The Employee Costs of Corporate Debarment in Public Procurement[*][†][‡]

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Abstract

This paper studies an increasingly popular anti-corruption policy — corporate debarment or blacklisting — to understand how both disclosing illicit corporate practices and the sanctions for these practices affect firm and worker outcomes. I exploit a unique policy change in Brazil that imposed stricter penalties for corrupt firms. I combine the universe of firms that were publicly debarred and excluded from public procurement with detailed matched employer-employee administrative data. Using a matched difference-in-differences approach, I find that debarment is associated with a sizable decline in employment and an increase in the probability of exiting the formal sector. I also document that workers' annual earnings fall by about 22 percent after debarment. The impacts are driven by lost revenues from government contracts. Workers who have previously worked in debarred firms also experience earnings losses. The results shed light on the costs to workers when their employers are debarred in weighing the consequences of corruption crackdown.

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1 Introduction

Corporate debarment or blacklisting is an important anti-corruption measure, providing an instrument for governments to punish companies for corrupt practices. Due to the costs that corruption exerts on economic growth and development (Mauro (1995), IMF (2016)), national governments and international agencies have proposed several anti-corruption strategies. In particular, corporate debarment has been increasingly used as a strategy to target corrupt companies and deter corruption in many countries, such as Brazil, China, France, the U.K., and the U.S. (Zhou et al. (2017), Cerrone et al. (2021)). Despite its popularity, empirical evidence on the consequences of debarment remains scarce.[1]

Debarment is designed to act as a deterrence mechanism by increasing the costs of engaging in corrupt behavior. In principle, debarment results in a severe punishment for corrupt companies by preventing them from obtaining procurement contracts from the public sector for a period of time. However, perhaps surprisingly, the impacts on workers have generally been overlooked.[2] Focusing on debarment, this paper sheds light on how corruption crackdown can have impacts on companies and workers.

I empirically study the labor market consequences of corporate debarment. Brazil provides an ideal setting for studying it for at least three reasons. First, the country is known for the massive burden of government regulation, opening room for corrupt practices by firms. In 2018, Brazil was ranked 136 out of 137 countries for burden of government regulation by the World Economic Forum. Second, Brazil has experienced dramatic changes that increased corporate liability for illicit behavior. In 2013, a large anti-corruption investigation uncovered an unprecedented and extensive kickback scheme to obtain government contracts. In June of 2013, thousands of Brazilians protested against corruption and poor public service provision, prompting the largest protests in two decades. Motivated by general anti-corruption sentiments, two months later, the Congress announced several measures to curb corruption. In particular, the Anti-Corruption Law was enacted, coming into effect at the beginning of 2014, and it substantially increased the costs of engaging in illicit practices for companies. Third, the availability of rich data allows me to characterize the impacts of debarment on firms and workers.

This paper takes advantage of important features of the Anti-Corruption Law. Public offices are required to register and update the debarment database, managed by the federal government,

[1] Auriol and Søreide (2017) present a theoretical framework to conclude that debarment curbs corruption under certain conditions. Focusing on the U.S., Cerrone et al. (2021) empirically study how debarment affects procurement auctions by reducing collusion.

[2] Past research has shown the implications of sanctions on firm value using stock market returns (Ahn and Ludema (2020), Draca et al. (2019)) or on political outcomes (Marinov (2005), Allen (2008)). To my knowledge, no work has investigated the impacts of sanctions on labor market outcomes.

the Registry of Ineligible and Suspended Companies (henceforth CEIS). It is a public database that contains sanctions imposed on establishments and individuals convicted of engaging in irregular activities. Irregularities include, for instance, misconduct in bidding procedures, fiscal frauds and frauds in contracts with the public administration. Once the establishment is included in the CEIS data and officially debarred, it is no longer allowed to have contracts with public agencies until the sanctions expire. In some cases, debarred establishments may be required to pay fines. Some administrative sanctions can be disseminated by the mass media, broadening the information shock of debarment.

In the first part of the paper, I estimate the impacts of debarment on establishments. I employ the passage of the Anti-Corruption Law and leverage a unique and novel source of confidential data containing establishments that have been officially debarred due to illicit practices. I link the universe of establishments that are debarred between 2014 and 2016, immediately after the enactment of the new Law, to the Brazilian matched employer-employee data. The debarment data contains several characteristics of sanctions, such as their start and end dates. The labor market data provides complete coverage of all workers and establishments in the formal sector, including establishment size, paid earnings[3], workers' characteristics, dates when workers are hired and fired, and other variables.

To quantify how debarment affects establishments' performance, summarized by selected labor market outcomes, I adopt event study and matched difference-in-differences approaches exploiting variation in the timing of debarment events, defined as the year when establishments are included in the CEIS data, across establishments for identification. Using rich information at the establishment level, I apply a matching algorithm to construct a set of comparable establishments that are not debarred to form the control group. I then compare how the outcome variables evolve for debarred and control establishments around the time of debarment.

I find that, after debarment, excluded establishments experience, on average, a 47.7 percent decline in total number of employees and are 15.1 percentage points more likely to exit the formal sector. Total monthly payroll also decreases. I show that these effects are robust to alternative specifications and variable definitions. I also document that the employment effect is still negative and persistent for a sample of "establishment stayers", which consists of establishments that survive the debarment shock. I discuss three potential mechanisms that may explain the impacts on establishment aggregates. First, I investigate the role of fines, reflecting on labor market outcomes. I argue that not all debarred establishments are required to pay fines. Even for those that get fined, the maximum cap is too small to entirely explain my results. Second, the public nature of

[3]Throughout the paper, I use the terms "earnings" and "wages in the formal sector" interchangeably.

debarment suggests there is scope for reputational damage. Excluded establishments may struggle to retain or attract clients and workers or even secure credit. Third, debarment acts as a negative demand shock since excluded establishments are not allowed to obtain public contracts. Using detailed procurement data, I show that debarment is associated with lower propensity to both bid for and win procurement contracts from the federal government. I also document that the impacts of debarment are larger for establishments more dependent on government relationships.

In the second part of the paper, I use worker-level data to analyze how debarment affects workers' earnings. I implement a matching algorithm based on individual characteristics to create a control group of workers. I then adopt a difference-in-differences design that compares workers from debarred establishments to a matched comparison group of workers before and after debarment. The advantages of using worker-level data are twofold. First, the average earnings at debarred establishments declines in tandem with measures of employment. The aggregate impacts could, for instance, be confounded by compositional changes. Second, debarment may trigger changes in ownership or tax identifiers, making the results difficult to interpret. The worker-level data mitigates these concerns by allowing me to track the same workers over time.

I document that debarment generates significant earning losses in the formal sector even three years after the event. On average, debarment is associated with a 22 percent decline in earnings. I show that debarment has negligible effects on earnings for workers that remain employed during the analysis period, suggesting that the bulk of earning losses are due to unemployment. While most groups of workers based on observable characteristics, such as gender, educational level or occupation, are negatively affected by the policy, I find some evidence that highly skilled and paid and more tenured workers experience relatively larger unemployment and earning losses.

In the last part of the paper, I scrutinize the role of reputation or information shocks in explaining the findings. Information on excluded establishments is easily accessible in a public and online database. In addition, some sanctions can be disclosed by relevant media, potentially magnifying the information shock of revealing illicit practices. To understand whether unemployment and earning losses are partly driven by reputational effects, I examine the labor reallocation of individuals who are separated from debarred establishments prior to the debarment. I track these workers over time to assess their labor market outcomes after debarment takes effect. This exercise allows me to rule out the direct effect of debarment related to performance and isolate the information shock.

I find some evidence that reputational effects contribute to unemployment and earnings losses. In particular, I find that debarment is associated with a 1.4 percentage point decrease in the probability of being employed and a 12.2 percent drop in earnings. These point estimates, however, are small and noisier relative to the benchmark results, suggesting that reputational damage plays only a secondary role in the findings. Instead, the establishment- and worker-level results can be interpreted as direct consequences of stricter penalties imposed by debarment.

These results have several policy implications. This paper suggests that the costs of debarment extend beyond the main target, the corrupt firms. Corruption crackdown also adversely affects workers, especially those who are highly skilled and paid and have higher tenure and even those who were laid off from the excluded firms before debarment, generating significant and negative impacts on labor market outcomes. This could lead to persistent earning losses in the long run (Lachowska et al. (2020)). While this paper does not quantify the benefits of debarment, it sheds light on the costs that corruption crackdown may induce from the labor market perspective, contributing to the debate over the most promising strategies to fight against corruption. My results underscore the need to fully consider the benefits and costs of these strategies.

This work contributes to three strands of literature. First, there is a large literature in Economics studying corruption (Shleifer and Vishny (1993), Shleifer and Vishny (1994), Svensson (2005), Glaeser and Saks (2006), Olken and Pande (2012)), particularly how corruption affects firm behavior (Mauro (1995), Svensson (1999), Kaufmann and Wei (1999), Fisman and Svensson (2007), Olken and Barron (2009), Sequeira and Djankov (2014), Smith (2016), Bai et al. (2017)). I complement this literature by focusing on workers who remain largely understudied.

Second, there are several papers studying approaches to fight against corruption (Svensson (2005)). Anti-corruption strategies include, for instance, private enforcement of public laws through lawsuits (Hay and Shleifer (1998)), improving citizen access to information and their power to monitor public service quality and officials (Reinikka and Svensson (2005), Reinikka and Svensson (2011)), and "hiring integrity" from the private sector (Yang (2005)). Several works have examined the effects of anti-corruption policies. In China, Lin et al. (2016) analyze how anti-corruption reform impacts shareholder valuations. Zeume (2017) assesses the causal effects of bribery on firm value by exploiting an anti-bribery regulation in the U.K. Karpoff et al. (2017) focus on the enforcement of the U.S. Foreign Corrupt Practices Act (FCPA), which prohibits U.S. companies from paying bribes to foreign government officials. I build on this literature by tracing the impact of debarment, another popular anti-corruption instrument, on establishments' and workers' outcomes.[4] More broadly, I provide evidence that corruption crackdown affects the labor force.

In the Brazilian context, other studies have focused on another important anti-corruption policy: local government audits (Ferraz and Finan (2011)). Colonnelli and Prem (2017), Lagaras et al.

[4]There is also another strand of literature focused on empirical evidence that negative reputation costs can be costly to firms (Karpoff et al. (2008), Murphy et al. (2009), Armour et al. (2017), Akey et al. (2021)), though the shocks studied in these papers are not necessarily related to corruption. In addition, these studies mostly focus on market values, not labor market outcomes.

(2017) and Bologna Pavlik and Harger (2018) study how an innovative audit program affects firms and local economies. This program randomly audits local governments to discourage misuse of public funds among public administrators and fostering civil society participation in the control of public resources (Ferraz and Finan (2008)). While the audit reports have information on firms involved in illicit practices with local governments, it does not necessarily imply that these firms are punished. For instance, Colonnelli and Prem (2017) find that corrupt firms listed in audit reports show better performance after the audit: they experience higher employment, sales and investment, supporting the "sand in the wheel" view of corruption.

On the other hand, debarment has a different purpose: consistent with the classical Becker (1968)'s model in which agents weigh the costs and benefits when deciding whether to engage in a criminal activity, debarment acts as a deterrence mechanism by making punishment more severe through exclusion from having contracts with the government or participating in public bidding procedures during a period of time. As a result, debarment is also expected to promote corporate governance and enhance integrity in the relationships between firms and the government.[5] The origin of debarment probably dates back to 1884, when the U.S. Congress required several government contracts to be awarded to the lowest responsible bidder (West et al. (2006)). In 1928, the U.S. Comptroller General admitted the use of debarment as a preventive instrument. Over the next decades, other countries rarely used debarment as a sanction. Starting in the mid-1990s, motivated by the growing international interest in corruption, debarment rapidly gained popularity among national governments and international agencies concerned about suppliers exploiting institutional weaknesses in developing countries (Auriol and Søreide (2017)). Several examples of countries and agencies that adopted debarment policies over the last two decades include the World Bank, Bangladesh, China, Japan, India, Indonesia, Nigeria, Pakistan, and Vietnam. Despite its popularity, there is little evidence on the consequences of debarment. My findings complement the recent literature analyzing the consequences of debarment (Auriol and Søreide (2017), Cerrone et al. (2021)) and, more broadly, the literature on policy instruments designed to reduce corruption.

Third, this paper speaks to a strand of literature related to earnings losses of displaced workers (Jacobson et al. (1993), Sullivan and Von Wachter (2009), Von Wachter et al. (2009), Couch and Placzek (2010), Von Wachter et al. (2011), Lachowska et al. (2020)) exploiting similar empirical methodology. I also provide suggestive evidence that corruption crackdown constitutes an important source of workers' earnings losses through higher unemployment by comparing the estimates to the displacement literature.

[5] Conversations with government officials indicate that audit reports are one of the several sources that the federal government relies on to investigate potentially corrupt firms. Other sources to start an investigation include, for instance, media reports and whistleblowing.

This paper is organized as follows. Section 2 describes the institutional context, including the creation of CEIS and the Anti-Corruption Law. Section 3 outlines the data and the matching algorithm I use to construct the samples of interest. Section 4 delineates the empirical strategies and Section 5 presents the main results. In Section 6, I offer some concluding remarks.

2 Institutional Context

2.1 The Creation of CEIS Database

Corruption has long been a concern in Brazil. In light of this problem, several anti-corruption measures have been implemented as attempts to halt corrupt practices. In 2003, the federal government created the Office of the Comptroller-General (*Controladoria Geral da União* – CGU), an autonomous federal agency responsible for conducting internal control activities and public audits and implementing corrective and disciplinary measures to prevent and combat corruption, among other duties (Morosini and Vaz Ferreira (2014)).

In December of 2008, following efforts to enhance transparency and improve debarment enforcement in public administration, CGU launched a new and public database, CEIS.[6] It was initially conceived to be a public database compiling information from various sources to disclose establishments who have been sanctioned and debarred for being engaged in illicit practices in bidding procedures, fiscal frauds, or frauds in contracts held with public administration.[7] CGU became responsible for consolidating and disclosing the list of individuals and establishments with valid debarment sanctions imposed by public entities after going through a rigorous process of investigation and condemnation.

Debarment sanctions spanned temporary suspension of participation in bidding, impediment of contracting with public administration and declaration of inability to bid or contract with public administration. In practice, these sanctions implied that punished companies were excluded from having contracts with any public entity or participating in public bidding procedures during the sanction period. Although these debarment sanctions already existed before 2008, the creation of CEIS database disclosing information on punished companies helped to centralize and publicly reveal

[6]The database was launched in December of 2008 to celebrate the International Anti-Corruption Day without any regulation in place. CGU only released the first regulation formally establishing CEIS and its functioning in March of 2010 (*Portaria CGU 516*).

