analysis shows that the vast majority of firms interacting with the local government benefit from the anti-corruption program, including incumbent firms most exposed to relationships with the local government. The exception is a

[42]Investment is measured as total capital expenditures over total assets. The estimated coefficients for GD-Procurement and GD-Corrupt represent increases of 0.07 and 0.05 standard deviations, respectively.

small set of politically connected firms that together represent less than 0.5% of all firms and less than 1% of total employment in the municipality. Consistent with our findings, Colonnelli et al. (2019) find that the vast majority of firms involved in irregularities with the government are not the active perpetrators of corruption and in fact benefit from the anti-corruption crackdown; on the other hand, the small set of firms actively involved in the corruption suffers after the audits take place. All in all, our firm-level results are consistent with a large literature on the individual benefits that politically connected firms accrue through corruption, but mainly highlight that most firms at the local level suffer from the rent-seeking activity of public officials and politicians.[43]

Together with the previous municipality-level results, our findings suggest that the anti-corruption audits positively affect the local economy through the relaxation of entry barriers and a reduction in the costs of doing business for government-dependent firms.[44]

5.2. **Surveying Owners and Managers of Government-Dependent Firms.** We attempt to further inform theories on corruption and firms using largely qualitative evidence from new surveys of owners of small and medium government-dependent firms representative of our context, which help us unpack *how* corruption introduces extra costs of doing business to firms.

We administered the face-to-face surveys to owners or managers of firms located in the Brazil's southeastern state of Minas Gerais in August and September 2017. We restricted our attention to municipalities around the city of Nova Lima meeting the CGU eligibility criteria for the anti-corruption audits, and to firms with up to 30 employees that had sold goods or services to local governments in the previous year.

[43]A caveat to our analysis is that we can only identify the effects of the audits on local firms, since the audits are randomized at the level of the municipality. Therefore, underlying our framework is the assumption that the *physical* location of a firm is an important determinant of its interaction with the local government, through either public procurement or other types of relationships (e.g., business regulations, licensing requirements, taxes, exemptions, etc.). However, firms located outside the audited municipalities may also have business relationships with them, as shown by Colonnelli et al. (2019); consistently with our analysis of spillovers in Section 4.2, this suggests we may be underestimating the aggregate costs of corruption for the private sector.

[44]In Table A7, we show that our effects are also unlikely to be driven by compositional changes due to differential exit of firms from the sample, as the audits have a zero effect on firm deaths both on average and across the sets of government-dependent and politically connected firms.

After applying these restrictions, we randomly sampled 175 firms, and were able to survey 115 of them, for a response rate of approximately 66%.[45]

We summarize the main findings from these surveys in Figure 5 and Table 9, and from which we draw five main conclusions. First and foremost, firms consider corruption to be a major cost of doing business, ranking behind only "taxes and regulations" as the primary barrier to both entry in a new market as well as firm growth and expansion. Of the 115 firms, 112 state that corruption affects business operations, and two-thirds of them believe their growth rate would increase dramatically (by more than 10%) in a world without corruption. Interestingly, corruption seems to be a friction to investment (82%), to financial decisions (79%), to choices to expand to new markets and products (77%), and to bid for public procurement contracts (68%). These findings are consistent with the presence of various distortions highlighted by the academic literature when thinking of corruption as a tax (Shleifer and Vishny, 1993; Fisman and Svensson, 2007). Indeed, the uncertainty around corruption plays an important role, which is reflected in the reluctance or inability of more than half the firms to respond to questions about corruption's prevalence and about the size of "unofficial payments" (i.e., bribes). Only 21% of firms say they know ex-ante how much they must pay in bribes to public officials, with the typical bribe being around 6% of the transaction value (even though only 15 firms decided to answer this latter question). Corruption is perceived as pervasive, with firms thinking it affects approximately half of government contracts and half the firms in their sector.

A second finding, consistent with our results, is that firms report corruption to mostly involve politicians and other public officials, rather than other firms (Figure 5, Panel C). Third, public procurement is the primary area where corruption happens, even though firms also highlight its pervasiveness throughout several other encounters with public officials, such as for procedures to obtain licenses, permits, and authorizations, and for tax administration purposes (Panel D). Fourth, despite all these issues, doing business with the government is still considered a rather competitive market, with firm efficiency (rather than political connections and collusion) seen as the main determinant to obtain a government contract (Panel E). Relatedly, as shown in Table 9, 75% of firms report this market to be competitive, and 55% deem unofficial payments to public officials a necessary cost to compete. The fifth

[45]The surveys were conducted by a local research assistant, who disclosed the purely academic goal of the research was to understand the role of corruption in government-firm relationships. Participation was voluntary and no incentives were provided. The list of government providers have only recently been made available, through the "transparency portals" of each municipality.

and final conclusion we draw from the survey responses, in Table 9, is that almost all firms consider initiatives to punish corrupt officials necessary to improve the business environment, even though they believe the government has mostly been unsuccessful in this endeavor and lament difficulties in reporting corruption to higher levels of government when local officials commit irregularities.

6. Conclusion

We provide evidence that an anti-corruption crackdown on municipal governments positively affects local economic activity by making it easier for government-dependent firms to enter and grow. We establish causality thanks to the unique features of the Brazil's 2003-2014 randomized audit program, and we show that our effects are tightly linked to the specific channel of reduced corruption. Suggestive back-of-the-envelope calculations indicate the presence of potentially large multipliers generated by government spending on anti-corruption efforts.

A key takeaway from our findings is that a model where corruption rents are extracted by public officials and politicians, rather than firms, seems to better fit the patterns we document. This observation may likely apply to similar local contexts in other emerging economies and, more generally, to interactions between governments and firms where the former is in the position of power.

Other takeaways relate to the policy implications of our findings, as we emphasize the importance of accounting for the role of the private sector when designing policies aimed at reducing corruption in the public sector, since the spillover effects on the former can be significant. Additionally, in light of the severe costs they face because of corruption, our results show that private sector firms may be important allies in the fight against corruption.

There are several avenues of future research. Importantly, more work is needed to identify specific mechanisms through which corruption deters firm entry and hampers firm growth. Collecting panel data on bribes and firm choices in corrupt environments, for example through detailed firm surveys, is a promising way forward. Similarly, field experiments linking corruption and firm activity have the potential to overcome the challenges driven by the dearth of natural experiments in this context. Moreover, while our design is limited to a partial equilibrium analysis, attempts to capture general equilibrium effects and macro-implications of anti-corruption programs deserve further attention. Finally, there is significant scope for new research that studies how corruption affects within-firm resource allocation, which mostly remains a black box.

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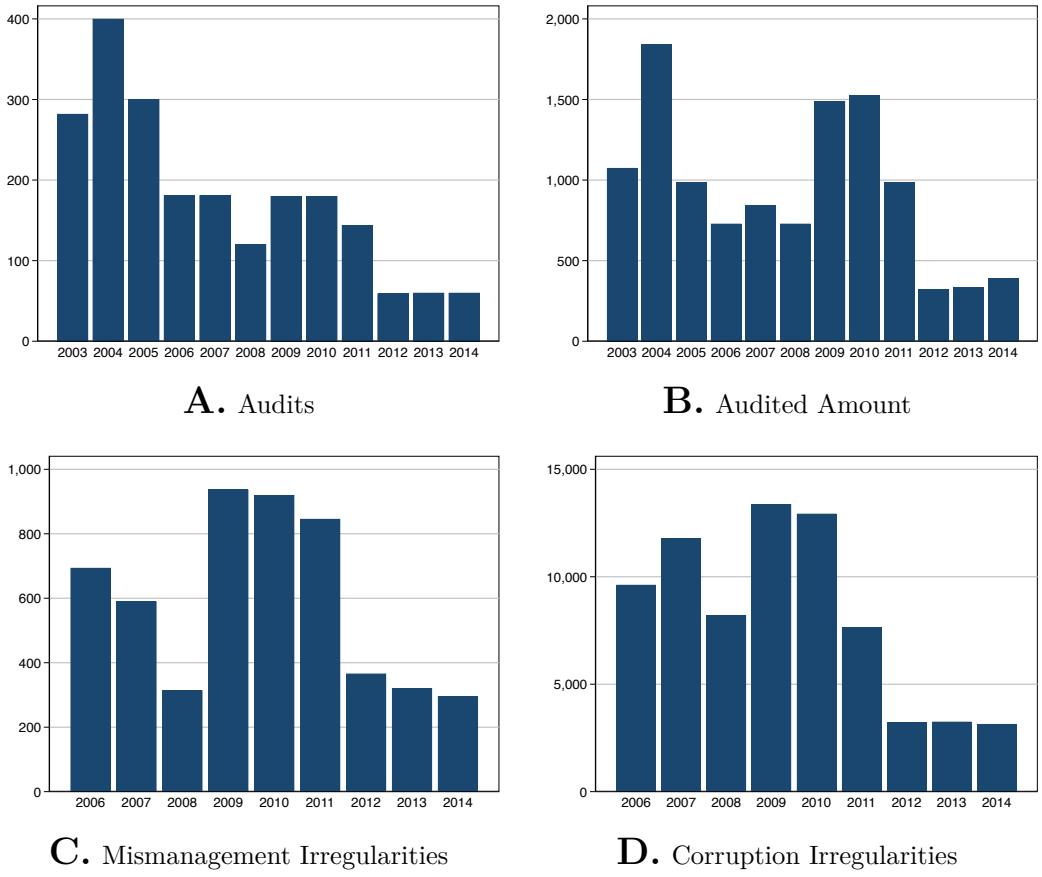
FIGURE 1. **The Anti-Corruption Program Over Time**