[7]These illicit practices are typically characterized as non-compliance with one of the following regulations: Law 8,666 (enacted in June of 1993), which establishes general rules about public bids and contracts; Law 10,520 (enacted in July of 2002), which regulates the e-procurement system; Law 8,429 (enacted in June of 1992), which targets misconduct in public office; Law 9,504 (enacted in September of 1997), which governs the funding of political parties and campaigns; and Law 8,443 (enacted in July of 1992), which establishes penalties for any damage to the public treasury, like frauds in public tenders.

companies engaged in illicit practices. For simplicity, throughout the paper, I refer to ***corporate debarment or blacklisting*** as an event in which companies are included in the CEIS database.

Until the Anti-Corruption Law was enacted, public entities were not required to keep the database updated. The new legislation was also a watershed in terms of both legal compliance and corporate liability for corrupt practices.

2.2 The Anti-Corruption Law

In the first half of 2013, an investigation revealed a large kickback scheme involving private firms and government contracts. Various members of the business and political elite were investigated and convicted for corrupt practices. In June of 2013, Brazil experienced an unprecedented wave of protests against corruption and bad provision of public services (Aguilar and Ferraz (2014)).[8] In response to these complaints, the federal government enacted the Anti-Corruption Law (Law 12,846/2013) in August of 2013, imposing new measures to combat corruption, inspired by the international trend of anti-corruption efforts.[9,10] The new Law came into effect in January of 2014.

Important to the paper, the Anti-Corruption Law mandated that all public entities from all government branches (executive, judicial, and legislative) and levels (federal, state and local governments) must register and update all debarment sanctions in the CEIS database. Indeed, before 2014, there were very few records from several state governments, such as Distrito Federal, Rio de Janeiro, and Rio Grande do Sul. These states started to actively update the CEIS database with appropriate information only after the Anti-Corruption Law.[11] The range of possible penalties also expanded. Some firms, for example, were required to pay fines. In practice, exclusion from government contracts still constitutes the main punishment. These penalties can be applied through judicial or administrative decisions. In the case of judicial decisions, the judge is expected to send an official letter to CGU requiring both the firm and the sanction to be registered in the CEIS database once the decision becomes final and unappealable.

The administrative process, on the other hand, is conducted in a different way. The first step

[8]Other sources of dissatisfaction included, for instance, police brutality, excess spending on the 2014 World Cup and poor public service delivery.

[9]For instance, the Anti-Corruption Law holds individuals giving out bribes equally responsible as public officials receiving them, recognizing corporate liability for bribery. Companies may be punished, rather than individuals, implying that firing an employee is not enough to rid the company of responsibility. Anecdotal evidence suggests that firms are investing more in compliance.

[10]International examples include stricter enforcement of the Foreign Corrupt Practices Acts (FCPA) in the U.S., as well as countries like China and the United Kingdom enacting new anti-corruption measures or requiring a more stringent enforcement of existing regulations.

[11]The CEIS database contains the start date of debarment, along with states and government branches and levels, allowing me to notice different patterns before and after 2014. For instance, considering the universe of establishments punished by state governments, Rio de Janeiro accounts for 1.8 (7.73) percent of the cases between 2008 and 2013 (2014 and 2018). I also observe similar changing patterns after the Anti-Corruption Law for other states.

generally consists of media reports, whistleblowing, audit reports or other sources indicating possible irregularities by a firm or an individual. If there is enough evidence of wrongdoing, there is a thorough investigation and the accused has the right to defend himself against the charge. The main penalty, if applicable, takes force by the end of the short administrative process, which cannot last more than six months. In few cases, firms may also be required to pay a fine up to 20 percent of their gross revenues or up to 60 million *reais* (about USD 15 million) when calculating revenue is not possible, and the sanctions must be disseminated by the mass media, such as government gazettes or relevant newspapers based on circulation. Convicted firms and individuals are then registered in the CEIS database. After serving the sentence, debarment is revoked and the firm may participate in bidding procedures again.

2.3 Information Disclosure

The public nature of debarment is an important feature of this paper. Government gazettes and relevant newspapers are examples of media sources through which administrative sanctions are disclosed.[12] Although there is no direct evidence demonstrating that individuals learn about debarment events as the CEIS dataset does not have information on media sources utilized to reveal the sanctions, anecdotal evidence indicates that the information on banned firms is widespread. I include some examples illustrating how this information diffused to the public. For instance, an article from the online portal *O Globo*, part of the biggest media conglomerate in Latin America, states that: "The Company X was registered by CGU in the CEIS database. The company — suspect of participating in the subway and train cartel in São Paulo and Brasília — has been barred from contracting with the federal government due to alleged bribery payments in *Correios*[13] procurements." (*O Globo*, 03/07/2014).

Other prominent examples come from *Folha de São Paulo*, the second most popular newspaper: "The number of ineligible and suspended companies and individuals by CGU increased by 27.3 percent in 2016. The main penalty consists of being prevented from signing contracts with public administration (. . .). This happens when frauds are detected from their relationships with federal, state or municipal governments or state-owned companies." (*Folha de São Paulo*, 12/22/2016). In March of 2016, another article announced that "CGU published the decision to ban another contractor (. . .) from contracting with public administration (. . .). It was proven that Y coordinated

[12] Despite the general requirement that administrative sanctions should be disseminated by the mass media, the legislation provides little guidance on where and how to disclose such sanctions. In addition, the CEIS platform does not require public entities to inform whether and which media was used to reveal the sanctions levied on punished companies. Official government gazettes (e.g. *Diários Oficiais*) and relevant newspapers are some examples of media sources for announcing the sanctions.

[13] *Correios* is a state-owned Brazilian company that operates the national postal service.

actions with other competitors to reduce competitiveness in several bidding processes by combining prices and winners (...). It was demonstrated that the company paid bribes to public agents amounting to 3 million *reais* (equivalent to USD 0.75 million) to sign a contract with *Petrobras*[14] (...)." (*Folha de São Paulo*, 06/09/2016).

These articles from relevant media outlets suggest that information on excluded firms may have reached the public. In addition, this paper focuses on debarment events from 2014 to 2016, a period coinciding with Operation Car Wash, launched in March of 2014 and still ongoing, which uncovered a vast corruption scheme involving corporations and politicians and resulted in unprecedented thousands of arrest warrants against former presidents of the Republic, Congress and Senate members, state governors, and businessmen. Several debarred companies were convicted of corruption charges triggered by this investigation, gaining massive media coverage in Brazil. Google web searches about the corruption scandal indicate that citizen interest in the topic has substantially increased. According to CGU, the number of online accesses to the CEIS platform jumped from 82,837 in January of 2011 to 459,161 in December of 2017, reaching a record high of nearly 600,000 visits in August of 2017.

3 Data

This paper uses several data sources. First, I use confidential debarment data provided by CGU. Second, I utilize matched employer-employee records covering the formal sector, the *Relação Anual de Informações Sociais* (RAIS). Third, I gather information on online bids and winners of federal procurement contracts.

3.1 The Debarment Data

The confidential CEIS dataset (Controladoria Geral da União (2019)) includes all establishments that have been debarred since its inception. It contains detailed information on names and tax identifiers, types of sanction, start and end dates of sanction, and government agencies responsible for applying the penalties.

I make some restrictions to the CEIS dataset. Because debarment sanctions target establishments, not necessarily firms, I begin by restricting the analysis to establishments, as the original data also include punished individuals. To avoid duplicate observations, I maintain the earliest sanction each establishment has. Next, I remove establishments that had cancelled sanctions. Figure 1 shows the evolution of the number of debarred establishments. There is a clear increasing

[14] *Petrobras* is a semi-public Brazilian multinational corporation in the petroleum industry.

trend starting in 2008, when CEIS is created, and reinforced by the Anti-Corruption Law in 2014.

I then limit the sample to establishments whose initial years of debarment are between 2014 and 2016 for three main reasons. First, measures implemented by the Anti-Corruption Law began to take effect in 2014. Prior to this year, public entities were not required to list excluded establishments in the CEIS data, raising concerns about selection bias. For instance, state governments from Distrito Federal, Rio de Janeiro and Rio Grande do Sul only started to update the CEIS database with information on excluded establishments in 2014. Table A1, Appendix A, displays summary statistics using information from CEIS data and highlights several patters. In addition to some differences in the geographic composition of debarment cases before and after 2014 (Columns (3) and (5)), we notice that the average length of punishment prior to 2014 is 29.13 months, considerably higher than the average of 19.93 months after 2014, indicating selection of more severe sanctions getting reported to the CEIS platform before the Anti-Corruption Law. Second, the institutional context discussed in Section 2.3 suggests a higher public awareness of corruption after 2014.[15] Third, data on online bids and winners of federal procurement contracts, essential to probe the mechanisms behind the main results, are only available since 2013. Therefore, to facilitate the interpretation of the results, I restrict the sample to establishments that have been debarred since 2014. In Section 5.4, I revisit this sample restriction by expanding the analysis to include establishments that have been debarred before 2014.

Because my goal is to track these establishments in labor market data at least two years later after initial debarment and these data are only available until 2018, I drop establishments that have been debarred since 2017. These restrictions leave me with a sample of 6,862 establishments[16] to be matched with employer-employee data through tax identifiers.

3.2 Labor Market Data

I extract labor market information from RAIS (Brazilian Ministry of Labor and Employment (2019)), the Brazilian matched employer-employee data that the Ministry of Economy collects each year. It provides a comprehensive and high-quality overview of the entire formal sector (Dix-Carneiro (2014)). To incorporate three years before debarment into the main analysis, I use annual

[15]In a simple Becker (1968)'s framework applied to corruption, the Anti-Corruption Law could deter more establishments from engaging in corrupt practices through more severe or certain punishments or through higher opportunity cost of corruption. By requiring debarred establishments to be included in the CEIS database, public agencies no longer have discretion to choose who gets reported, increasing the likelihood of effective punishment. A higher public awareness of corruption since 2014 may raise the opportunity cost of corrupt practices (e.g. through relatively lower profit streams). Therefore, the degree that debarment can affect establishments may differ during the years before and after the Anti-Corruption Law.

[16]The same sample has 6,823 firms. The data allows me to distinguish between establishments and firms. However, I restrict my analysis to establishments since most firms have only one establishment. In Section 5.4, I show that the results are quite robust to the analysis at the firm level.

RAIS data for the period between 2011 and 2018, the last year in which the data are available.[17] In all years, each entry consists of detailed worker-level information on demographic characteristics (e.g., gender, age and educational level), average earnings in the year, number of hours worked, occupation, admission date, along with other variables. Establishment-level information include, for instance, industry, establishment size (in terms of number of employees), and municipality and state where the establishment is located.

I have gained access to worker- and establishment-level tax identifiers, which are unique and do not change over time. Identified data has two advantages. First, because the CEIS data also contain establishment identifiers, I can match debarred establishments to RAIS and recover labor market information. Second, I can track workers over time and across establishments.

Although the RAIS data are incredibly detailed in some regards, there are important caveats to mention. RAIS includes only formal workers, lacking information on employees who are out of the labor force, informally employed, unemployed, or self-employed (Dix-Carneiro and Kovak (2017)). This is an important limitation in a country where about 40 percent of all employed workers were in the informal sector in 2017. It is not possible, for instance, to know whether a missing record is due to unemployment/non-participation or the worker being in the informal sector.

I match the final list of 6,862 debarred establishments to RAIS data from any year between 2011 and 2018. I then drop observations without any employee in all three years before initial debarment, leaving a sample of 3,294 establishments to implement the matching algorithm.[18]

3.3 Minor Sources

While the labor market data are the primary source of data, I rely on two minor sources to complete the main analysis. First, for the matching algorithm, I obtain municipal population estimates from the 2010 Demographic Census, carried out by IBGE (Instituto Brasileiro de Geografia e Estatística (2010)). Second, I use information on online bids and winners of federal procurement contracts from *Portal de Transparência* and *API de Compras Governamentais* websites, regularly updated by the federal government, for the years between 2013 and 2018 (API de Compras Governamentais (2019), Portal da Transparência (2019a), Portal da Transparência (2019b)).

[17]Section 5.4 uses RAIS data for the period between 2002 and 2018.

[18]Columns (1) to (10) of Table A1, Appendix A, display summary statistics for establishments facing debarment using information from CEIS. I use four distinct samples: the original sample of debarred establishments (Columns (1) and (2)), after restricting to establishments that have been debarred between 2008 and 2013 (Columns (3) and (4)), after restricting to establishments that have been debarred between 2014 and 2016 (Columns (5) and (6)), after matching post-2014 establishments with RAIS (Columns (7) and (8)), and after implementing the matching algorithm described in Section 3.4 (Columns (9) and (10)).

3.4 The Matching Procedure

I construct two samples for the main analysis, which I refer as the establishment and worker samples.

Establishment Sample. In order to create a suitable control group for debarred establishments, allowing me to estimate counterfactual trends, I implement a matching procedure using the labor market, municipal population and procurement data. Columns 1 and 2 of Table 1, together with Table A2, Appendix A, indicate sizable differences between debarred and non-debarred establishments. On average, debarred establishments are larger in terms of employment and monthly payrolls[19], are concentrated in specific sectors, such as construction, are more likely to bid for procurement contracts with the federal government, and are located in more populated municipalities.

The first control group uses each of the three years before official debarment — $[t-3, t-1]$ — and matches each debarred establishment to a counterfactual establishment from the same two-digit industry[20] and state with the closest propensity score, in which is estimated by predicting treatment using a linear probability model with the following set of variables: annual distributions of total employment and average earnings for the three years before debarment, whether the establishment has bid for and won procurement contracts with the federal government in the three years before debarment, whether it is a single establishment, and size of municipality where the establishment is located.[21] I compute deciles of the employment and average earnings distributions considering the universe of all private establishments in the RAIS data in each of the years.[22] In Appendix B, I describe the data and the matching procedure in detail. In Section 5.4, I show that the main results are robust to alternative ways of constructing the matching algorithm. In particular, I document that the the main conclusions are similar to matching exclusively on the year before official debarment rather than the pre-event trends, to relying on coarsened exact matching (Iacus et al. (2012)) instead of a propensity score matching, and to allowing multiple control units for each treated establishment.[23]

[19] Earnings are adjusted to 2018 Brazilian *reais* using inflation index from Central Bank of Brazil (2021).

[20] Appendix B includes a list of 17 industries generated for the analysis.

[21] I aggregate population information into seven groups: less than 5,000 inhabitants; equal or more than 5,000 and less than 10,000; equal or more than 10,000 and less than 20,000; equal or more than 20,000 and less than 50,000; equal or more than 50,000 and less than 100,000; equal or more than 100,000 and less than 500,000; and more than 500,000. Numbers are drawn from the Demographic Census in 2010, the most recent year. While information on annual population are available, they mostly consist of estimates and are more sensitive to measurement errors. The Census is the most reliable source of population records.

[22] I exclude public administration establishments from the sample because they are subject to a different legal framework, and their workers are employed under different contracts and are entitled to special benefits (Arnold (2018)).

[23] Because multiple candidates may arise for each treated unit, I force the main algorithm to generate one candidate for each treated establishment. It avoids weighting issues when splitting the sample to estimate heterogeneous effects. I also ensure that potential control establishments are not associated to more than one debarred establishment. In

The matching strategy is similar to other papers using a matched difference-in-differences design (Jäger and Heining (2019), Arnold (2020), He and Le Maire (2020)) and is essential to the first part of the analysis because it recovers a counterfactual group of establishments that have similar pre-event trends but do not face debarment. With the control group in hand, I can document how debarment affects selected outcomes. Starting from a sample of 3,294 debarred establishments, the above matching routine returns 3,179 establishments as the control group to be used in the main analysis.

Worker Sample. I take a few steps to define the sample of workers. First, I recover all individuals that worked in a establishment for the three years ($[t-3, t-1]$) before it was debarred. I apply a tenure restriction to extract workers that are more attached to companies since Brazil has substantial turnover in the labor market. I also find similar results when alternatively considering individuals that have worked in a establishment in the year prior to debarment. The tenure restriction, however, offers an additional advantage of following the displacement literature (Jacobson et al. (1993), Couch and Placzek (2010), Lachowska et al. (2020)) and allowing to benchmark my results to this literature. Second, to keep the analysis comparable, the set of debarred establishments to which workers are associated is the same as in the establishment-level analysis. Third, to get candidates to form a comparison group of workers, I generate a sample of workers that have worked at least three years in establishments that have never been debarred.

For each worker in treated establishments, I select the comparison worker with the closest propensity score, which is calculated by predicting treatment using a linear probability model with the following set of worker characteristics in $t-1$: age bins (5 years age bins), indicator for male, indicator for disability, indicator for white, educational level, two-digit industry, and occupation categories.[24] I do not explicitly match workers based on earnings since this is one of the outcomes of interest. Nonetheless, an alternative algorithm that also includes deciles of earnings distributions yields similar results. These restrictions altogether yield a sample with 81,333 workers in the treated group.

Section 5.4, I relax this restriction by allowing up to three and five control units for each treated establishment and find that the results are not sensitive to choosing one or multiple counterfactual units.

[24] Educational categories are grouped into eleven categories: illiterate, incomplete primary education, primary education, incomplete lower secondary education, lower secondary education, incomplete upper secondary education, upper secondary education, incomplete tertiary education, tertiary education, Master degree, and PhD degree. For occupation, I use the 2002 Brazilian Classification of Occupations (CBO), which classifies jobs based on their skill and task content to construct four occupation categories: managerial, professional, blue collar, and white collar lower level positions.

3.5 Summary Statistics

Establishment Sample. Table 1 presents summary statistics for both groups — debarred establishments and the comparison group — using labor market data in all three years prior to debarment. Punished establishments have, on average, 164 employees, while the control group has an average of 114 workers. The average earnings in debarred establishments are similar (1,823 Brazilian *reais*, equivalent to USD 347) to the comparison group (1,958 Brazilian *reais* or USD 373). The two samples are also similar in terms of having bid for a procurement contract from the federal government, location and industry. We notice that the matching algorithm does not fully eliminate differences between debarred and non-debarred establishments. My identification strategy, however, does not require balance on covariates. It relies on parallel trends between both groups of establishments before debarment.

Worker Sample. Table 2 displays summary statistics for workers with three years tenure in the year immediately prior to debarment for both treated and control groups. Table 2 confirms that both groups of workers are comparable along observable dimensions, such as gender, disability, firm size, education, age, occupation, and tenure, reinforcing that the matching algorithm does a good job finding a balanced control group. The main difference comes from earnings. On average, in the year before debarment, workers in punished establishments earn about 2,256 Brazilian *reais* (equivalent to USD 430) per month, which is less than control workers' earnings (2,572 Brazilian *reais* or USD 490). In addition, nearly 62 (63) percent of treated (comparison) workers are male, 83 (81) percent are employed in large establishments with at least 100 employees, 10 (11) percent have a college degree, and 3 percent hold a managerial position.

4 Empirical Strategy

4.1 Establishment-Level Analysis

In the first part of the empirical analysis, the goal is to estimate the reduced form effects of debarment on establishments. I adopt an event study approach based on when establishments are registered in the CEIS database. For each punished establishment in the analysis sample, I define the year before debarment as $t = -1$, and all remaining years are indexed relative to that year.

To assess the impact of debarment, I estimate the following matched difference-in-difference model using the establishment-level sample:

$$y_{jft} = \sum_{k=-3}^{k=2} [\beta_k \times \mathbf{1}(t_j = t^* + k) \times Debarment_{jf} + \theta_k \times \mathbf{1}(t_j = t^* + k)] + \alpha_j + \alpha_t + \varepsilon_{jft}, \qquad (1)$$

in which subscripts j, f, and t stand for establishment, firm, and year; $\mathbf{1}(t_j = t^* + k)$ are dummies indicating a debarment event in year k relative to the debarment year t^*; $Debarment_{jf}$ is an indicator variable for debarred establishments; α_j are establishment fixed effects; α_t are year fixed effects; and y_{jft} is the outcome of interest. Year fixed effects control for common shocks affecting the establishments each year. Establishment fixed effects control for time-invariant characteristics of establishments that might be correlated with the outcomes of interest and their inclusion in the CEIS database. Standard errors are clustered at the firm level.[25]

4.2 Worker-Level Analysis

To further understand how debarment affects workers' earnings, I estimate the following specification using the worker-level data:

$$y_{it} = \sum_{k=-3}^{k=2} [\beta_k \times \mathbf{1}(t_i = t^* + k) \times Debarment_i + \theta_k \times \mathbf{1}(t_i = t^* + k)] + \alpha_i + \alpha_t + \varepsilon_{it}, \qquad (2)$$

in which subscripts i and t stand for worker and year; $\mathbf{1}(t_j = t^* + k)$ are dummies indicating a debarment event in year k relative to the debarment year t^*; $Debarment_i$ is an indicator variable for individuals that have worked in debarred establishments in the past three years before the event; α_i are worker fixed effects; α_t are year fixed effects; and y_{it} is the labor market outcome of interest. Standard errors are two-way clustered both at the worker and pre-event firm levels.

From Equations (1) and (2), the post-event coefficients of interest — β_k — capture the dynamics effects of debarment relative to the year before the event. Identification in Equation (1) (Equation (2)) relies on the timing of debarment being uncorrelated with the outcomes of interest, *conditional* on establishment (worker) and time fixed effects. The key identifying assumption is that establishments' (workers') outcomes for treated and control establishments (workers) would have followed parallel trends in $k > 0$ if no debarment had occurred for treated establishments (workers). I test this assumption by assessing whether the pre-event coefficients of interest are statistically indistinguishable from zero.

[25] Since the data have both establishment and firm identifiers, I also implement a firm-level analysis. In Section 5.4, I show that establishment- and firm-level results generate similar estimates.

One possible concern with the identifying assumption is that the debarment treatment may be correlated with other unobservable and concurrent shocks, confounding the estimated effects. For instance, debarment may result in a loss in political connections during election years, and thereby affecting the outcomes of interest. The context implies that establishments are debarred at different years in calendar time. Thus, it is unlikely that these shocks only affect debarred establishments and their timing exactly coincide with the timing of debarment, mitigating concerns related to unobservable shocks affecting the coefficients of interest.

5 Results

5.1 Impacts on Establishments

Figure 2 displays $\hat{\beta}_k$, along with 95 percent confidence intervals, after estimating Equation (1) for selected labor market variables. The treatment and control groups present similar trends in the years before debarment. Indeed, for all outcomes, the pre-event coefficients are statistically equal to zero, lending support to the parallel trends assumption. The lack of pre-trends indicates that the matching procedure is effective, since it is designed to mitigate pre-trend differences.

Figure 4(a) shows the point estimates for log employment.[26] Immediately after debarment, there is a sharp and significant decline in log employment, a pattern that is even stronger in the subsequent years. In the year of debarment, log employment falls by -0.378 (SE = 0.030). The coefficient grows in absolute value to -0.710 (SE = 0.039) and -0.856 (SE = 0.044) in the next two years. As shown in Column 1 of Table 3, the point estimate summarizing the average impact in the first three years after debarment is -0.649 (SE = 0.035), equivalent to a 47.7 percent $((100 * [\exp(-0.649) - 1]))$ decline in employment.[27]

Figure 4(b) presents the results when the outcome variable is an indicator for exiting the formal sector, which is equal to one if the establishment does not have any formal employee in the given year. The average impact including the two years after debarment shown in Column 2 of Table 3 indicates that punished establishments are 15.1 percentage points (p.p.) more likely to exit the formal sector following debarment.

[26] To deal with zeroes in the data, I add one to the outcome variables so that log is defined for all establishment-year pairs. I also probe the robustness of my results by using the variables in levels and by applying inverse hyperbolic sine transformations to the variables.

[27] To obtain the aggregate estimates, I run the following specification:

$$y_{jft} = \alpha_j + \alpha_t + \beta \times PostDebarment_{jf} + \varepsilon_{jft} \tag{3}$$

in which the subscripts and the set of fixed effects are the same as in Equation (1), and $PostDebarment_{jf}$ is an indicator variable equal to 1 for all years after debarment in debarred establishments. As before, standard errors are clustered at the firm level.

I also document the event-study estimates for unconditional log earnings in the formal sector. The term "unconditional" means that I use the full sample, regardless of whether the firm exits the formal sector after debarment. Figure 4(c) shows that, in the debarment year, average log earnings fall by -0.582 (SE = 0.058) in punished establishments relative to the control group. The coefficient summarizing the decline reaches -1.582 (SE = 0.107) two years later. The average effect on log earnings is -1.147 (SE = 0.078), a 68.2 percent decrease. These findings are unsurprisingly mechanical as debarment is associated with higher probability of exiting the formal sector. Figure 4(d) and Column (4) of Table 3 report the findings for log monthly payroll, also suggesting substantial decline.[28]

I next consider an alternative sample with establishment stayers that have at least one employee in all periods in the window of [-3, 2] years around the debarment event and replicate the above exercises. Figure C1 and Table C1, both in Appendix C, depict the point estimates. Following debarment, the average log employment, earnings, and monthly payroll decreases by -0.180 (SE = 0.024; about 16.5 percent), -0.066 (SE = 0.016; around 5.92 percent), and -0.264 (SE = 0.034; nearly 23.2 percent), respectively. The impacts are more modest, albeit still large and statistically significant. I interpret these findings as evidence that the overall negative effects still exist even after excluding establishments that exit the formal sector or attempt to escape from sanctions by creating new tax identifiers.

In Table C2, Appendix C, I implement additional checks to confirm that the results are robust to alternative variable definitions. In Columns (1) and (2), I show that the main conclusions regarding employment effects remain roughly the same when considering total number of employees as the outcome variable and applying its inverse hyperbolic sine transformation. Columns (3) through (6) repeat this exercise by including absolute earnings per worker and monthly payroll as the dependent variables. Columns (1) through (4) of Table C3, Appendix C, include municipality-specific trends to control for any policy or unobservable shocks specific to municipalities. The point estimates are similar. Columns (5) through (8) alternatively add 2-digit industry-specific trends, and the estimated coefficients do not change. I also document that the aggregate impacts mask substantial heterogeneity across sectors and sizes (in total employment). Tables C4 and C5, Appendix C, show that the effects are predominantly concentrated in larger establishments and in some sectors, such as real estate and construction, though establishments from all sectors and sizes experience significant impacts.

I next discuss three explanations behind the main findings. Disentangling these mechanisms has

[28] The analysis is restricted to the formal sector, which is a limitation considering the context with a large informal sector. Laid-off individuals are not necessarily unemployed or out of the labor force as some of them may be employed in the informal sector. Unconditional log earnings impute zero earnings to individuals outside the formal sector.

relevant policy implications and offers insights on the relationship between corruption crackdown and firm performance. First, debarred companies may be financially constrained by the fines imposed on them, reflected in the reduction of tangible costs, such as price and quantity of labor inputs. The data unfortunately has very few observations with information on fines to bring statistical power to investigate this channel.[29] In addition, the maximum cap for fines is set at 20 percent of annual gross revenues. This ceiling may be too low to entirely explain the drop in labor market outcomes. Second, the public nature of debarment may generate an information shock for punished companies. For instance, they may struggle to retain or attract clients and workers or even secure credit after being debarred. I partially test for this mechanism in Section 5.3 by focusing on workers, accounting for the limitation that I do not have data on sales performance or loans to observe other margins of response.

Third, government agencies are relevant buyers. In my sample, 62 percent and 51 percent of establishments bid for or won procurement contracts with the federal government between 2013 and 2018, respectively. This high proportion suggests that debarment has the potential to induce a negative demand shock for punished companies. I empirically test for this channel in two ways using the procurement data.[30] First, I estimate Equation (1) using indicator variables for whether the establishment bids for and wins a procurement contract with the federal government. Figure C2 and Table C6, Appendix C, point to a sizable decline by 4.7 p.p. and 6.1 p.p. in the likelihood of bidding for and winning contracts following debarment.[31] Second, I test whether the impacts on establishments are stronger among industries with more intense connections with the government. Considering the universe of federal contracts awarded in 2013, the first year of the data, I compute the distribution of total value of contracts by 2-digit industry and define industries above the median as those with stronger connections with the government. I also construct an alternative measure for government dependence for each establishment calculated as the ratio of total revenues obtained from federal government contracts and the annual payroll in 2013. I define establishments strongly dependent on the government as those having at least 25 percent of payroll expenses covered by revenues from federal government.[32] Table C7, Appendix C, indicates that the estimates are slightly larger among establishments more connected with the federal government. Both Tables C6 and C7 provide support for the role of demand shocks in explaining the previous results.

The employment effects shown here are substantially larger than the ones documented by the

[29] Only 0.42 percent of observations from the CEIS data before sample restrictions have information on fines.

[30] The data only covers a subset of government purchases, missing information on purchases from other government agencies, such as state and municipal governments. To my knowledge, there is no centralized data on purchases from all government agencies in Brazil.