A. Audits

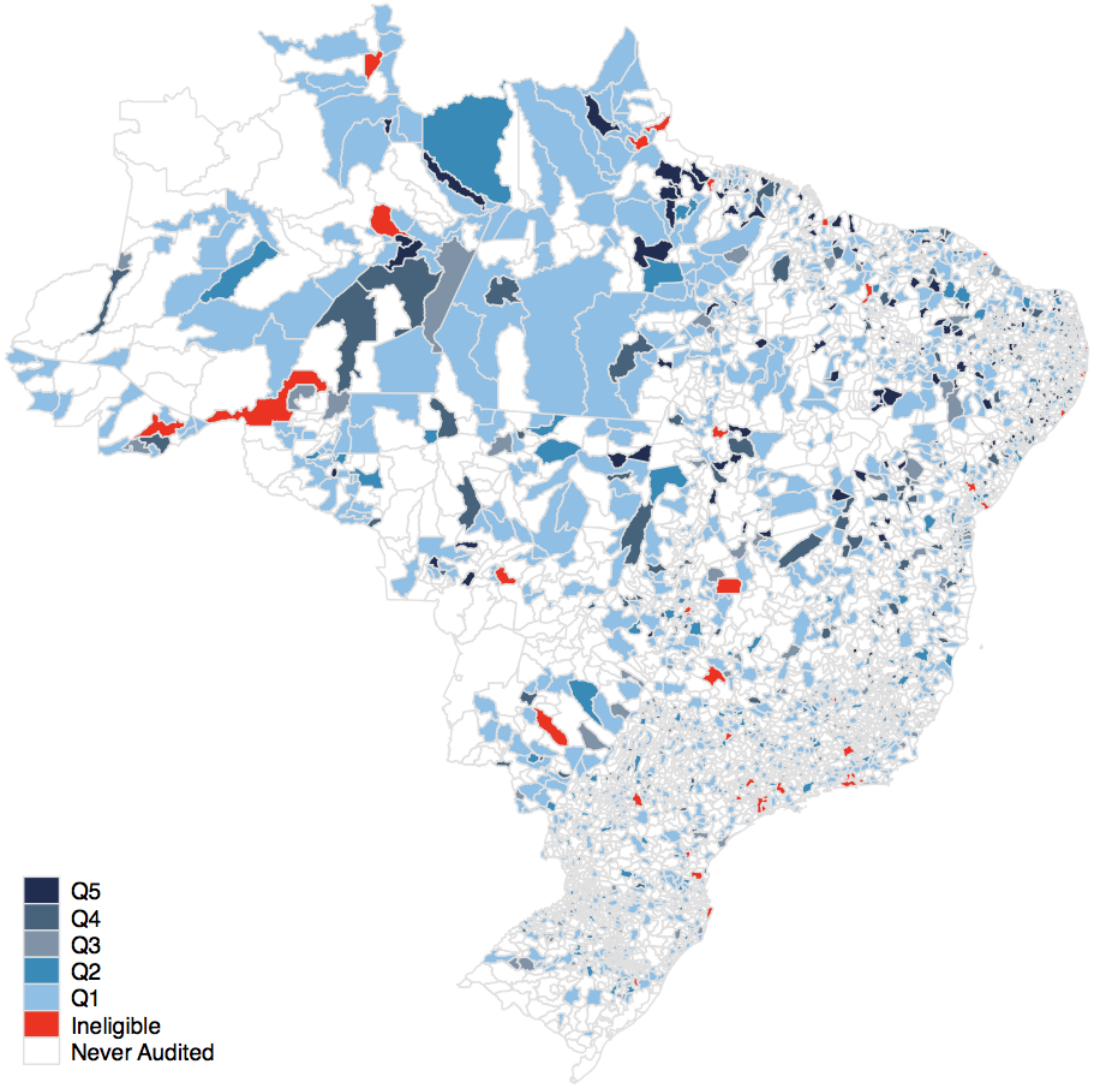
B. Audited Amount

C. Mismanagement Irregularities

D. Corruption Irregularities

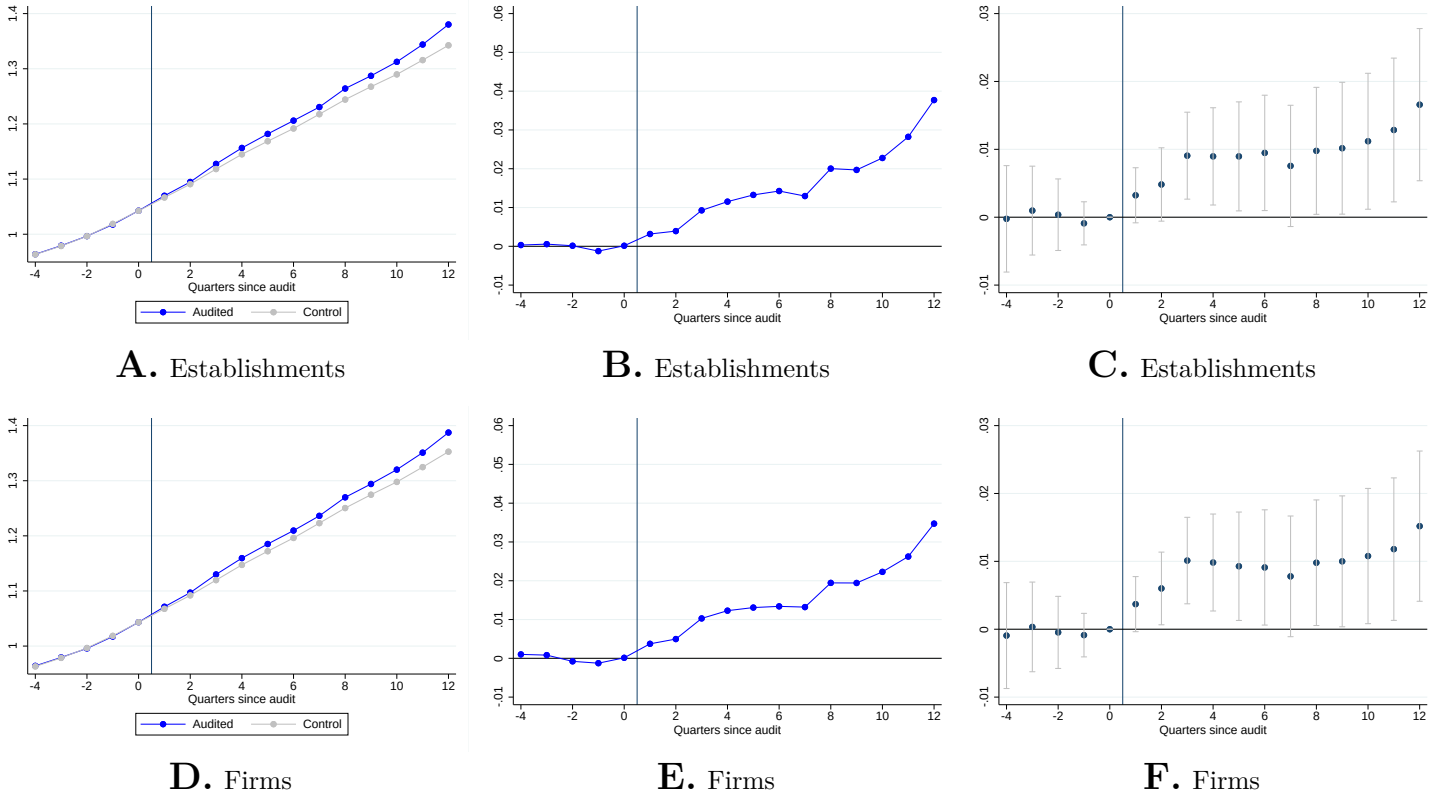
Notes: This figure illustrates the yearly variation in the anti-corruption program, using administrative data from CGU for 2006-2014. Panel A shows the total number of audits. Panel B shows the total amount of municipal resources audited, in real US millions of dollars. Panel C shows the total number of mismanagement irregularities. Panel D shows the total number of acts of corruption, which include cases of moderate and severe irregularities, as classified by CGU.