[31] Interestingly, for establishment stayers, Columns (2) and (4) of Table C6, Appendix C, indicate smaller effects, suggesting that they are less affected by this shock.

[32] The results are not sensitive to this threshold definition.

literature examining the role of demand shocks in firm growth (Ferraz et al. (2015), Pozzi and Schivardi (2016), Atkin et al. (2017), Cho (2018), Gugler et al. (2020)). Debarment constitutes a negative demand shock that is qualitatively different from other positive demand shocks typically documented in the literature in several ways.[33] In addition to the scope for reputation effect in a debarment shock, the procurement market usually has a sizable learning component as firms that win government contracts are likely to participate in future bidding processes with higher values and sell a broader set of products in other markets (Ferraz et al. (2015)), leading to an increasing dependence on government contracts over time. Therefore, the exclusion from government contracts and public bidding procedures is expected to generate even larger impacts on firm outcomes in comparison to winning government contracts. Another difference is that corruption may act as a tax on companies to conduct business (Shleifer and Vishny (1993), Olken (2007), Colonnelli and Prem (2017)), and corrupt establishments are likely to have extracted corruption rents prior to debarment to survive and grow. As a result, the exclusion from government contracts forces them to return to levels reflecting their quality in the absence of corruption taxes. Although this can be interpreted as a negative demand shock, it is qualitatively different from other typical demand shocks examined in the literature.

5.2 Impacts on Workers

The impact of debarment on workers' earnings is of interest. It is difficult to interpret establishment-level results because companies may alter the composition of their employees in response to shocks. In this case, impacts on earnings may simply reflect compositional changes rather than changes in earnings for similar workers. To circumvent this difficulty, I examine the impacts of debarment on earnings using worker-level data. Instead of using establishments as the unit of analysis, I estimate Equation (2), which directly compares workers attached to debarred establishments to a matched control group before and after debarment.

Figures 3(a) – 3(c) present results for the dynamics of workers' employment and earnings around debarment shocks. The pre-event coefficients are statistically equal to zero for all outcomes, validating the event-study strategy. Figure 3(a) depicts the employment estimates, indicating that, in comparison to the control group, workers from debarred establishments experience a sharp decrease in the probability of being employed in the formal sector after debarment, regardless of whether they still work in punished establishments. The average post-event coefficient in Column (1) of

[33] For instance, Cho (2018) shows that firms who were awarded government contracts, grants, and loans through the American Recovery and Reinvestment Act of 2009 experience a 3.5 increase in total employment. In the context of procurement contracts, Gugler et al. (2020) document that winning a government contract in Austria leads to a 3 percent increase in the workforce. Ferraz et al. (2015) find that winning at least one contract immediately increases firm growth by 2.2 percentage points in Brazil.

Table 4 suggests a decline of 3.4 p.p. (SE = 0.014).

Figure 3(b) shows the event-study estimates for log earnings in the formal sector using the full sample. Column (2) of Table 4 documents that workers from debarred establishments experience a a relative decline in log earnings by -0.247 (SE = 0.096), equivalent to a 21.9 percent decrease in earnings relative to counterfactual earnings after debarment. This finding is unsurprising as workers are also more likely to not be employed in the formal sector and, therefore, are assigned zero earnings. Conditioning the sample to observations with positive earnings, Column (3) indicates that the point estimate is no longer statistically significant, reinforcing the role of unemployment in the earnings results.[34]

To better understand the magnitude of the earning losses of 22 percent, I compare it to the worker displacement literature. For example, Jacobson et al. (1993) document earnings losses of about 40 percent during the first year of displacement using a sample of displaced workers in Pennsylvania. Couch and Placzek (2010) and Lachowska et al. (2020) find lost earnings of 33 and 45 percent at the time of displacement in Connecticut and Washington State. During the recession in the U.S., Davis and Von Wachter (2011) estimate a 39 percent decline in earnings at the year of displacement. In the Brazilian context, Bhalotra et al. (2021) document a 42 percent decline in labor income up to three years following displacement for workers displaced in mass layoffs between 2012 and 2014. In this paper, I show that the earning losses of 22 percent from debarment are computed from a sample of workers regardless of being displaced from punished establishments and that debarment leads to 47.7 percent decline in employment. Therefore, the earning losses are strikingly comparable to the displacement literature, supporting the interpretation that unemployment plays a major role.

Turning to heterogeneous impacts based on workers' characteristics to understand potential distributional implications, I investigate whether some groups of workers are disproportionately affected by this anti-corruption instrument.[35] Table C9, Appendix C, reports the estimates across different characteristics, including gender, race, skill (measured by level of education), age groups, occupation, tenure, and wage distribution. While all groups of workers are severely hit by debarment, I find some evidence that managers (Panel F), workers with longer tenure (Panel G) and highly paid employees (Panel H) display higher unemployment rates and earning losses, bearing

[34] In Table C8, Appendix C, I perform several robustness checks, confirming that the results are robust to alternative definitions of the outcome variables and additional controls.

[35] To investigate heterogeneous effects based on individuals characteristics extracted from the year before debarment, I estimate the following model:

$$\begin{aligned} y_{it} = \beta_1 \times Post_t + \beta_2 \times Post_t \times Debarment_i + \beta_3 \times Post_t \times Heterogeneity_i + \\ \beta_4 \times Post_t \times Debarment_i \times Heterogeneity_i + \alpha_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{4}$$

in which the set of fixed effects and outcomes is the same as in Equation (2). β_4 is the interaction term of interest and is reported in Table C9, Appendix C.

higher costs of corruption crackdown

5.3 Isolating the Information Shock Channel

I have shown that debarment is associated with negative effects on employment and unconditional earnings. As discussed in Section 5.1, there are at least two reasons that may explain these findings. First, debarment is designed as an anti-corruption policy to punish firms for corrupt practices by excluding them from government procurement processes. This sanction may generate a negative demand shock, especially for firms heavily dependent on public contracts. I refer to this channel as the *direct effect* of debarment. The second explanation comes from the institutional context. Information on excluded companies is listed in a public database and sanctions can be disseminated by the mass media, suggesting a role for *information shock*.

In order to investigate whether the negative impacts of debarment are partly driven by the information shock channel, I scrutinize the labor reallocation of workers separated from employment. In particular, I test for whether there are reputation effects from having worked at least three years in the recent past in a punished company. I create a sample of workers that are laid off *prior* to debarment events from the same set of establishments used in the establishment-level analysis. I track them over the subsequent years, even they are working in a different company, to assess whether there are differences in labor market outcomes after their original firm is debarred. Any difference could be interpreted as the information shock brought by debarment. This strategy allows me to rule out the direct effect and isolate the information shock given the limitation that I do not have data on revenues or loans to observe other margins of response.

I estimate Equation (2) using a sample of workers from both treated and control establishments that have been laid off in any of the three years before debarment. Figures 4(a) and 4(b) plot the point estimates for employment and log unconditional earnings in the formal sector. Both outcomes experience similar trends prior to the event, validating the research design. After debarment, there is a negative, albeit small and somewhat noisy, impact. Columns (1) and (3) of Table 5 point to a decline by 1.4 (SE = 0.010) p.p. and 0.131 (or 12.2 percent decrease) (SE = 0.072) in employment and log earnings, respectively. When controlling for time-varying worker's characteristics, such as worker's age and age squared, the effect becomes smaller, although still negative.

In Table C10, Appendix C, I consider separate samples of separated workers depending on whether they have been laid off one, two or three years prior to debarment to examine whether the effect of the information shock is stronger for individuals laid off closer to debarment dates. I do not find evidence of a systematic pattern. Taken together, the findings suggest that debarment also affects workers who were connected to punished companies before the policy shock.

5.4 Robustness Checks

Appendix D provides robustness tests for the main analysis. I begin with checks related to the construction of the sample, followed by alternative matching procedures. I also discuss the intensity of treatment and the effects on firms rather than establishments.

Pre-2014 Debarment Analysis. For the reasons outlined in Section 3.1, the main analysis is restricted to establishments that have been debarred after the enactment of the Anti-Corruption Law. On the other hand, there is also a set of establishments excluded from public procurement between 2008 and 2013 in the CEIS data, for which I assess the labor market consequences. Tables D1 and D2, Appendix D, report the establishment- and worker-level results. Overall, the estimates are in line with the post-2014 analysis, consistent with debarment inducing large and negative impacts on workers.

Alternative Sample Restriction. As described in Section 3.2, in order to obtain the final sample to implement the matching algorithm, I drop establishments without any employee in all three years prior to initial debarment. As a robustness check, I relax this restriction by allowing establishments to not have any employee in any of the three years prior to debarment.[36] Table D3, Appendix D, shows that the impacts on establishments are quite similar to the baseline estimates.

Alternative Matching Algorithms. I also explore the sensitivity of the main results to a number of different matching specifications. First, instead of matching on each of the three years before official debarment ($[t-3, t-1]$), I match only on the level (i.e., the year prior to debarment, $t-1$). It allows me to assess pre-trends in a more flexible way, including whether treated and control establishments were on different trajectories prior to debarment. Second, I change the algorithm to generate up to three and five control units, rather than just one counterfactual unit, for each treated establishment. Third, instead of using propensity score matching to construct a suitable comparison group, I rely on coarsened exact matching procedure (Iacus et al. (2012)). Overall, Figures D2 to D4 and Table D4 show that the main estimates are robust to these alternative matching approaches.

Treatment Intensity. Thus far, all specifications have considered debarment at the extensive margin without accounting for differences in the severity of illicit practices. Because I do not have data on irregularities that have prompted the sanctions, I alternatively utilize the length of

[36] In other words, the establishments need to appear in the RAIS data at least once in three years before debarment. This condition allows me to ensure that the establishments have been operating before debarment and recover some establishment-level information extracted from RAIS for the matching procedure, such as industry and location. This alternative sample restriction produces a sample of 4,649 establishments to implement the matching algorithm, which is similar to the one described in Section 3.4, except for a slight modification: when the establishment does not have any employee in any of the three years before debarment, I assign zero value.

debarment as a proxy for the intensity of the punishment. In general, more illicit practices entail longer punishments. I define establishments that are above and below the median of 12 months of punishment as those experiencing a higher and lower treatment intensity. I present the main results in Tables D5 to D7, Appendix D. I find that the impacts on establishments and workers as well as the reputation mechanism are substantially stronger among establishments experiencing a more intense treatment, consistent with debarment largely affecting establishments with more severe irregularities.

Firm-Level Results. While the institutional context indicates that the debarment occurs at the establishment level, I also examine the impacts of the policy on firms. Tables D8 and D9, Appendix D, show that the magnitudes of the establishment- and firm-level results are quite similar, which is unsurprising given that 88 percent of the debarred sample consists of single-establishment firms (Column (1) of Table 1). I also do not find evidence of significant spillovers in other establishments within firms, reinforcing that the results are primarily driven by the policy shock rather than a reallocation of activity across establishments within firms.

6 Concluding Remarks

Despite the importance of anti-corruption strategies in curbing illicit practices, their impacts on labor market outcomes remain understudied. Exploiting several features of the Brazilian context, this paper evaluates the effects of an instrument that has also been increasingly adopted by many countries: corporate debarment or blacklisting. I leverage novel data on debarment combined with matched employer-employee data and adopt a matched difference-in-differences approach to investigate how debarment affects establishments and workers.

I find that debarred establishments experience sizable and negative impacts on selected labor market outcomes. On average, they experience a 47.7 percent decline in total number of employees and are 15.1 percentage points more likely to exit the formal sector. In addition, using worker-level data, I show that employees' annual earnings fall by around 22 percent.

Three additional findings are consistent with the results being driven by the unemployment effects. First, I argue that the financial burden due to fines is unlikely to explain the results since the data indicates that very few excluded companies are fined. By law, the maximum cap for fines is 20 percent of annual gross revenues, which may be considered too small to justify the labor market impacts. Second, I provide evidence that relationships with the government are relevant as at least half of the sample has bid for procurement contracts with the federal government. I find a sizable decline in the probability of bidding for and winning procurement contracts after debarment as well as larger impacts for establishments with stronger connections with the government, supporting the prediction that debarment constitutes a severe demand shock. Third, I find small evidence of the role of information shock, although it contributes in some extent to the unemployment effects.

An important implication of my analysis is that anti-corruption policies may generate unintended impacts on other groups beyond the main target. In the case of debarment, it is still unclear what unit should be debarred (e.g., firm, owners or firm division) as the main target can largely vary across countries. In principle, sanctions designed to only affect particular agents, such as owners or a firm division, may be too weak to generate a credible threat as they can avoid such punishments or even redistribute their impacts to less influential sub-groups within the debarred firm (Draca et al. (2019)). On the other hand, debarment at the firm level has the potential to increase the costs of engaging in corrupt practices and generate larger benefits as its impacts can be easily distributed across both targeted and non-targeted sub-groups within the firm. This paper shows that the combination of demand and information shocks lead to losses for workers connected to punished firms. Although most sub-groups of workers are affected by the policy, I find that highly skilled and paid and more tenured workers experience higher unemployment and earning losses. The findings therefore suggest that policymakers should consider the employee costs created by debarment when considering the potential consequences of this policy instrument, not just the possibility of increasing corporate transparency and integrity. More broadly, in light of the ongoing debate surrounding the policy instruments to combat corruption, any evaluation of the consequences of a corruption crackdown should also consider the employee costs when weighing its costs and benefits.

More recently, Auriol and Søreide (2017) has theoretically shown that debarment is only effective when the number of competitor firms in the market is not too large, firms care about future sales streams, and the probability of being caught is sufficiently high. In other cases, debarment may be ineffective. Alternative policy recommendations should also consider coupling debarment with other complementary sanctions, such as fines, prison terms and financial incentives, to generate a larger deterrence effect. Evaluating the effectiveness of these policy instruments is an interesting avenue for future research.

The main limitation of this paper is that I am not able to estimate the benefits of debarment due to lack of additional data. Debarment may also induce substantial reallocation of resources from corrupt to (potentially) non-corrupt firms, and firm-level data on operational and nonoperational costs, revenues, assets, and investment from annual surveys would allow me, for instance, to observe whether corrupt firms make up for losing government contracts and whether debarment has a long-run effect on integrity and productivity or ultimately benefits firms' competitors. Removing competitors may also generate a reduced competition in the procurement market, resulting lower quality or higher prices, especially in oligopolistic markets (Cerrone et al. (2021)). These effects are the opposite of what procurement contracts are intended to deliver. These possible consequences of debarment are left for future work. Therefore, this paper should be viewed as a first step toward characterizing the impacts of debarment on the economy.

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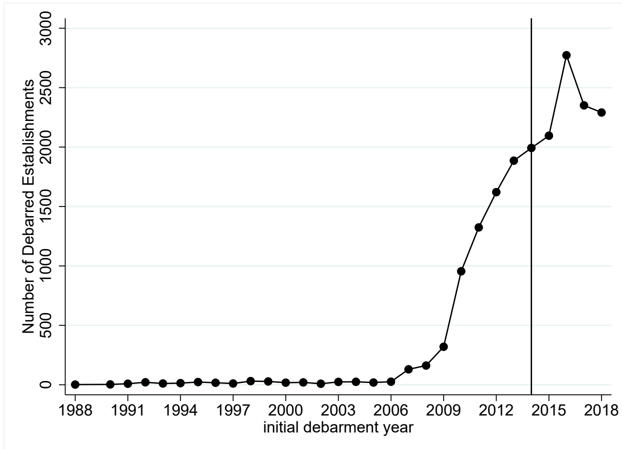
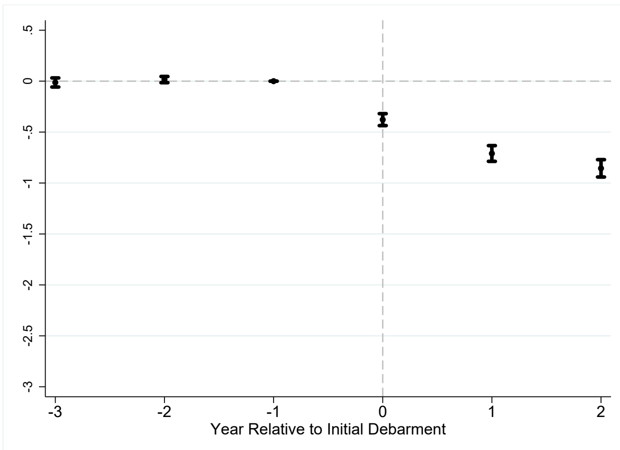
7 Figures

Figure 1: Evolution of Debarred Establishments

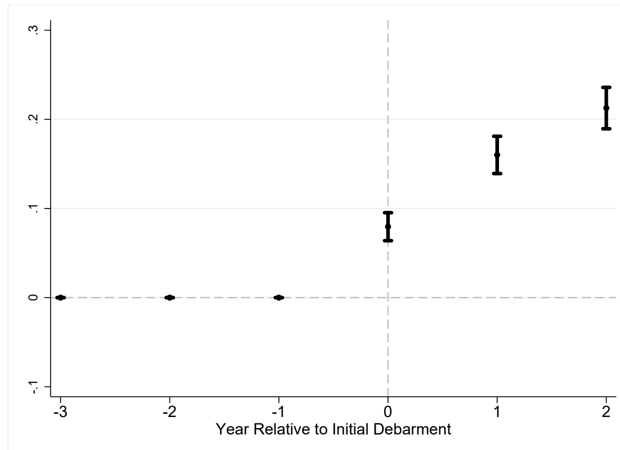


Note: Graph shows, on the left axis, how the total number of debarred establishments has rapidly evolved since 1988. Information is obtained from CEIS data, carried out by CGU.

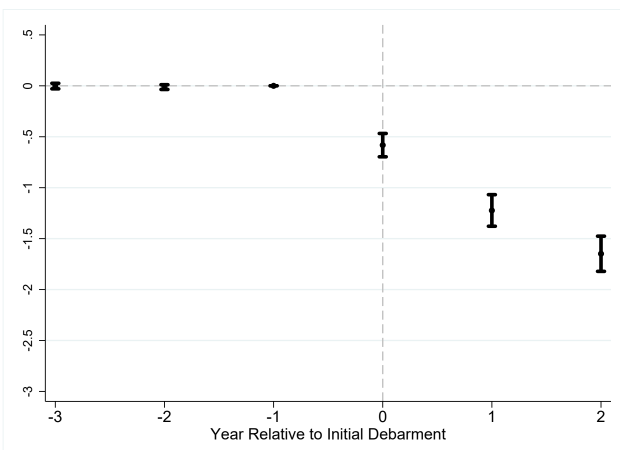
Figure 2: The Effects of Debarment on Establishments' Outcomes



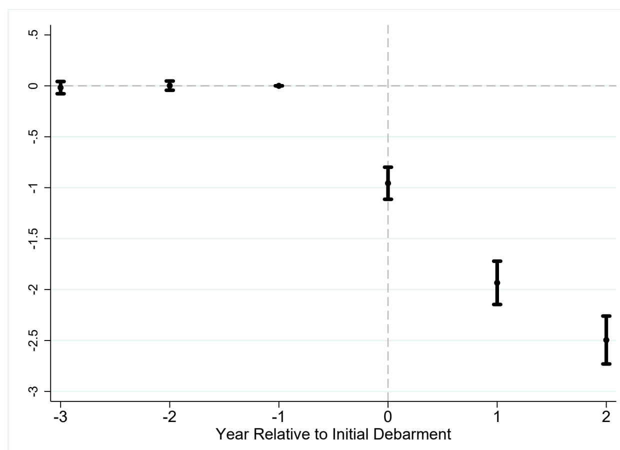
(a) Log Employment



(b) Exit



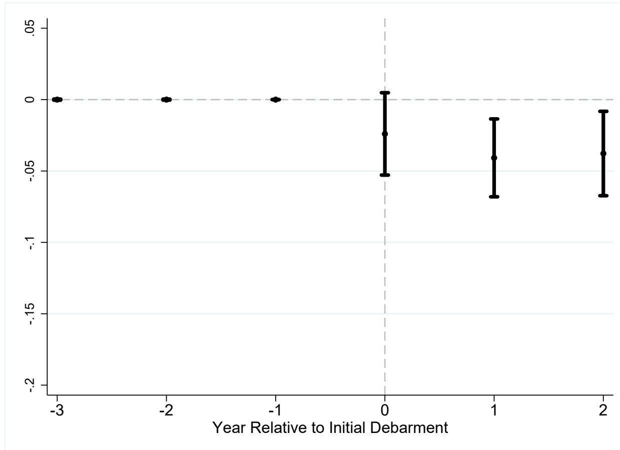
(c) Log (Uncond.) Earnings



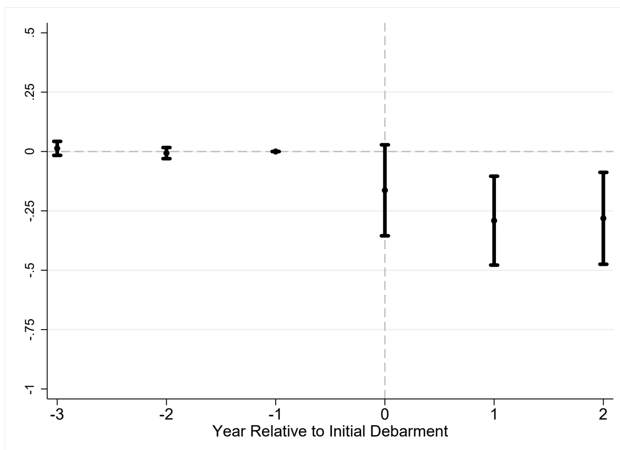
(d) Log (Uncond.) Payroll

Note: This figure reports point estimates of the annual effects of debarment on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before debarment. More details can be found in Table 3

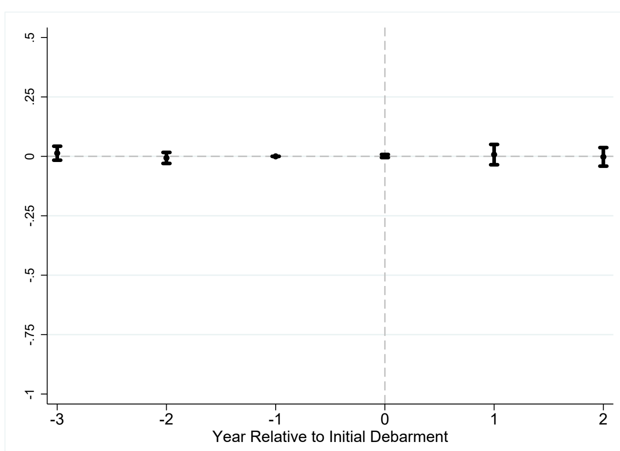
Figure 3: The Effects of Debarment on Workers' Outcomes



(a) Employment



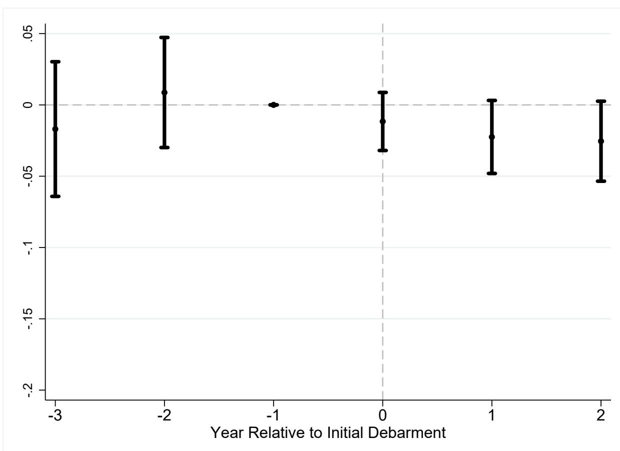
(b) Log (Uncond.) Earnings



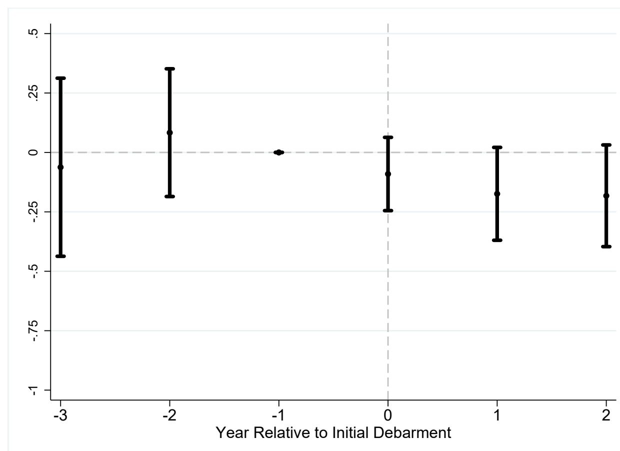
(c) Log (Cond.) Earnings

Note: This figure reports point estimates of the annual effects of debarment on different outcomes using the worker-level sample from RAIS data. The omitted category is the year before debarment. More details can be found in Table 4.

Figure 4: The Information Shock Channel



(a) Employment



(b) Log (Uncond.) Earnings

Note: This figure reports point estimates of the information shock channel of debarment on employment and unconditional log earnings. The omitted category is the year before debarment. More details can be found in Table 5

8 Tables

Table 1: Descriptive Statistics: Establishments

	(1)	(2)	(3)	(4)
	Debarred Establishments		**Control Establishments**	
	Mean	SD	Mean	SD
Main Variables				
# Employees	164.49	729.59	113.66	533.50
Log Employees	3.19	1.76	3.14	1.56
Monthly Payroll *(in reais)*	367,709.51	2,543,142.81	247,625.94	1,294,277.79
Log Monthly Payroll *(in reais)*	10.44	2.05	10.44	1.91
Average Earnings per Employee	1,823.04	1,337.89	1,958.27	1,544.03
Log Earnings per Employee	7.38	0.54	7.42	0.62
Bid for Procurement Contract	0.62	0.48	0.62	0.49
Win Procurement Contract	0.51	0.50	0.51	0.50
Single Establishment	0.88	0.33	0.88	0.33
Location				
Central-West Region	0.14	0.35	0.14	0.35
North Region	0.04	0.20	0.04	0.20
Northeast Region	0.15	0.36	0.15	0.36
South Region	0.22	0.41	0.22	0.41
Southeast Region	0.45	0.50	0.45	0.50
Average Municipality Population	2,234,094.66	3,422,168.89	2,255,615.53	3,453,405.58
Sector				
Construction	0.16	0.36	0.16	0.36
Commerce	0.41	0.49	0.41	0.49
Transp., Storage & Commun.	0.05	0.21	0.05	0.21
Transformation Industry	0.13	0.34	0.13	0.34
Real Estate	0.21	0.41	0.21	0.41
Other Categories	0.04	0.20	0.04	0.20
N	3,179		3,179	

Note: This table reports descriptive statistics for establishments using information from CEIS and RAIS data. The first two columns refer to a sample of debarred establishments that are matched to RAIS data. Columns (3) and (4) report summary statistics for the matched control group after implementing the matching algorithm. Further details on the matching algorithm are found in Section 3.4. Summary statistics are computed from RAIS data using the averages in the annual window [-3,-1] before debarment. The variables are: average and log number of employees, average and log monthly payroll (expressed in Brazilian *reais*), average and log earnings per worker (also expressed in Brazilian *reais*), an indicator for whether the establishment has bid for and won a procurement contract with the federal government between 2013 and 2018, an indicator for whether the establishment is a single establishment from the associated firm, indicator variables for whether the establishment is located in Central-West, North, Northeast, South and Southeast regions, average population of the municipality in which the establishment is located, and indicator variables for economic sector the establishment belongs to (administration, construction, commerce, transportation, storage and communication, transformation industry, or other sectors).

Table 2: Descriptive Statistics: Workers

	(1)	(2)	(3)	(4)
	Treated Workers		**Control Workers**	
	Mean	SD	Mean	SD
Earnings				
Earnings *(in reais)*	2,256.96	2,996.49	2,572.38	3,585.70
Log Earnings	7.29	1.32	7.35	1.44
Gender				
Male	0.62	0.49	0.63	0.48
Disability				
Disabled	0.02	0.13	0.01	0.11
Race				
White	0.46	0.50	0.46	0.50
Firm Size				
0-9 Employees	0.03	0.16	0.02	0.16
10-49 Employees	0.08	0.26	0.10	0.31
50-99 Employees	0.06	0.25	0.06	0.25
100+ Employees	0.83	0.37	0.81	0.40
Education				
Basic Education	0.41	0.49	0.39	0.49
High School	0.50	0.50	0.50	0.50
College	0.10	0.29	0.11	0.31
Age				
less or equal to 25	0.09	0.28	0.09	0.28
26-35	0.32	0.46	0.33	0.47
36-45	0.29	0.45	0.30	0.46
more than 45	0.30	0.46	0.29	0.45
Occupation				
Managerial	0.03	0.16	0.03	0.17
Professional	0.11	0.31	0.13	0.33
White Collar Lower Level	0.18	0.39	0.16	0.36
Blue Collar	0.68	0.47	0.68	0.47
Tenure				
3- years	0.57	0.49	0.53	0.50
4 years	0.14	0.34	0.14	0.34
5 years	0.09	0.29	0.08	0.28
6+ years	0.20	0.40	0.25	0.43
# Unique Workers	81,333		81,333	

Note: This table reports descriptive statistics for workers using information from CEIS and RAIS data. The first two columns refer to a sample of workers from debarred establishments. Columns (3) and (4) report summary statistics for the matched control workers. Further details on the sample and the matching algorithm are found in Section 3.4. Total number of unique workers is reported in the last row. The variables are: average and log earnings (in Brazilian *reais*), indicator variable for male worker, indicator variable for disability, indicator variable for whether the race is white, indicator variables for establishment size, indicator variables for whether the worker has basic education, high school and college education, indicator variables for different age groups (less or equal to 25 years old; 26–35; 36–45; above to 45), indicator variables for whether the worker holds a managerial, professional, white collar lower level and blue collar position, and indicator variables for different tenure lengths (three years or less, four years, five years, and six years or more).