FIGURE 2. **Corruption Across Brazilian Municipalities**



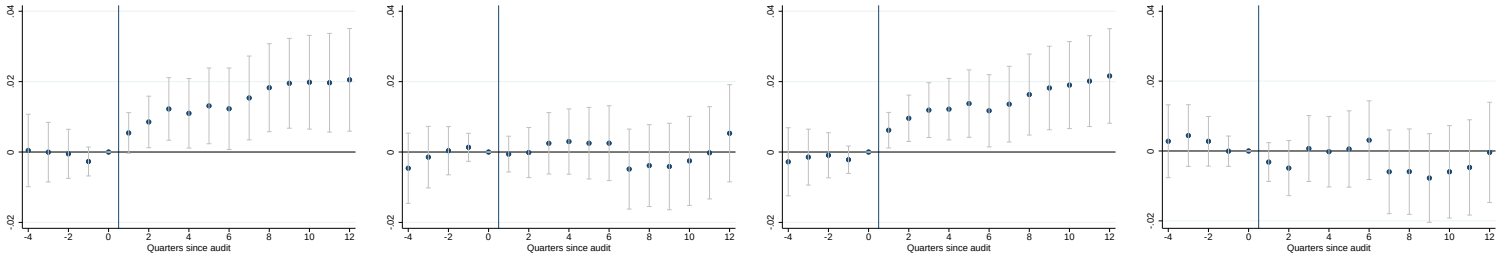
Notes: This figure shows a map of Brazilian municipalities to illustrate the spatial variation of audits and corruption intensity, using administrative data from CGU. In red, we show the municipalities that are ineligible for the program. In white, we show the municipalities that are eligible for the program but have never been selected between. In shades of blue, we highlight the municipalities that have been audited (for the first time) as part of the CGU program. A darker shade means the audit uncovered a higher share of corrupt irregularities in the audited funds, after scaling by municipality size using the number of establishments.

FIGURE 3. **Audits and Local Economy: Raw Data and Point Estimates**

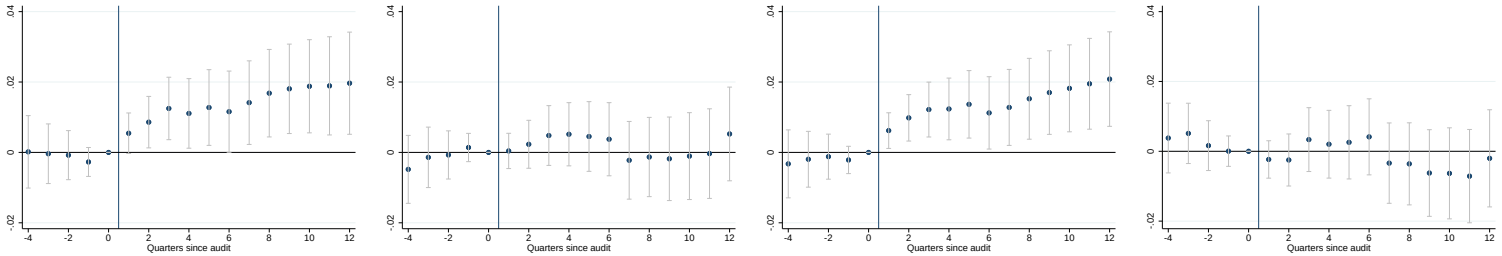


Notes: This figure reports the raw data and the dynamic coefficients obtained from the estimation of equation 4.1 together with 95% confidence intervals. The specification for Panels C and F is $y_{mt} = \alpha_m + \alpha_t + \sum_{k=-4}^{k=-1} \mu_k + \sum_{k=1}^{k=12} \mu_k + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] around the audit quarter. Panels A and D reports the raw data for total number of establishments and total number of firms in audited versus control municipalities, de-meaned using the average in the pre-audit period. Panels B and E report the difference in the de-meaned raw data between audited and control municipalities. Panels C and F presents the dynamic coefficients using as dependent variable, y_{mt}, the logarithm of the number of establishments and the logarithm of the number of firms.

FIGURE 4. **Audits and Local Economy: Government-Dependent Sectors**



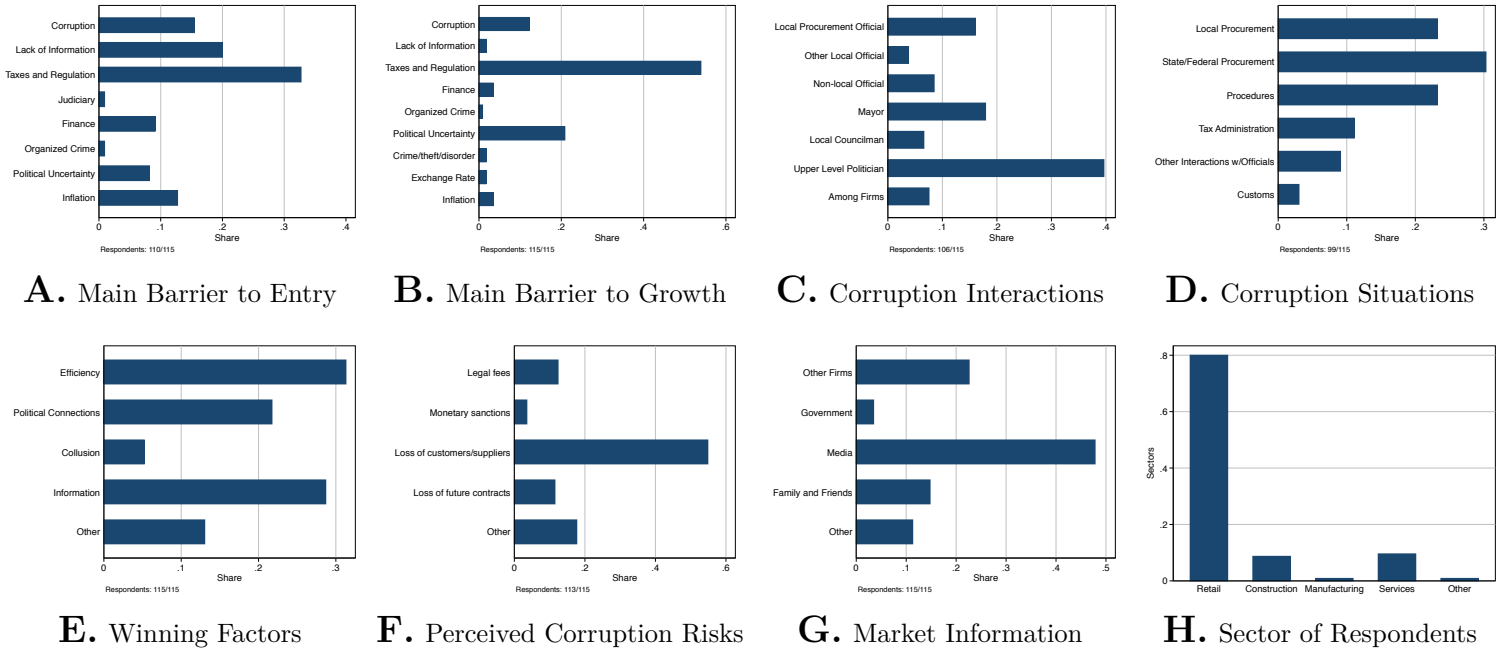
A. GD-Proc: Estab. **B.** Non-GD-Proc: Estab. **C.** GD-Corrupt: Estab. **D.** Non-GD-Corrupt: Estab.



E. GD-Proc: Firms **F.** Non-GD-Proc: Firms **G.** GD-Corrupt: Firms **H.** Non-GD-Corrupt: Firms

Notes: This figure reports the dynamic coefficients obtained from the estimation of equation 4.1 together with 95% confidence intervals. The specification is $y_{mt} = \alpha_m + \alpha_t + \sum_{k=-4}^{k=-1} \mu_k + \sum_{k=1}^{k=12} \mu_k + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] around the audit quarter. The dependent variables in each panel is the sum of the number of establishments (Panels A to D) and firms (Panels E to H) in each government-dependent sector, or its complement, as defined in section 4.2.

FIGURE 5. **Firm-Level Survey Responses**



A. Main Barrier to Entry **B.** Main Barrier to Growth **C.** Corruption Interactions **D.** Corruption Situations

E. Winning Factors **F.** Perceived Corruption Risks **G.** Market Information **H.** Sector of Respondents

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

TABLE 1. Summary Statistics: Local Economy

	(1) Mean	(2) SD	(3) p10	(4) Median	(5) p90
Panel A: Eligible municipalities					
N Business Establishments	251.11	710.67	6.00	53.00	555.00
N Firms	239.38	672.00	6.00	51.00	532.00
Establishment Size	11.21	74.63	1.00	3.00	17.00
Firm Size	16.10	169.11	1.00	3.00	18.00
Private Sector Workers	2,815.60	9,688.85	18.00	324.00	5,837.00
Wage Private Sector	460.66	1,137.98	282.50	418.13	648.01
Public Sector Workers	685.18	1,447.31	122.00	335.00	1,382.00
Wage Public Sector	513.28	1,096.10	232.22	466.16	759.66
Establishments GD-Proc	141.42	400.83	3.00	30.00	309.00
Establishments GD-Corrupt	156.23	420.21	4.00	35.00	355.00
Establishment Size GD-Proc	9.94	68.62	1.00	3.00	15.00
Establishment Size GD-Corrupt	10.53	67.37	1.00	3.00	17.00
Total Credit (M USD)	49.03	195.45	0.65	11.56	99.02
GDP per capita	4,686.43	5,492.51	1,411.96	3,401.38	8,798.00
Panel B: Audited municipalities					
N Business Establishments	248.91	691.45	6.00	52.00	575.00
N Firms	237.57	654.82	6.00	50.00	548.00
Establishment Size	11.12	79.29	1.00	3.00	17.00
Firm Size	15.78	135.36	1.00	3.00	19.00
Private Sector Workers	2,768.95	8,703.45	17.00	326.00	5,961.00
Wage Private Sector	457.22	239.78	282.70	416.53	647.28
Public Sector Workers	716.72	1,140.78	125.00	383.00	1,496.00
Wage Public Sector	497.98	727.49	224.44	459.45	747.95
Establishments GD-Proc	141.22	394.73	3.00	30.00	324.00
Establishments GD-Corrupt	156.29	415.52	4.00	34.00	370.00
Establishment Size GD-Proc	9.91	72.78	1.00	3.00	15.00
Establishment Size GD-Corrupt	10.40	70.79	1.00	3.00	17.00
Total Credit (M USD)	45.05	134.33	0.66	11.74	98.98
GDP per Capita	4,675.53	6,865.43	1,381.61	3,196.34	8,401.58