Table 3: Effects of Debarment on Establishments' Outcomes

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.649***	0.151***	-1.147***	-1.790***
	(0.035)	(0.009)	(0.064)	(0.092)
Sample Size	38,148	38,148	38,148	38,148
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
# Establishments	6,358	6,358	6,358	6,358
# Debarred Establishments	3,179	3,179	3,179	3,179
Mean Dep. Var (Control)	3.14	0	7.42	10.44

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several establishments' outcomes restricted to the formal sector: log employment, likelihood of exiting the formal sector, log (unconditional) earnings and log (unconditional) monthly payroll using information from both CEIS and RAIS data. The estimation sample consists of annual window $[-3, 2]$ around debarment between 2014 and 2016. All columns refer to Equation (1). Number of establishments and establishment-year pairs is reported. Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

Table 4: Effects of Debarment on Workers' Outcomes

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
PostDebarment	-0.034**	-0.247**	-0.000
	(0.014)	(0.096)	(0.011)
Sample Size	975,996	975,996	883,131
Worker FE	✓	✓	✓
Year FE	✓	✓	✓
# Workers	162,666	162,666	162,666
# Treated Workers	81,333	81,333	81,333
Mean Dep. Var (Control)	1	7.35	7.35

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment between 2014 and 2016. Further details on how the sample is constructed can be found in Section 3.4. All columns refer to Equation (2). Number of workers, treated workers and worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

Table 5: The Information Shock Channel

	(1) employment	(2) employment	(3) log earnings	(4) log earnings
PostDebarment	-0.014	-0.012	-0.131*	-0.119*
	(0.010)	(0.009)	(0.072)	(0.068)
Sample Size	586,908	586,908	586,908	586,908
Worker FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Worker Controls	×	✓	×	✓
Mean Dep. Var (Control)	0.71	0.71	5.39	5.39

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table tests the information shock channel by estimating the aggregate effects of debarment on employment and log unconditional earnings in the formal sector using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment between 2014 and 2016. Further details on how the sample is constructed can be found in Section 5.3. All columns refer to Equation (2). In Columns (2) and (4), I add time-varying controls, such age and age squared. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

ONLINE APPENDIX (NOT FOR PUBLICATION): THE EMPLOYEE COSTS OF CORPORATE DEBARMENT IN PUBLIC PROCUREMENT

Christiane Szerman

A Appendix A – Additional Information from Debarment Data

Table A1: Descriptive Statistics Using CEIS Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Debarred (Initial Sample)		**Debarred Between 2008 and 2013**		**Debarred Between 2014 and 2016**		**Matched with RAIS data**		**Debarred (Final Sample)**	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Government Level										
Federal	0.62	0.48	0.66	0.47	0.65	0.48	0.62	0.48	0.62	0.49
State	0.31	0.46	0.31	0.46	0.30	0.46	0.32	0.47	0.33	0.47
Municipal	0.06	0.24	0.03	0.16	0.04	0.21	0.05	0.22	0.05	0.23
Government Branch										
Executive	0.73	0.44	0.70	0.46	0.71	0.45	0.75	0.43	0.75	0.43
Judiciary	0.20	0.40	0.22	0.41	0.22	0.41	0.19	0.39	0.19	0.39
Legislative	0.02	0.14	0.02	0.13	0.03	0.17	0.02	0.15	0.02	0.15
Others	0.05	0.21	0.07	0.25	0.04	0.20	0.04	0.19	0.04	0.18
Type of Sanction										
Impediment	0.40	0.49	0.32	0.47	0.43	0.50	0.45	0.50	0.45	0.50
Suspension	0.41	0.49	0.46	0.50	0.41	0.49	0.44	0.50	0.44	0.50
Prohibition	0.08	0.27	0.11	0.31	0.08	0.27	0.06	0.23	0.06	0.23
Others	0.11	0.31	0.10	0.31	0.07	0.26	0.05	0.23	0.05	0.22
Length (# months)	21.76	21.69	29.13	24.83	19.95	19.41	18.17	17.22	18.12	17.19
Location										
Central-West Region	0.32	0.47	0.40	0.49	0.34	0.47	0.33	0.47	0.33	0.47
North Region	0.06	0.23	0.05	0.21	0.05	0.21	0.04	0.20	0.04	0.20
Northeast Region	0.16	0.36	0.14	0.34	0.16	0.36	0.15	0.36	0.15	0.35
South Region	0.13	0.34	0.07	0.26	0.14	0.35	0.16	0.36	0.16	0.37
Southeast Region	0.30	0.46	0.28	0.45	0.32	0.46	0.32	0.47	0.33	0.47
N	19,112		6,268		6,862		3,294		3,179	

Note: This table reports descriptive statistics (mean and standard deviation) for establishments using information from CEIS database and RAIS data. The variables are drawn from the CEIS data. The first two columns refer to a sample of establishments listed in CEIS before being matched with RAIS data. Columns (3) and (4) consider establishments that are included in CEIS between 2008 and 2013, while Columns (5) and (6) consider establishments that are included in CEIS between 2014 and 2016. Columns (7) and (8) further restrict the sample to establishments debarred between 2014 and 2016 that are matched with RAIS data. Columns (9) and (10) describe the sample of establishments debarred between 2014 and 2016 after applying the matching algorithm. Further details on the matching algorithm are found in Section 3.4. The set of variables is the following: indicator variables for whether the establishment is punished by the federal, state or municipal government, indicator variables for which government branch is responsible for the punishment (executive, judiciary, legislative or neither of them), indicator variables for the types of sanctions, encompassing temporary **suspension** in participation in public bids, **impediment** of bidding and contracting with the public administration, **prohibition** of contracting with the public administration and receiving benefits and incentives, and **other categories**, average length of debarment, expressed in months, and indicator variables for whether the debarment occurred in Central-West, North, Northeast, South and Southeast regions. The total number of establishments in each sample restriction is reported in the last row.

Table A2: Descriptive Statistics for All Establishments

	(1)	(2)
	All Establishments	
	Mean	SD
Main Variables		
# Employees	18.27	135.62
Log Employees	1.87	1.11
Monthly Payroll *(in reais)*	38,444.55	446,004.27
Log Monthly Payroll *(in reais)*	8.75	1.61
Average Earnings per Employee	1,526.72	1,271.35
Average Log Earnings per Employee	7.18	0.66
Bid for Procurement Contract	0.01	0.09
Win Procurement Contract	0.01	0.07
Unique Firm	0.85	0.36
Location		
Central-West Region	0.09	0.28
North Region	0.04	0.19
Northeast Region	0.16	0.37
South Region	0.22	0.41
Southeast Region	0.50	0.50
Average Municipality Population	1,662,300.49	3,232,904.87
Sector		
Construction	0.04	0.20
Commerce	0.45	0.50
Transp., Storage & Commun.	0.05	0.22
Transformation Industry	0.11	0.31
Real Estate	0.13	0.34
Other Categories	0.22	0.41
N	4,058,403	

Note: This table reports descriptive statistics for all establishments in the formal sector using information from RAIS data. Summary statistics are computed from RAIS data using the averages between 2011 and 2013, the years before initial debarment in the main analysis. The variables are: average and log number of employees, average and log monthly payroll (expressed in Brazilian *reais*), average and log earnings per worker (also expressed in Brazilian *reais*), indicators for whether the establishment has bid for and won a procurement contract with the federal government between 2013 and 2018, an indicator for whether the establishment is a single establishment from the associated firm, indicator variables for whether the establishment is located in Central-West, North, Northeast, South and Southeast regions, average population of the municipality in which the establishment is located, and indicator variables for economic sector the establishment belongs to (construction, commerce, transportation, storage and communication, transformation industry, real estate, or other sectors).

B Appendix B – Data Appendix

This appendix provides further details on the data sources described in Section 3, including subsections on further data description and sample construction.

B.1 Data Description

The Debarment Data. The CEIS file contains the following variables: sanction identification number, process number, government agency responsible for the process, government agency's state, level of government agency (federal, state, or municipal), type of punished agent (establishment or individual), tax identifier, name, type of punishment, start and end dates of punishment, total amount of fine, status of punishment, cancellation date, reason for cancellation, reactivation date, and reason for reactivation. The CEIS file is available under a confidentiality agreement with CGU (*Controladoria Geral da União*).

The Labor Market Data. The *Relação Anual de Informações Sociais* (RAIS) is main labor market data source. RAIS is linked employer-employee register with worker and establishment tax identifiers collected by the Brazilian Ministry of Economy and is available under a confidentiality agreement with the agency. The raw RAIS data are mostly provided in state-year files and all variables are standardized across years. I use data spanning the years between 2002 and 2018.

The Procurement Data. Information on all online bidders and winners of procurement contracts obtained with the federal government are extracted from *Portal de Transparência* and *Compras Governamentais* websites, updated by the federal government. Due to data availability, the first year of data used in this paper is 2013.

Data on Municipal Population. Information on municipal population are drawn from the Demographic Census in 2010, the most recent Census data available. Although information on estimated annual population are available, they are more sensitive to measurement errors. The Demographic Census, sourced from IBGE (*Brazilian Institute of Geography and Statistics*), provides the most reliable information.

B.2 Sample Selection and Variable Construction

In order to get the main sample of debarred establishments, I make some restrictions to the CEIS dataset. I begin by restricting the analysis to establishments, as the original data also include punished individuals. To avoid duplicate observations, I maintain the earliest sanction each establishment has. I also remove establishments that had cancelled sanctions or invalid tax identification numbers. Lastly, I delete all sanctions starting before 2014 and after 2016. These restrictions altogether yield a sample of 6,862 establishments to be matched with RAIS data through establishment tax identifiers. When extending the analysis to include establishments that were debarred between 2008 and 2013, I replicate these steps by maintaining the relevant information.

The establishment tax identifier (CNPJ) is unique to a given establishment over time and consists of fourteen digits. The first eight digits correspond to the firm, while the last six digits correspond to an establishment within the firm. Therefore, as a robustness check, I also adopt an alternative sample restriction at the firm level that follows the same steps using the first eight digits of establishment tax identifier instead of all fourteen digits.

I describe here the steps to prepare the sample to be matched with the debarment data and the **variables** used in the establishment-level analysis. I compute the **total number of employees** and **total average payroll** for all establishments every year based on worker-level files. Payrolls are adjusted to 2018 Brazilian *reais*. Each establishment is assigned its modal legal classification, **municipality** (and, therefore, **state** and **region**), industry code, and establishment size group.[37] I then keep establishments from private sector based using legal classification of each establishment. I also remove units from the Brazilian Central Bank and with invalid industry codes. **Average earnings** are defined as total average payroll divided by number of employees. Considering the universe of all establishments that survived to former sample restrictions, I compute the **deciles of the employment and average earnings** in each of the years. I also generate an indicator variable for whether each establishment a **single establishment** each year.

Using the first two digits of industry code[38], each establishment is assigned to one of the following 17 **economic sectors (or industries)** (defined as sections by IBGE): agriculture, cattle, and forestry (section A); fishing (section B); extractive industries (section C), transformation industries (section D); production and distribution of electricity, gas, and water (section E); construction (section F); wholesale and retail trade; repair of motor vehicles and motorcycles (section G); accommodation and food service activities (section H); transporting, storage, and communication (section I); financial activities (section J); real estate activities (section K); public administration, defence, and social security (section L); education (section M); health and social services (section N); other services activities (section O); domestic services (section P); and international organizations and other extraterritorial institutions (section Q).

Other variables are also created using procurement and municipal population data, such as

[37] Size is recorded as a variable with 10 possible values: zero workers, up to 4 workers, between 5 and 9 workers, between 10 and 19 workers, between 20 and 49 workers, between 50 and 99 workers, between 100 and 249 workers, between 250 and 499 workers, between 500 and 999 workers, more than 1000 workers.

[38] Industry code follows the National Classification of Economic Activities (CNAE), versio 1.0, based on UN ISIC - International Standard Industrial Classification of All Economic Activities.

indicator for whether the establishment bids for procurement contracts with the federal government each year and **population group** for municipality where the establishment is located.[39]

B.3 Matching Procedure

I use the labor market, municipal population and procurement data to construct the matching algorithm for the **establishment sample**. For each establishment debarred in a given year of debarment, I consider a set of non-debarred establishments from the same economic sector and state as possible candidates for the control group. The algorithm uses information extracted from RAIS in all the three years before (years $t-3$, $t-2$ and $t-1$). More precisely, for each pair of economic sector and state, I run a simple linear probability model predicting treatment using the following set of variables as regressors: deciles of total employment in years $t-3$, $t-2$ and $t-1$, deciles of annual earnings in years $t-3$, $t-2$ and $t-1$, indicator variables for whether the establishment won procurement contracts with the federal government in years $t-3$, $t-2$ and $t-1$, indicator variable for whether the establishment bid for procurement contracts with the federal government in any of the three years before debarment, whether it is a single establishment in all of the three years before debarment, and population group representing the size of municipality where the establishment is located. For each treated establishment, I keep one control candidate with the closest propensity score. I also ensure that potential control establishments are not associated to more than one debarred establishment. I consecutively repeat the above steps for all years with debarment events.

As robustness checks, I consider the following changes in the matching algorithm. First, instead of considering the previous regressors from all the three years before debarment, I utilize regressors from year $t-1$ and from years $t-2$ and $t-1$ separately. Second, instead of using a simple linear probability model with the closest propensity score, I implement a more restrictive version of matching: one-to-one coarsened exact matching (Iacus et al. (2012)) with the same set of regressors as before (and, again, within pairs of economic sector and state). Third, instead of allowing only one control establishment with the closest propensity score for each treated establishment, I flexibly allow up to three and five control candidates with the highest propensity scores. I find very similar conclusions. Section 5.4 describes the results in detail.

For the worker-level analysis, I construct a **worker sample** also using a matching algorithm. In

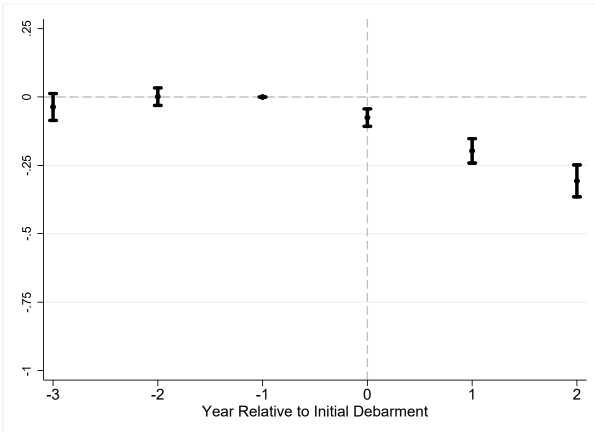
[39] I aggregate population information into seven groups: less than 5,000 inhabitants; equal or more than 5,000 and less than 10,000; equal or more than 10,000 and less than 20,000; equal or more than 20,000 and less than 50,000; equal or more than 50,000 and less than 100,000; equal or more than 100,000 and less than 500,000; and more than 500,000.

particular, from the set of matched debarred establishments (i.e., the final debarred establishment sample after the matching steps outlined above), I recover all individuals who worked in these establishments in all three years before they were debarred (years $t-3$, $t-2$ and $t-1$). I repeat this step for the set of matched control establishments. I then recover the relevant characteristics extracted from RAIS in year $t-1$: age groups (I create 5 years age bins), indicator for male, indicator for disability, indicator for white, educational categories (there are 11 educational categories: illiterate, incomplete primary education, primary education, incomplete lower secondary education, lower secondary education, incomplete upper secondary education, upper secondary education, incomplete tertiary education, tertiary education, Master degree, and PhD degree), occupational categories (more precisely, I use the 2002 Brazilian Classification of Occupations (CBO), which classifies jobs based on their skill and task content to construct four occupation categories: managerial, professional, blue collar, and white collar lower level positions), and economic sector. Thereafter, with the sample of workers from matched treated and control establishments in hand, I estimate a simple linear probability model predicting treatment using the above characteristics. For each worker from treated establishment, I keep one comparison worker from control establishment with the closest propensity score.

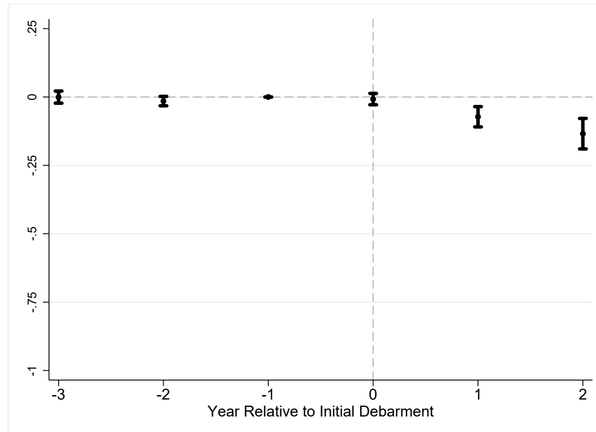
As a last step, the resulting establishment and worker samples are matched to relevant years of RAIS data (years $[t-3,\, t+2]$) to recover the outcomes of interest.

C Appendix C – Additional Results

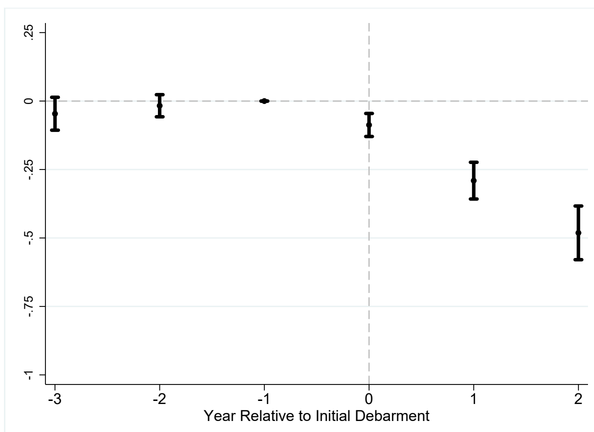
Figure C1: Effects of Debarment on Establishments' Outcomes Using Establishment Stayers



(a) Log Employment



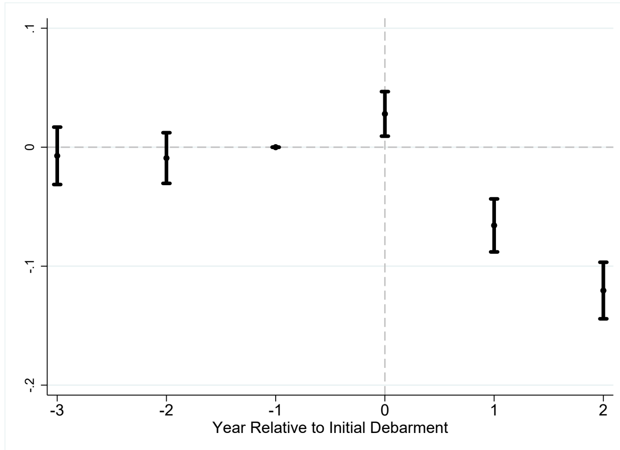
(b) Log Earnings



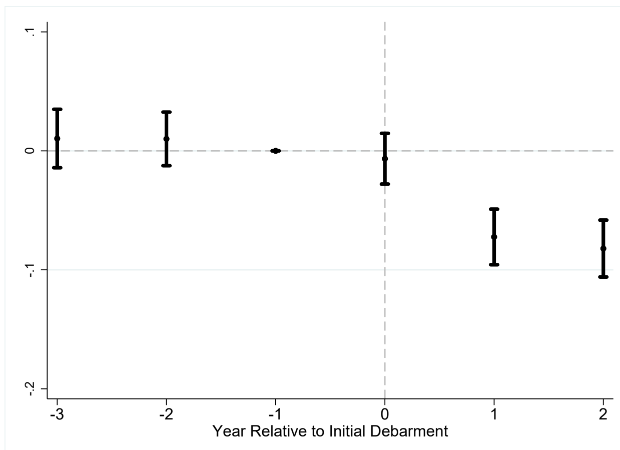
(c) Log Payroll

Note: This figure reports point estimates of the annual effects of debarment on different establishment-level outcomes from RAIS data. The omitted category is the year before debarment. The sample consists of establishment stayers, that is, active establishments that have at least one employee in all years from the window of [-3, 2] years around the debarment event. More details can be found in Table C1.

Figure C2: Effects of Debarment on Procurement Contracts



(a) Bidding Federal Contract



(b) Winning Federal Contract

Note: This figure reports point estimates of the annual effects of debarment on the likelihood of bidding for and winning procurement contracts with the federal government. The omitted category is the year before debarment. More details can be found in Table C6.

Table C1: Effects of Debarment on Establishments' Outcomes for Stayers

	(1) log employment	(2) log earnings (cond.)	(3) log payroll (cond.)
PostDebarment	-0.180***	-0.066***	-0.264***
	(0.024)	(0.016)	(0.034)
Sample Size	27,306	27,306	27,306
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
# Establishments	4,506	4,506	4,506
# Debarred Establishments	1,902	1,902	1,902
Mean Dep. Var (Control)	3.25	7.45	10.59

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several establishments' outcomes: log employment, log earnings and log monthly payroll in the formal sector using information from both CEIS database and RAIS data. The sample consists of active establishments that have at least one employee in all years of annual window [-3,2] around debarment between 2014 and 2016. I refer to them as establishment stayers. All columns refer to Equation (1). Number of establishments and establishment-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are clustered at the firm level.

Table C2: Robustness Checks: Establishments' Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Alternative Measures for Outcome Variables					
	employm. (level)	employm. (arc sin)	earnings (level)	earnings (arc sin)	payroll (level)	payroll (arc sin)
PostDebarment	-62.782***	-0.746***	-262.773***	-1.254***	-128,967.369***	-1.897***
	(10.672)	(0.040)	(24.444)	(0.070)	(24,730.618)	(0.098)
Sample Size	38,148	38,148	38,148	38,148	38,148	38,148
Establishment FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean Dep. Var (Control)	113.66	3.73	1,958.27	8.11	247,625.94	11.13

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports several robustness checks at the establishment-level using information from CEIS database and RAIS data. Odd columns refer to number of employees, earnings per worker and total monthly payroll as the dependent variables. Even columns consider the inverse hyperbolic sine transformation of these variables as the dependent variables. Number of establishment-year pairs is reported. Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

Table C3: Additional Robustness Checks: Establishments' Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Municipality-Specific Trends				**Industry-Specific Trends**			
	log employm.	exit	log earnings (uncond.)	log payroll (uncond.)	log employm.	exit	log earnings (uncond.)	log payroll (uncond.)
PostDebarment	-0.652***	0.154***	-1.155***	-1.800***	-0.648***	0.149***	-1.138***	-1.782***
	(0.033)	(0.008)	(0.060)	(0.086)	(0.031)	(0.008)	(0.059)	(0.083)
Sample Size	38,148	38,148	38,148	38,148	38,148	38,148	38,148	38,148
Establishment FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Municipality-Specific Trends	✓	✓	✓	✓	×	×	×	×
Industry-Specific Trends	×	×	×	×	✓	✓	✓	✓
Mean Dep. Var (Control)	3.14	0	7.42	10.44	3.14	0	7.42	10.44

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports several robustness checks using establishment-level sample extracted from CEIS and RAIS data. Columns (1)–(4) refer to the same set of outcome variables from Table 3 after including municipality-specific trends in the set of controls of Equation (1). Columns (5)–(8) add 2-digit industry-specific trends. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are clustered at the firm level.

Table C4: Heterogeneous Effects by Economic Sector

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: Construction (N = 5,976)				
PostDebarment	-0.679***	0.135***	-1.095***	-1.783***
	(0.089)	(0.022)	(0.161)	(0.232)
Panel B: Commerce (N = 15,648)				
PostDebarment	-0.374***	0.150***	-1.104***	-1.455***
	(0.032)	(0.012)	(0.089)	(0.111)
Panel C: Transp., Storage & Commun. (N = 1,824)				
PostDebarment	-0.722***	0.160***	-1.146***	-1.845***
	(0.156)	(0.038)	(0.282)	(0.406)
Panel D: Transf. Industry (N = 5,064)				
PostDebarment	-0.373***	0.084***	-0.639***	-1.014***
	(0.067)	(0.018)	(0.137)	(0.191)
Panel E: Real Estate (N = 8,124)				
PostDebarment	-1.307***	0.213***	-1.606***	-2.921***
	(0.096)	(0.018)	(0.131)	(0.215)
Panel F: Other Sectors (N = 1,512)				
PostDebarment	-0.673***	0.108**	-1.025***	-1.739***
	(0.159)	(0.043)	(0.305)	(0.430)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of debarment on establishments' outcomes by economic sector using establishment-level sample extracted from CEIS and RAIS data. All columns refer to Equation (1) restricted to one of the following sectors: construction, commerce, transportation, storage and communication, transformation industry, real estate or other sectors. Standard errors are clustered at the firm level.

Table C5: Heterogeneous Effects by Establishment Size

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: 0-9 Employees (N = 17,430)				
PostDebarment	-0.289***	0.144***	-1.059***	-1.317***
	(0.030)	(0.013)	(0.092)	(0.113)
Panel B: 10-49 Employees (N = 12,444)				
PostDebarment	-0.606***	0.120***	-0.965***	-1.591***
	(0.051)	(0.013)	(0.097)	(0.142)
Panel C: 50-99 Employees (N = 2,922)				
PostDebarment	-0.999***	0.171***	-1.356***	-2.373***
	(0.137)	(0.026)	(0.198)	(0.324)
Panel D: 100+ Employees (N = 5,352)				
PostDebarment	-1.606***	0.198***	-1.481***	-3.109***
	(0.134)	(0.020)	(0.150)	(0.277)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of debarment on establishments' outcomes by establishment size using establishment-level sample extracted from CEIS and RAIS data. All columns refer to Equation (1) restricted to one of the following sizes: 0-9 employees, 10-49 employees, 50-99 employees, and equal or more than 100 employees. Standard errors are clustered at the firm level.

Table C6: Effects of Debarment on Procurement Contracts

	(1) bidding contract	(2) bidding contract	(3) winning contract	(4) winning contract
PostDebarment	-0.047***	0.012	-0.061***	-0.007
	(0.008)	(0.010)	(0.008)	(0.010)
Sample Size	38,148	27,036	38,148	27,036
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Sample	All	Stayers	All	Stayers
Mean Dep. Var (Control)	0.36	0.38	0.29	0.31

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on the probability of bidding for and winning procurement contracts with the federal government using CEIS, procurement and RAIS data. In the first column, the sample consists of all establishments from the matching procedure described in Section 3.4 considering the period for which procurement data is available. In the second column, the sample is further restricted to establishments that have at least one employee in all years of annual window [-3,2] around debarment, the establishment stayers. All columns refer to Equation (1). Means of dependent variables are computed from pre-event years of the matched control group. Standard errors are clustered at the firm level.

Table C7: Heterogeneous Effects by Intensity of Connection with the Government

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: Sectors Less Connected with Government (N = 3,456)				
PostDebarment	-0.703***	0.129***	-1.031***	-1.733***
	(0.119)	(0.026)	(0.194)	(0.292)
Panel B: Sectors More Connected with Government (N = 34,692)				
PostDebarment	-0.645***	0.153***	-1.156***	-1.797***
	(0.034)	(0.008)	(0.062)	(0.088)
Panel C: Lower Dependence on Federal Contracts (N = 28,008)				
PostDebarment	-0.637***	0.135***	-1.040***	-1.677***
	(0.039)	(0.009)	(0.070)	(0.101)
Panel D: Higher Dependence on Federal Contracts (N = 10,140)				
PostDebarment	-0.681***	0.194***	-1.441***	-2.102***
	(0.055)	(0.015)	(0.108)	(0.148)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports heterogeneous effects of debarment by size on establishments' outcomes by intensity of connection with the government using establishment-level sample extracted from CEIS and RAIS data. All columns refer to Equation (1). I use two measures of government dependence. First, considering the universe of federal government contracts awarded in 2013, I create the distribution of total value of contracts by 2-digit industry and divide these sectors into medians. Panels A and B show the results after restricting the sample to sectors below and above the median to represent weaker and stronger connections with the government. Second, I compute the ratio of total revenues obtained from federal government contracts and the annual payroll for each establishment. Panels C and D report the estimates after restricting the sample to establishments with lower and higher dependence on these contracts measured as having less than and at least 25 percent of payroll expenses covered by revenues from federal contracts, respectively. Standard errors are clustered at the firm level.