Notes: This table reports summary statistics at the municipality level, using RAIS data for the period 2002-2017. The sample in Panel A includes *all* eligible municipalities, including those that are audited. The sample in Panel B includes *only* municipalities audited as part of the CGU anti-corruption program in 2003-2014. All variables are described in the text. *N Business Establishments (Firms)* is the total number private sector establishments (firms) in the municipality, *Establishment (Firm) Size* is defined as the number of employees in the establishment (firm), *Private (Public) Sector Workers* is defined as the total number of workers in the private (public) sector, *Wage Private Sector* is the average wage in the private (public) sector, *Establishments GD-Proc (Corrupt)* is defined as the total number of establishment in GD-Procurement (Corrupt) sectors (as defined in section 4.2), *Establishments Size GD-Proc (Corrupt)* is defined as the average number of employees in establishments in GD-Procurement (Corrupt) sectors, *Total Credit (M USD)* is the total amount of credit by banks in 2016 USD, *GDP per Capita* is the GDP per capita in 2016 USD.

Table 2. Are Audits Random?

	(1)	(2)	(3)	(4)	(5)
		Characteristics in (t-1)			
	Ever Audited	03-05	06-08	09-11	12-14
Private Sector Workers	0.001	0.000	-0.002	0.002	-0.016
	(0.028)	(0.028)	(0.026)	(0.029)	(0.016)
N Business Establishments	-0.007	-0.010	0.010	-0.006	0.002
	(0.015)	(0.014)	(0.014)	(0.011)	(0.006)
Establishment Size	0.007	0.001	0.005	0.003	-0.004
	(0.005)	(0.011)	(0.008)	(0.009)	(0.002)
Share Employment in Small Establishments	-0.004	-0.010	0.001	0.000	0.003
	(0.008)	(0.007)	(0.008)	(0.007)	(0.004)
Share Employment in Medium Establishments	-0.006	-0.007	-0.004	-0.007	-0.002
	(0.008)	(0.008)	(0.008)	(0.007)	(0.004)
Share Manufacturing	0.003	0.000	0.001	0.010	0.001
	(0.008)	(0.007)	(0.005)	(0.006)	(0.004)
Share Services	-0.000	0.003	0.014*	-0.006	0.004
	(0.008)	(0.008)	(0.007)	(0.006)	(0.005)
Share Retail	0.006	0.017**	0.002	0.006	0.003
	(0.009)	(0.008)	(0.007)	(0.007)	(0.005)
Share Construction	-0.002	0.007	-0.002	-0.009*	-0.003
	(0.007)	(0.007)	(0.006)	(0.005)	(0.003)
Public Sector Workers	-0.016	0.002	-0.019	-0.029	0.007
	(0.016)	(0.018)	(0.016)	(0.018)	(0.009)
Share Workers in Public Sector	0.002	-0.030	0.003	0.019	-0.012
	(0.022)	(0.021)	(0.019)	(0.021)	(0.013)
Population	0.003	-0.005	0.000	0.019	-0.005
	(0.015)	(0.016)	(0.014)	(0.014)	(0.008)
GDP	0.025	0.031	0.005	0.018	-0.001
	(0.016)	(0.027)	(0.022)	(0.021)	(0.013)
Share Informal Employment	-0.000	0.006	0.006	-0.007	-0.005
	(0.012)	(0.011)	(0.009)	(0.008)	(0.005)
Income Gini	0.009	0.015	-0.004	0.008	0.002
	(0.007)	(0.009)	(0.007)	(0.007)	(0.004)
Share Illiterate Population	0.009	0.016	0.012	-0.008	-0.006
	(0.014)	(0.014)	(0.011)	(0.011)	(0.006)
Share Population > 8 Years Education	0.006	0.004	0.008	-0.002	-0.002
	(0.011)	(0.010)	(0.008)	(0.008)	(0.005)
Opposition Mayor		0.005	-0.009	0.002	-0.001
		(0.014)	(0.011)	(0.010)	(0.006)
Observations	5,505	4,574	4,039	3,984	3,740
R-squared	0.037	0.051	0.019	0.022	0.011
State FE	Yes	Yes	Yes	Yes	Yes
Mean DV	0.340	0.209	0.106	0.0916	0.0316

Notes: This table illustrates the randomness in the selection of municipalities to audit. All columns present the coefficients from cross-sectional regressions of the type $Audited_{ms} = \alpha_s + \gamma \times X_m + \epsilon_{ms}$. The outcome variable, $Audited_{ms}$, is an indicator for whether the municipality is audited in a given set of years as described in each column. All specifications include state fixed effects. The sample includes all eligible municipalities. We use as regressors variables measured in 2002 (Column 1), or in the year before the audit (Columns 2 to 5). We standardize each variable by their mean and standard deviation. The variables used come from the multiple data sources discussed in section 3, and they are mostly discussed in the previous table and in the text. Small establishments have up to 3 employees, while medium establishments have more than 3 but no more than 10. *Share Informal Employment*, *Income Gini*, *Share Illiterate Population*, *Share Population > 8 Years Education* come from the decennial census of 2000. *Opposition Mayor* is an indicator for whether the municipal mayor belongs to opposition party of the president's. Robust standard errors are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. The Impact of Anti-corruption Audits on the Local Economy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			GD-Procurement		Non-GD-Procurement		GD-Corrupt		Non-GD-Corrupt	
	Establishments	Firms	Establishments	Firms	Establishments	Firms	Establishments	Firms	Establishments	Firms
PostAudit	0.009**	0.009**	0.014***	0.014***	0.001	0.003	0.015***	0.015***	-0.005	-0.004
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392
R-squared	0.984	0.985	0.977	0.977	0.979	0.980	0.979	0.979	0.978	0.979
p-value GD vs Non-GD			0.049	0.095	0.049	0.095	0.001	0.002	0.001	0.002
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	4.179	4.138	3.607	3.593	3.370	3.309	3.748	3.734	3.142	3.070
SD Dep Var	1.611	1.607	1.606	1.598	1.602	1.595	1.599	1.590	1.616	1.609

Notes: This table illustrates the main effects of the audits 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 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarter-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-quarter fixed effects. *Establishments* is the log of the total number of private sector establishments in the municipality. *Firms* is the log of the total number of private sector firms in the municipality. Dependent variables in columns 1 and 2 refer to all sectors in the municipality, columns 3 and 4 to *GD-Procurement* sectors, columns 5 and 6 to *Non-GD-Procurement* sectors, columns 7 and 8 to *GD-Corrupt* sectors, and columns 9 ad 10 to *Non-GD-Corrupt* sectors. These classifications are defined in section 4.2. The *p-value GD vs Non-GD* presents the p-value for the difference between *PostAudit* coefficients in GD vs Non-GD sectors. *Avg Dep Var* and *SD Dep Var* are computed using eligible non-audited municipalities and audited municipalities in the 4 quarters before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4. The Impact of Audits on Other Measures of Economic Activity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Firm Census and Financial Development						
	PAC/PAS		BCB			
	Sales	Investment	Credit	Deposits		
PostAudit	0.062*	0.148	0.024*	0.032**		
	(0.033)	(0.096)	(0.014)	(0.013)		
Observations	25,112	25,112	43,185	43,185		
R-squared	0.865	0.772	0.905	0.893		
Municipality FE	Yes	Yes	Yes	Yes		
Year FE	Yes	Yes	Yes	Yes		
Avg Dep Var	15.84	1.190	15.67	14.63		
SD Dep Var	2.280	3.735	2.008	1.488		
Panel B: Formal and Informal Employment						
	PNAD (Pop. share)		PNAD (Inf. share)		Decennial Census	
	Informal	Formal	Informal	Formal	Informal	Formal
PostAudit	0.020***	0.009***	0.018***	0.008***	0.046***	0.012
	(0.003)	(0.002)	(0.002)	(0.001)	(0.011)	(0.009)
Observations	56,015	56,015	56,015	56,015	10,840	10,840
R-squared	0.989	0.993	0.997	0.999	0.971	0.987
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	7.602	7.631	7.759	7.128	7.490	7.016
SD Dep Var	1.054	1.097	1.158	1.462	0.977	1.311

Notes: This table illustrates the main effects of the audit on other measures of local economic activity. The table mostly 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 2003-2014 and all eligible non-audited municipalities, and covers the window [-1,+3] years 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. *Sales* is the logarithm of (the sum of) total sales by local firms in the PAC and PAS datasets. *Investment* is defined as the (sum of) total capital expenditure over total assets by local firms in the PAC and PAS datasets. *Credit* and *Deposits* use municipality-level data from the Central Bank and are defined in section 3. The construction of dependent variables in Panel B, which measure informal and formal employment, is discussed in section 3. Columns 5 and 6 estimate an analogous difference-in-difference specification using two data points, namely the decennial censuses of 2000 and 2010. *Avg Dep Var* and *SD Dep Var* are computed using eligible non-audited municipalities and audited municipalities in the year before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. The Characteristics of New Firms

	(1) Employment	(2) Payroll per Employee	(3) Future 3-yr Emp. Growth	(4) Future 5-yr Emp. Growth	(5) Workers with HS education	(6) Manager with HS education	(7) Worker Ability (AKM)	(8) Manager Ability (AKM)	(9) Share Emp. from Inf./Unemp.
PostAudit	-0.003	0.001	-0.010	-0.022	0.001	0.004	-0.000	0.020**	0.001
	(0.004)	(0.002)	(0.015)	(0.021)	(0.002)	(0.005)	(0.001)	(0.009)	(0.002)
Observations	2,432,496	2,432,496	1,258,071	886,829	2,432,496	270,946	2,087,518	244,044	2,432,496
R-squared	0.139	0.309	0.042	0.046	0.085	0.095	0.002	0.094	0.082
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
3 Digit Sector-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.658	5.894	1.288	1.768	0.641	0.538	0.003	0.010	0.585
SD Dep Var	0.853	0.427	2.577	3.466	0.409	0.479	1.184	1.032	0.421

Notes: This table illustrates the impact of the audit on the characteristics of firms in their year of entry. The table reports the coefficients obtained from the estimation of equation 4.4. The specification is $y_{fsmt} = \alpha_m + \alpha_t + \alpha_{st} + \beta \times PostAudit_{mt} + \epsilon_{mt}$. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-1,+3] years 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 3-digit sector-by-year fixed effects. *Employment* is defined as the logarithm of total employment in the year of entry, *Payroll per Employee* is defined as total payroll over total employment in the year of entry, *Future 3(5)-yr Emp. Growth* is defined as employment in year 3 (5) over employment in the year of entry minus one, *Workers (Managers) with HS education* is defined as the share of all employees (managers) who completed at least high-school education, *Worker (Manager) AKM* is defined as the average worker (managers) fixed effect estimated following Abowd et al., 1999, *Share Emp. from Inf./Unemp* is defined as the share of employees in the first year who were not employed in the formal sector in the previous year. *Avg Dep Var* and *SD Dep Var* are computed using eligible non-audited municipalities and audited municipalities in the year before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6. Heterogenous Effects of the Audits

	(1)	(2)	(3)	(4)	(5)
				Audited Microregion	
	High Corruption Municipalities	Media Coverage	Year Before Election		Media Coverage
PostAudit × Z	0.061***	0.013*	0.014*		0.032***
	(0.009)	(0.008)	(0.008)		(0.006)
PostAudit	-0.030***	0.004	0.005	0.010**	0.010*
	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)
Observations	260,456	277,392	277,392	69,866	69,866
R-squared	0.984	0.984	0.984	0.993	0.994
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	4.187	4.179	4.179	4.179	4.076
SD Dep Var	1.608	1.611	1.611	1.682	1.682

Notes: This table illustrates the heterogeneous effects of the audit on the local economy. The table reports the coefficients obtained from the estimation of equation 4.3. The specification is $y_{mt} = \alpha_m + \alpha_t + \gamma \times Z_m \times PostAudit_{mt} + \beta \times PostAudit_{mt} + \epsilon_{mt}$ and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. The dependent variable is the log of the total number of private sector establishments in the municipality. $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarter-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-quarter fixed effects. Z_m are fixed municipality characteristics that include: *High Corruption Municipalities*, an indicator for municipalities where the audit uncovers above-median irregularities (scaled by municipality size) based on administrative data from CGU; *Media coverage*, an indicator for municipalities whose audit is directly covered by the media (as described in the text); *Year Before Election*, an indicator equal to one if the audit takes place the year before a municipal election. Columns 4 and 5 estimate a similar specification for the impact of the audit on nearby municipalities, i.e. municipalities in the same micro-region of an audited one, as described in the text. This regressions exclude audited municipalities, and the treatment dummy is equal to one the first time a micro-region has one of its municipality that is audited. In column 5, *Media Coverage* is the number of audited municipalities in the micro-region with media coverage dummy equal to one. *Avg Dep Var* and *SD Dep Var* are computed using eligible non-audited municipalities and audited municipalities in the year before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7. Incumbent Government-Dependent Firms

	(1) GD-Proc	(2) GD-Corr	(3) GD-Procurement	(4)	(5) GD-Corrupt	(6)
	Ln Emp	Ln Emp	Sales per Emp	Investment	Sales per Emp	Investment
PostAudit × Incumbent × Z	0.013***	0.013***				
	(0.004)	(0.004)				
PostAudit × Incumbent	0.007	0.006				
	(0.004)	(0.004)				
PostAudit × Z	0.010***	0.011***	0.048***	0.020**	0.039***	0.015**
	(0.002)	(0.003)	(0.014)	(0.008)	(0.015)	(0.007)
PostAudit	-0.016***	-0.017***	0.006	-0.024***	0.010	-0.021**
	(0.002)	(0.003)	(0.016)	(0.008)	(0.013)	(0.009)
Observations	73,461,266	73,461,266	312,430	134,713	312,430	134,713
R-squared	0.831	0.831	0.927	0.429	0.918	0.429
Avg Dep Var	1.286	1.286	10.09	0.176	10.09	0.176
SD Dep Var	1.163	1.163	1.669	0.280	1.669	0.280
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table illustrates the heterogeneous effects of the audit across local firms. The table reports the coefficients obtained from the estimation of equation 5.1. The specification is $y_{fmt} = \alpha_f + \alpha_t + \gamma_1 \times Z_f \times Incumbent_f \times PostAudit_{mt} + \gamma_2 \times Incumbent_f \times PostAudit_{mt} + \gamma_3 \times Z_f \times PostAudit_{mt} + \beta \times PostAudit_{mt} + \epsilon_{fmt}$, and is discussed in Section 5.1. The sample includes all establishments in municipalities audited in the period 2003-2014 and all establishments in eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. *Ln Emp* is the log of total employment in the establishment; *Sales per Emp* is the logarithm of total sales over total employment; *Investment* is defined as total capital expenditure over total assets. $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarter-years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. $Incumbent_f$ is an indicator variable equal to one for firms that are alive at the time of the audit and in the previous four quarters. Z_f are two establishment characteristics, as indicated in the top labels: *GD-Procurement* (columns 1, 3, 4) is an indicator for belonging to a GD-Procurement sector; *GD-Corrupt* (columns 2, 5, 6) is an indicator for belonging to a GD-Corrupt sector. All specifications include establishment and year-quarter fixed effects. *Avg Dep Var* and *SD Dep Var* are computed using establishments in eligible non-audited municipalities and audited municipalities in the year before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 8. The Impact of Audits on Politically Connected Firms

	(1)	(2)	(3)	(4)	(5)
	Politically Connected Firms				
	Firm Donor	Manager Donor	Politician	Candidate	Party Member
PostAudit × Incumbent × Z	-0.033**	-0.160***	-0.324***	-0.182***	-0.101***
	(0.015)	(0.030)	(0.106)	(0.038)	(0.007)
PostAudit × Incumbent	0.015***	0.015***	0.015***	0.015***	0.023***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
PostAudit	-0.010***	-0.010***	-0.010***	-0.010***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	73,461,266	73,461,266	73,461,266	73,461,266	73,461,266
R-squared	0.831	0.831	0.831	0.831	0.831
Establishment FE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	1.286	1.286	1.286	1.286	1.286
SD Dep Var	1.163	1.163	1.163	1.163	1.163

Notes: This table illustrates the heterogeneous effects of the audit across local firms. The table reports the coefficients obtained from the estimation of equation 5.1. The specification is $y_{fmt} = \alpha_f + \alpha_t + \gamma_1 \times Z_f \times Incumbent_f \times PostAudit_{mt} + \gamma_2 \times Incumbent_f \times PostAudit_{mt} + \gamma_3 \times Z_f \times PostAudit_{mt} + \beta \times PostAudit_{mt} + \epsilon_{fmt}$, and is discussed in Section 5.1. The sample includes all establishments in municipalities audited in the period 2003-2014 and all establishments in eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. The dependent variable is the log of the total employment at the establishment-level. $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarter-years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. All specifications include establishment and year-quarter fixed effects. $Incumbent_f$ is an indicator variable equal to one for firms that are alive at the time of the audit and in the previous four quarters. Z_f are establishment indicator variables that capture a firm's political connection status, measured at any time up to the audit: *Firm Donor* for firm that donated to a political campaign, *Manager Donor* for firms whose manager donated to a political campaign, *Politician* for firms whose manager was an elected politician, *Candidate* for firms whose manager was a political candidate (elected or not), *Party Member* for firms whose manager was a registered member of a political party. *Avg Dep Var* and *SD Dep Var* are computed using establishments in eligible non-audited municipalities and audited municipalities in the year before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Firm-Level Survey Responses

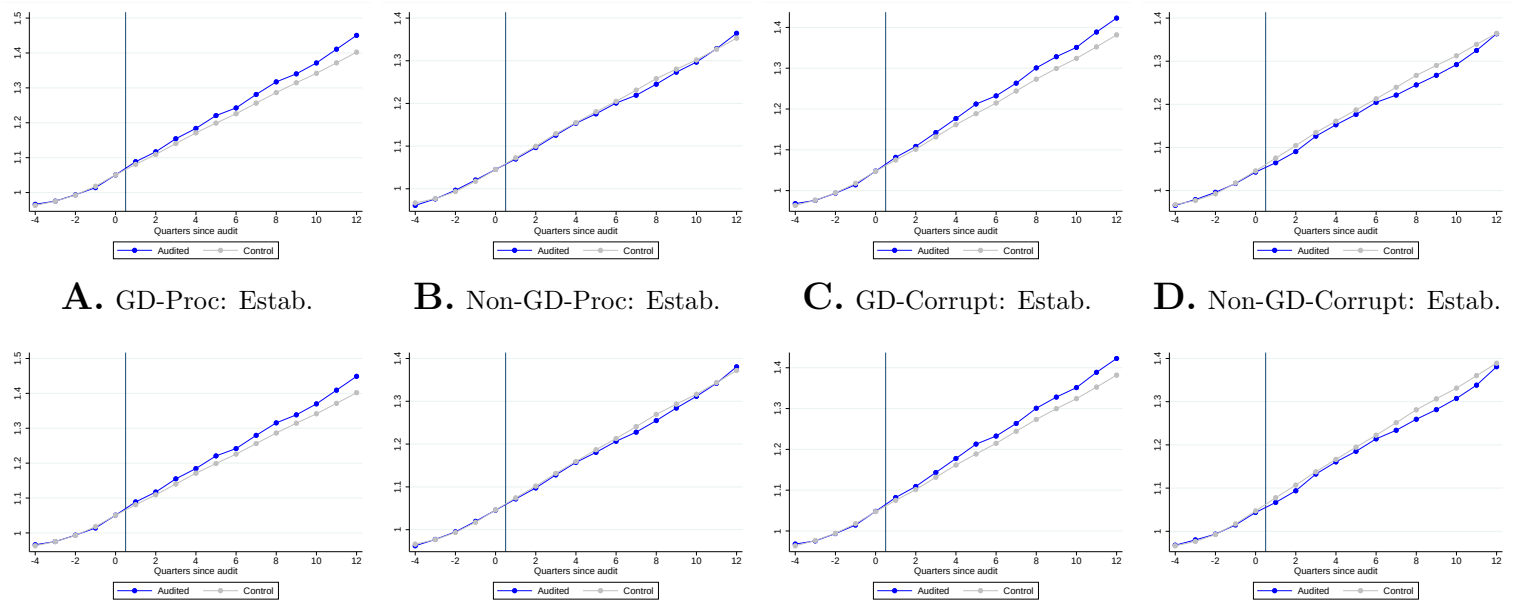
Question	Share	Responses	Don't Know
Corruption and Firm Strategy			
Does the presence of corruption affect your business operations or those of firms in your sector?	0.97	115	0
Does the presence of corruption affect investment and innovation?	0.82	115	0
Does the presence of corruption affect cash holdings and allocation of financial resources within the firm?	0.79	115	0
Does the presence of corruption affect decisions to expand to new markets and products?	0.77	115	0
Does the presence of corruption affect bidding strategy for public procurement contracts?	0.68	115	0
Does the presence of corruption affect contracts with private sector firms?	0.53	115	0
Does the presence of corruption affect hiring and firing activity and employee selection?	0.50	115	0
Does the presence of corruption affect organizational structure, delegation of power, and allocation of jobs and tasks?	0.29	115	0
In the absence of corruption, do you think your firm would be able to grow more than 10%?	0.65	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 Perceptions			
Do firms in your industry know in advance the precise amount necessary for extra unofficial payments to public officials?	0.21	115	0
When firms in your industry do business with the government, what percent of the contract value would typically need to be paid in additional or unofficial payments/gifts, in order to secure the contract?	5.78	14	101
What do you think is the percentage of firms doing public procurement in your sector who directly witnessed or were affected by a case of corruption?	53.27	47	68
What do you think is the percentage of local public procurement contracts affected by corruption?	47.35	57	58
Would you be able to compete for public procurement contracts without making unofficial payments to public officials?	0.55	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
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
Public Procurement			
Is the market for public procurement contracts in your industry competitive?	0.75	115	0
What percentage of your sales are accounted for by public procurement contracts?	7.22	105	10
In the hypothetical scenario in which you lose access to public procurement contracts, would you be able to maintain the same level of sales with only private sector contracts?	0.83	115	0
Do you 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. state, federal government, other public entities)?	0.09	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. When not otherwise specified, the column "Share" indicates the share of "Yes" to each question. The column "Responses" indicates the number of responses, while "Don't Know" represent the remaining number of firms who opt not to respond to that specific question.

APPENDIX

Appendix A.1. Figures and Tables

Figure A1. **Government-Dependent Sectors: Raw Data**

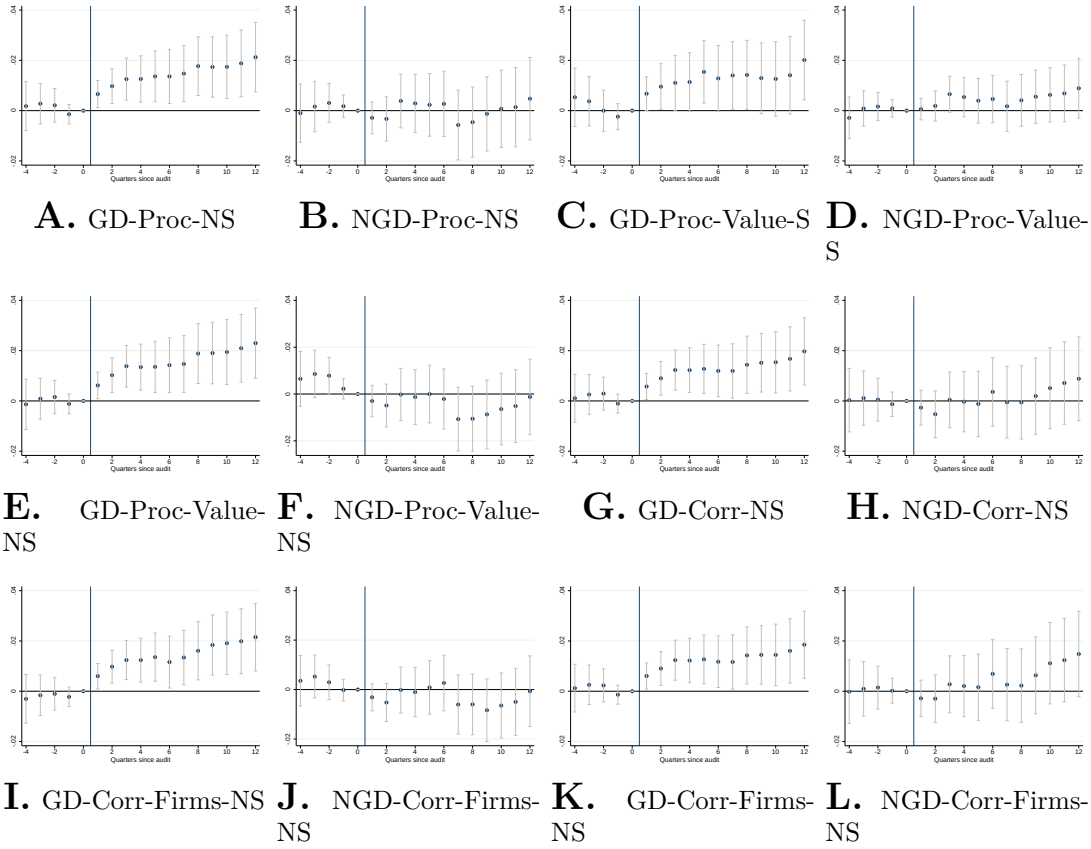


A. GD-Proc: Estab. **B.** Non-GD-Proc: Estab. **C.** GD-Corrupt: Estab. **D.** Non-GD-Corrupt: Estab.

E. GD-Proc: Firms **F.** Non-GD-Proc: Firms **G.** GD-Corrupt: Firms **H.** Non-GD-Corrupt: Firms

Notes: This figure reports the raw data for the log of the total number of private sector establishments (Panels A to D) and firms (Panels E to H) in each government-dependent sector, or its complement, as defined in section 4.2, and split between audited and control municipalities. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter.

FIGURE A2. **Government Dependence Sectors (Establishments): Alternative Measures**



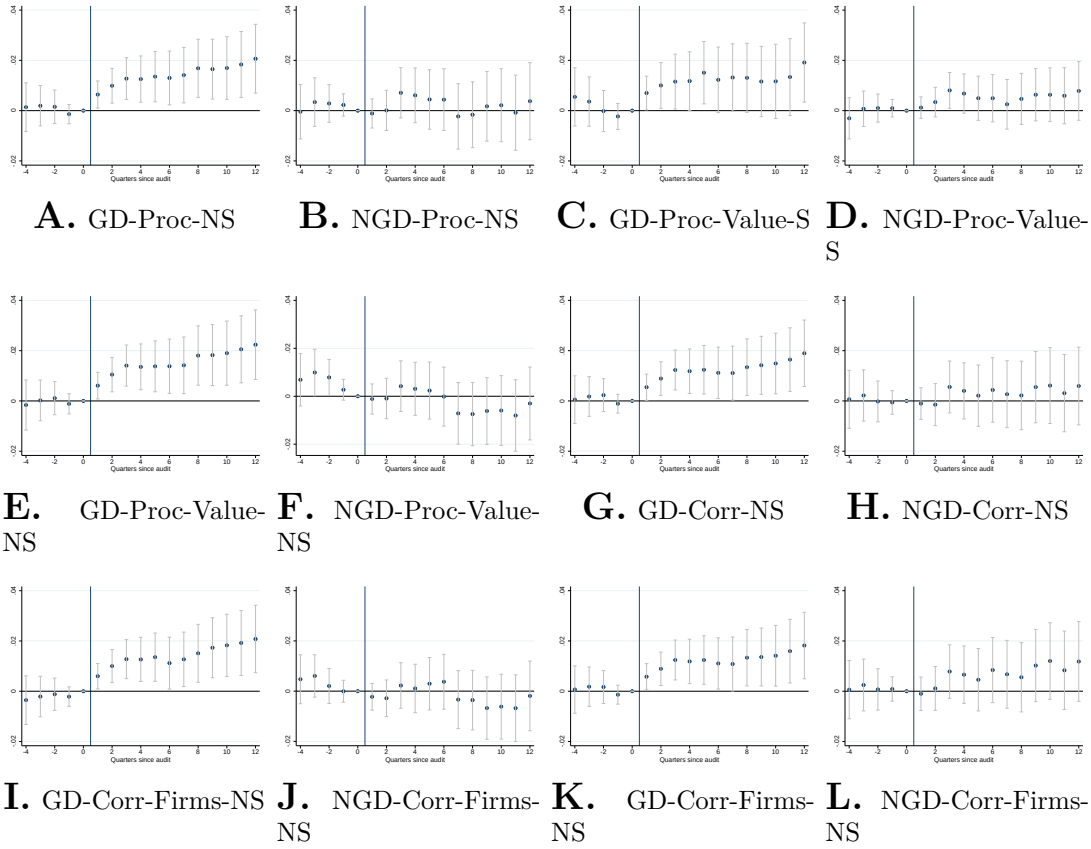
A. GD-Proc-NS **B.** NGD-Proc-NS **C.** GD-Proc-Value-S **D.** NGD-Proc-Value-S

E. GD-Proc-Value-NS **F.** NGD-Proc-Value-NS **G.** GD-Corr-NS **H.** NGD-Corr-NS

I. GD-Corr-Firms-NS **J.** NGD-Corr-Firms-NS **K.** GD-Corr-Firms-NS **L.** NGD-Corr-Firms-NS

Notes: This figure reports the dynamic coefficients obtained from the estimation of equation 4.1 together with 95% confidence intervals, for different measures of government-dependent sectors (and their complement). The specification is $y_{mt} = \alpha_m + \alpha_t + \sum_{k=-4}^{k=-1} \mu_k + \sum_{k=1}^{k=12} \mu_k + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. The dependent variable is the logarithm of private sector establishments. *GD* and *NGD* stand for government-dependent and non-government-dependent, respectively, and all alternative measures based on them are described in section 4.2.

FIGURE A3. **Government-Dependent Sectors (Firms): Alternative Measures**



A. GD-Proc-NS **B.** NGD-Proc-NS **C.** GD-Proc-Value-S **D.** NGD-Proc-Value-S

E. GD-Proc-Value-NS **F.** NGD-Proc-Value-NS **G.** GD-Corr-NS **H.** NGD-Corr-NS

I. GD-Corr-Firms-NS **J.** NGD-Corr-Firms-NS **K.** GD-Corr-Firms-NS **L.** NGD-Corr-Firms-NS

Notes: This figure reports the dynamic coefficients obtained from the estimation of equation 4.1 together with 95% confidence intervals, for different measures of government-dependent sectors (and their complement). The specification is $y_{mt} = \alpha_m + \alpha_t + \sum_{k=-4}^{k=-1} \mu_k + \sum_{k=1}^{k=12} \mu_k + \epsilon_{mt}$, and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. The dependent variable is the logarithm of private sector firms. *GD* and *NGD* stand for government-dependent and non-government-dependent, respectively, and all alternative measures based on them are described in section 4.2.

Table A1. Correlation Across Government Dependence Measures

	GD-Procurement				GD-Corrupt			
	Contracts scaled	Contracts unscaled	Value scaled	Value unscaled	Contracts scaled	Contracts unscaled	Firms scaled	Firms unscaled
GD-Proc-Contracts scaled	1							
GD-Proc-Contracts unscaled	0.588	1						
GD-Proc-Value scaled	0.556	0.338	1					
GD-Proc-Value unscaled	0.453	0.633	0.582	1				
GD-Corr-Contracts scaled	0.355	0.349	0.290	0.336	1			
GD-Corr-Contracts unscaled	0.195	0.433	0.103	0.347	0.558	1		
GD-Corr-Firms scaled	0.311	0.259	0.220	0.239	0.781	0.394	1	
GD-Corr-Firms unscaled	0.225	0.456	0.106	0.344	0.527	0.868	0.410	1

Notes: This table reports the correlation matrix for all measures of government dependence we create, as discussed in section 4.2.

TABLE A2. Most Common Government-Dependent Sectors

Panel A: GD-Procurement

Wholesale of materials for medical, surgical, orthopedic and dental use
Wholesale of office and stationery supply (books, newspapers and others)
Wholesale of machinery and equipment for dental/medical/hospital use
Wholesale of pharmaceutical products
Manufacturing of medicines
Retail of medical and orthopedic articles
Manufacturing of chemicals
Wholesale of computers and related supplies
Manufacturing of industrial gases
Wholesale of electrical equipment
Manufacturing of computer equipment
Manufacturing of pharmaceutical products
Manufacturing materials for medical, dental and optical use
Wholesale of petroleum and gas
Retail of books, newspapers, magazines and stationery
Manufacturing of computer peripherals
Manufacturing of timing equipment
Retail of electrical equipment
Wholesale of hardware and tools
Manufacturing of optical, photographic and cinematographic equipment and instruments
Wholesale of chemical and petrochemical products
Wholesale of general merchandise, excluding food or agricultural inputs
Wholesale food products
Wholesale of building materials
Retail of specialized of computer equipment and supplies

Panel B: GD-Corrupt

Wholesale of pharmaceutical products
Maintenance and repair of railway vehicles
Construction of highways and railways
Railway passenger transport
Manufacturing of cars, vans and utilities
Wholesale of health supplies
Treatment and disposal of non-hazardous waste
Collective road passenger transport
Wholesale trade of general merchandise, mainly food products
Retail of fuel
Patient removal services
Retail of food products (supermarkets)
Civil engineering works
Construction of buildings
Manufacturing of juices
Construction scrapers
Wholesale of processed cereals, pulses, flour and starch
Foundation works
Urbanization works - streets, squares and sidewalks
Electricity and telecommunications generation/distribution works
Retail of books, newspapers and stationery
Construction of water supply systems, sewage collection and related activities
Wholesale of office and stationery supply (books, newspapers and others)
Manufacturing of medicines
Wholesale of machinery and equipment for dental/medical/hospital use

Notes: This table reports some of the most common 4-digit sectors we classify as government-dependent, split by our two primary measures of GD-Procurement and GD-Corrupt, as discussed in section 4.2. We select the top 25 sectors, excluding specific ones for which the translation and interpretation is less clear.

TABLE A3. The Impact of Audits on the Local Economy: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No Window Restriction		Drop Audited Twice		Audited Post-2004		Audited Pre-2012	
	Establishments	Firms	Establishments	Firms	Establishments	Firms	Establishments	Firms
Panel A								
PostAudit	0.034***	0.034***	0.009**	0.010**	0.011**	0.012**	0.010**	0.011***
	(0.008)	(0.008)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
Observations	372,513	372,513	273,293	273,293	266,054	266,054	275,369	275,369
R-squared	0.983	0.984	0.984	0.985	0.984	0.984	0.984	0.985
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	4.127	4.086	4.180	4.139	4.181	4.141	4.178	4.137
SD Dep Var	1.624	1.619	1.611	1.606	1.611	1.606	1.612	1.607
Panel B								
	Ever Audited		Control for Audit Probability		No Winsorization		5% Winsorization	
	Establishments	Firms	Establishments	Firms	Establishments	Firms	Establishments	Firms
PostAudit	0.024***	0.026***	0.009**	0.009**	0.008**	0.009**	0.006*	0.007*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	31,725	31,725	277,392	277,392	277,392	277,392	277,392	277,392
R-squared	0.994	0.994	0.984	0.985	0.985	0.985	0.983	0.984
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	3.991	3.944	4.179	4.138	4.180	4.140	4.159	4.119
SD Dep Var	1.642	1.635	1.611	1.607	1.616	1.611	1.517	1.512

Notes: This table illustrates the robustness of the main effects of the audit on the local economy. The table reports the coefficients obtained from the estimation of equation 4.2. The baseline specification is $y_{mt} = \alpha_m + \alpha_t + \beta \times PostAudit_{mt} + \epsilon_{mt}$, and is discussed in Section 4.1. Unless otherwise specified and discussed in section 4.2, the sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarter-years after the audit in the audited municipality, and 0 otherwise. $PostAudit_{mt}$ is always 0 for never treated municipalities. *Establishments (Firms)* is the log of the total number of private sector establishments (firms) in the municipality. The various robustness tests are discussed in section 4.2. Notice that in the specification where we drop the never-audited municipalities (i.e., columns 1 and 2 of Panel B, we include year rather than year-quarter fixed effects; all other specifications include municipality and year-quarter fixed effects. *Avg Dep Var* and *SD Dep Var* are computed using data in the 4 quarters before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A4. The Impact of Audits on the Local Economy: Robustness to Alternative Measures of Government-Dependent Sectors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	GD-Procurement						GD-Corrupt					
	Contracts unscaled		Value scaled		Value unscaled		Contracts unscaled		Firms scaled		Firms unscaled	
	GD	Non-GD	GD	Non-GD	GD	Non-GD	GD	Non-GD	GD	Non-GD	GD	Non-GD
Panel A: Establishments												
PostAudit	0.012**	-0.001	0.010*	0.004	0.014***	-0.010*	0.011**	0.001	0.015***	-0.006	0.011**	0.004
	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392
R-squared	0.981	0.960	0.975	0.983	0.981	0.961	0.982	0.950	0.979	0.978	0.982	0.949
Avg Dep Var	3.960	2.501	3.118	3.786	3.945	2.565	4.020	2.200	3.732	3.170	4.018	2.203
SD Dep Var	1.692	1.301	1.647	1.551	1.691	1.332	1.679	1.232	1.594	1.624	1.681	1.224
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Firms												
PostAudit	0.012***	-0.000	0.010*	0.005	0.014***	-0.009*	0.011**	0.002	0.015***	-0.005	0.011**	0.005
	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392	277,392
R-squared	0.981	0.963	0.975	0.983	0.981	0.964	0.982	0.953	0.979	0.979	0.982	0.952
Avg Dep Var	3.943	2.405	3.107	3.738	3.929	2.469	4.001	2.089	3.718	3.099	4	2.090
SD Dep Var	1.683	1.267	1.640	1.544	1.682	1.306	1.670	1.183	1.585	1.617	1.672	1.173
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table illustrates the robustness of the main effects of the audit on the local economy, using various alternative definitions of government-dependent sectors. 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 2003-2014 and all eligible non-audited municipalities, and covers the window [-4,+12] quarters around the audit quarter. $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarter-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-quarter fixed effects. *Establishments (Firms)* is the log of the total number of private sector establishments (firms) in the municipality. The various definitions of government dependence are discussed in section 4.2. *Avg Dep Var* and *SD Dep Var* are computed using eligible non-audited municipalities and audited municipalities in the 4 quarters before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Satellite Night-time Lights and GDP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Window: [-1,3]			Window: [-3,5]			Window: Unconstrained		
	Night-time Lights	GDP Growth	Ln GDP pc	Night-time Lights	GDP Growth	Ln GDP pc	Night-time Lights	GDP Growth	Ln GDP pc
PostAudit	0.083	0.003	0.001	0.114**	-0.001	0.003	0.120**	-0.001	0.009*
	(0.082)	(0.003)	(0.004)	(0.057)	(0.002)	(0.004)	(0.051)	(0.002)	(0.005)
Observations	54,206	67,596	67,641	60,529	74,631	74,679	69,157	88,280	88,337
R-squared	0.156	0.073	0.949	0.148	0.068	0.948	0.141	0.063	0.944
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	0.242	0.0394	8.149	0.120	0.0435	8.126	0.0815	0.0426	8.090
SD Dep Var	3.892	0.140	0.717	3.474	0.141	0.717	3.497	0.142	0.717

Notes: This table illustrates the effects of the audit on the local economy, using outcome data on satellite night-time lights and GDP, measured at the yearly level. The table reports the coefficients obtained from the estimation of equation 4.2. The specification is $y_{mt} = \alpha_m + \alpha_t + \gamma \times Z_m \times PostAudit_{mt} + \beta \times PostAudit_{mt} + \epsilon_{mt}$ and is discussed in Section 4.1. The sample includes all municipalities audited in the period 2003-2014 and all eligible non-audited municipalities. Columns 1 to 3 cover the window [-1,+3] years around the audit year, Columns 4 to 6 covers the window [-3,+5], and Columns 7 to 9 do not impose any restriction on the window around the audit. $PostAudit_{mt}$ 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-quarter fixed effects. *Night-time Lights* is a standardized measure of local night-time lights, defined in section 3; *GDP Growth* is defined as the yearly growth in real GDP; *Ln GDP pc* is defined as the logarithm of GDP per capita. *Avg Dep Var* and *SD Dep Var* are computed using eligible non-audited municipalities and audited municipalities in the year before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A6. Does the Federal Government Channel More Resources to Audited Municipalities?

	(1)	(2)	(3)	(4)	(5)	
	Transfers			Grants		
	Federal Capital Transfers	Federal Revenue Transfers	Municipal Participation Fund	Total Value	Share of Funds Disbursed	Total Value of Federal Procurement
PostAudit	-0.135	0.002	0.021	-0.052	-0.001	0.045
	(0.101)	(0.063)	(0.063)	(0.097)	(0.008)	(0.061)
Observations	71,332	71,332	71,332	71,332	71,332	71,332
R-squared	0.282	0.259	0.237	0.322	0.248	0.604
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	2.864	14.69	14.37	10.40	0.621	1.869
SD Dep Var	5.029	2.765	2.762	4.888	0.390	4.429

Notes: This table illustrates the main effects of the audit on resources channeled by the federal government to municipalities. 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 2003-2014 and all eligible non-audited municipalities, and covers the window [-1,+3] years 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. *Federal Capital Transfers* are intergovernmental transfers of revenues from capital, *Federal Revenue Transfers* are intergovernmental transfers of current revenues, *Municipal Participation Fund* are federal government transfers from income and production taxes distributed according to the number of inhabitants, *Total Value* is the total value of block grants transferred from the federal to local governments, and *Share of Funds Disbursed* is the share of funds disbursed out of the grant's total amount. *Total Value of Federal Procurement* is the total value of federal procurement contracts granted to firms headquartered in the municipality. The data come from the CGU and the National Treasury's FIMBRA dataset, and all measures except column 5 are in logs. *Avg Dep Var* and *SD Dep Var* are computed using eligible non-audited municipalities and audited municipalities in the year before the audit. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Table A7. The Impact of Audits on Firm Death

VARIABLES	(1) Full Sample	(2) GD-Procurement	(3) non-GD-Procurement	(4) GD-Corrupt	(5) non-GD-Corrupt	(6) Pol. Connected	(7) non-Pol. Connected
PostAudit	0.004	0.001	0.006	0.003	-0.001	0.002	0.002
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.006)
Observations	277,392	277,392	277,392	277,392	277,392	277,392	277,392
R-squared	0.866	0.835	0.818	0.841	0.808	0.558	0.865
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	1.155	0.856	0.753	0.931	0.657	0.159	1.130
SD Dep Var	1.221	1.057	0.996	1.094	0.943	0.417	1.209

Notes: This table illustrates the main effects of the audit on firm death in the municipality. 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 2003-2014 and all eligible non-audited municipalities and covers the window [-4,+12] quarters around the audit quarter. The dependent variable is the logarithm of total establishments that die (i.e., exit RAIS) in a given year. $PostAudit_{mt}$ is an indicator variable taking value 1 for all quarter-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-quarter fixed effects. Robust standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.