Table C8: Robustness Check – Workers' Outcomes

	(1)	(2)	(3)	(4)	(5)
	Other Earnings Measures		**Occupation-Specific Trends**		
	(uncond.) earnings (sin transf.)	(cond.) earnings (sin transf.)	employment	log earnings (uncond.)	log earnings (cond.)
PostDebarment	-0.271**	-0.000	-0.034**	-0.247**	-0.000
	(0.106)	(0.011)	(0.014)	(0.097)	(0.011)
Sample Size	975,996	883,131	975,996	975,996	883,131
Worker FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Occupation-Specific Trends	×	×	✓	✓	✓
Mean Dep. Var (Control)	8.03	8.03	1	7.35	7.35

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports several robustness checks at the worker-level using information from CEIS and RAIS data. Columns (1) and (2) apply the inverse hyperbolic sine transformation on earnings. Columns (3)–(5) include 2-digit occupation-specific trends in the set of controls. Numbers of workers, is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm level.

Table C9: Heterogeneous Effects by Workers' Characteristics

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
Panel A: Gender			
PostDebarment $\times\mathbf{1}$(male)	-0.024	-0.137	-0.008
	(0.019)	(0.135)	(0.018)
Panel B: Disability			
PostDebarment $\times\mathbf{1}$(with disability)	-0.003	-0.124	-0.078*
	(0.027)	(0.186)	(0.046)
Panel C: Race			
PostDebarment $\times\mathbf{1}$(white)	-0.008	-0.128	-0.027*
	(0.021)	(0.140)	(0.016)
Panel D: Education			
PostDebarment $\times\mathbf{1}$(High School)	0.004	0.047	0.007
	(0.012)	(0.084)	(0.016)
PostDebarment $\times\mathbf{1}$(College)	-0.022	-0.201	-0.013
	(0.022)	(0.159)	(0.027)
Panel E: Age Group			
PostDebarment $\times\mathbf{1}$(age $<$ 25)	0.017	0.137	0.062***
	(0.016)	(0.110)	(0.016)
PostDebarment $\times\mathbf{1}$($26 \leq age < 36$)	0.013	0.118	0.082***
	(0.014)	(0.093)	(0.013)
PostDebarment $\times\mathbf{1}$($36 \leq age < 46$)	0.012	0.110	0.084***
	(0.011)	(0.076)	(0.012)
Panel F: Occupation			
PostDebarment $\times\mathbf{1}$(Managerial)	-0.068*	-0.520*	-0.024
	(0.040)	(0.270)	(0.027)
PostDebarment $\times\mathbf{1}$(Professional)	-0.053	-0.378*	0.012
	(0.035)	(0.226)	(0.023)
PostDebarment $\times\mathbf{1}$(Blue Collar)	-0.031	-0.178	0.015
	(0.037)	(0.229)	(0.018)
Panel G: Tenure			
PostDebarment $\times\mathbf{1}$(4 years)	0.021	0.166	-0.003
	(0.035)	(0.233)	(0.018)
PostDebarment $\times\mathbf{1}$(5 years)	0.005	0.078	0.020
	(0.018)	(0.124)	(0.021)
PostDebarment $\times\mathbf{1}$(6+ years)	-0.036**	-0.235**	0.007
	(0.017)	(0.119)	(0.019)
Panel I: Wage Distribution			
PostDebarment $\times\mathbf{1}$(Quintile 2)	-0.043	-0.306	0.054*
	(0.030)	(0.236)	(0.029)
PostDebarment $\times\mathbf{1}$(Quintile 3)	-0.022	-0.166	0.053*
	(0.030)	(0.242)	(0.030)
PostDebarment $\times\mathbf{1}$(Quintile 4)	-0.036	-0.264	0.067
	(0.030)	(0.242)	(0.030)
PostDebarment $\times\mathbf{1}$(Quintile 5)	-0.057*	-0.511**	0.016
	(0.031)	(0.253)	(0.032)
Sample Size	975,996	975,996	883,131
Worker FE	✓	✓	✓
Year FE	✓	✓	✓

Note: This table reports the effects of debarment on several workers' outcomes based on workers' characteristics in the year before the debarment event using information from CEIS and RAIS data. All columns refer to Equation (4). Only the estimates for β_4 are displayed. The dependent variables and sample are the same as in Table 2. I consider the following characteristics: indicator variable for male workers; indicator variable for disabled workers; indicator variables for educational levels (basic education is the omitted category); indicator variables for age groups (aged above 46 is the omitted category); indicator variables for occupational categories (white collar lower level position is the omitted category); indicator variables for tenure lengths (three years or less of tenure is the omitted category); and indicator variables for wage distributions (first quintile is the omitted category).

Table C10: The Information Shock Channel by Year of Separation

	(1) employment	(2) employment	(3) log earnings	(4) log earnings
Panel A (Separated One Year Before) (N = 176,112)				
PostDebarment	0.005	0.007	-0.009	0.005
	(0.024)	(0.023)	(0.174)	(0.176)
Mean Dep. Var (Control)	0.78	0.78	5.84	5.84
Panel B (Separated Two Years Before) (N = 226,818)				
PostDebarment	-0.026*	-0.025*	-0.241**	-0.233**
	(0.015)	(0.013)	(0.111)	(0.105)
Mean Dep. Var (Control)	0.70	0.70	5.31	5.31
Panel C (Separated Three Years Before) (N = 183,978)				
PostDebarment	-0.014	-0.012	-0.095	-0.081
	(0.012)	(0.011)	(0.093)	(0.088)
Mean Dep. Var (Control)	0.67	0.67	5.13	5.13
Worker FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Worker Controls	×	✓	×	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table tests the information shock channel by estimating the aggregate effects of debarment on employment and log unconditional earnings in the formal sector. I use information from CEIS and RAIS data. The estimation sample consists of annual window [-3,2] around debarment. All columns refer to Equation (1). In Columns (2) and (4), I add time-varying controls, such age and age squared. In Panel A, the sample consists of workers that have been displaced one year before the event. Similarly, Panels B and C consist of workers that have been displaced two and three years before the event, respectively. Further details on how the sample is constructed can be found in Section 5.3. Total number of worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

D Appendix D – Robustness Checks

In Appendix D, I provide a set of robustness tests for the main results in Section 5.

D.1 Debarment Prior to the Anti-Corruption Law in 2014

Table D1: Effects of Debarment on Establishments' Outcomes Prior to 2014

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.725***	0.132***	-0.952***	-1.685***
	(0.044)	(0.010)	(0.071)	(0.107)
Sample Size	32,136	32,136	32,136	32,136
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Dep. Var (Control)	3.10	0	7.26	10.24

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several establishments' outcomes restricted to the formal sector: log employment, likelihood of exiting the formal sector, log (unconditional) earnings and log (unconditional) monthly payroll using information from both CEIS and RAIS data. The estimation sample consists of annual window $[-3, 2]$ around debarment between 2008 and 2013. All columns refer to Equation (1). Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

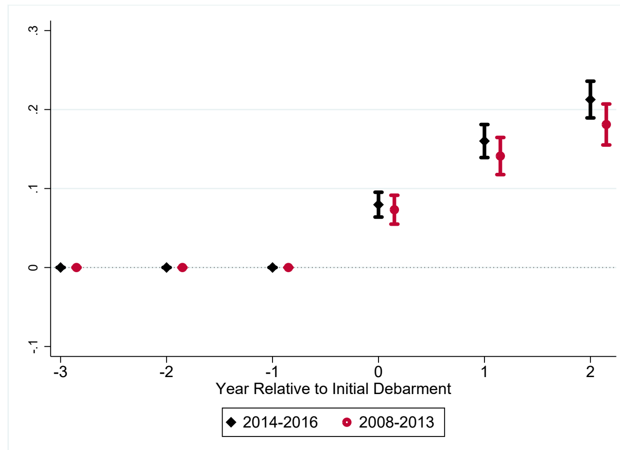
Table D2: Effects of Debarment on Workers' Outcomes Prior to 2014

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
PostDebarment	-0.044***	-0.280***	0.014
	(0.009)	(0.063)	(0.012)
Sample Size	474,156	474,156	436,559
Worker FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var (Control)	1	7.19	7.19

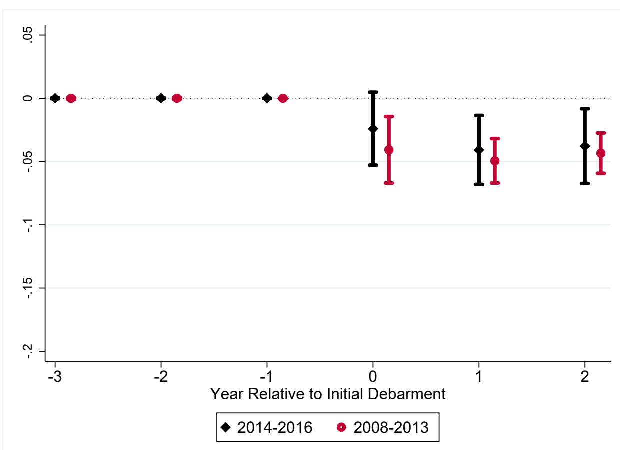
Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment between 2008 and 2013. All columns refer to Equation (2). Number of workers, treated workers and worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

Figure D1: Robustness Check: Debarment Prior to 2014

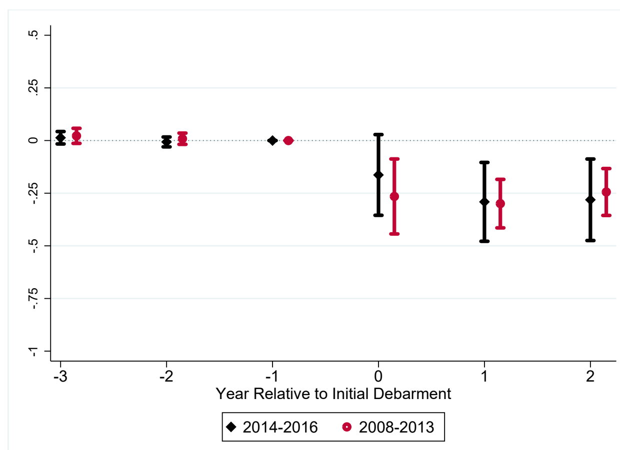
(a) Log Employment - Establishment



(b) Exit - Establishment



(c) Employment - Worker



(d) Log (Uncond.) Earnings - Worker

Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering establishments that have been debarred between 2014 and 2016 (in black) and between 2008 and 2013 (in red) separately. The omitted category is the year before debarment. More details can be found in Tables 3, 4, D1, and D2.

D.2 "No Employee" Restriction

Table D3: Effects of Debarment on Establishments' Outcomes: No "Employee" Restriction

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.507***	0.125***	-0.941***	-1.439***
	(0.030)	(0.008)	(0.062)	(0.085)
Sample Size	55,788	55,788	55,788	55,788
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
# Establishments	9,298	9,298	9,298	9,298
# Debarred Establishments	4,649	4,649	4,649	4,649
Mean Dep. Var (Control)	2.50	0.13	6.40	8.76

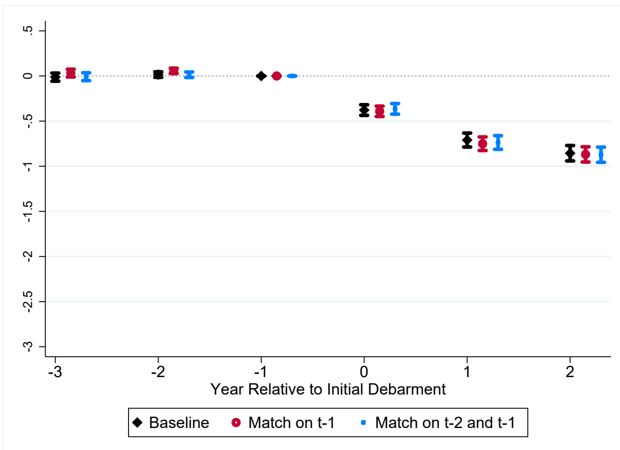
Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. I relax the tenure restriction described in Section 3.4 by allowing establishments to not have any employee in any of the three years prior to debarment. Further details on how the sample is constructed can be found in Section 5.4. The estimation sample consists of annual window [-3,2] around debarment between 2014 and 2016. All columns refer to Equation (2). Number of workers, treated workers and worker-year pairs is reported. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.

D.3 Alternative Matching Algorithms

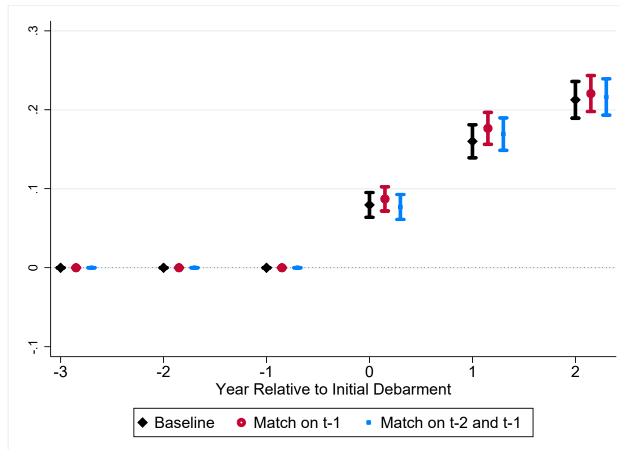
D.3.1 Matching on the Level

Instead of matching on three years before official debarment ($[t-3; t-1]$), I alternatively implement matching on the level ($t-1$), considering the

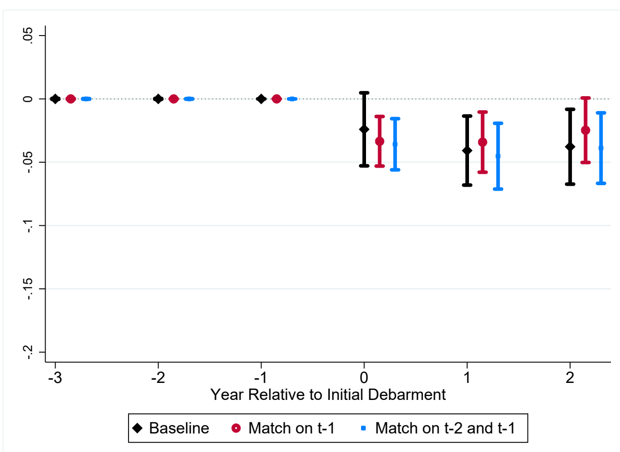
Figure D2: Robustness Check: Matching on Levels



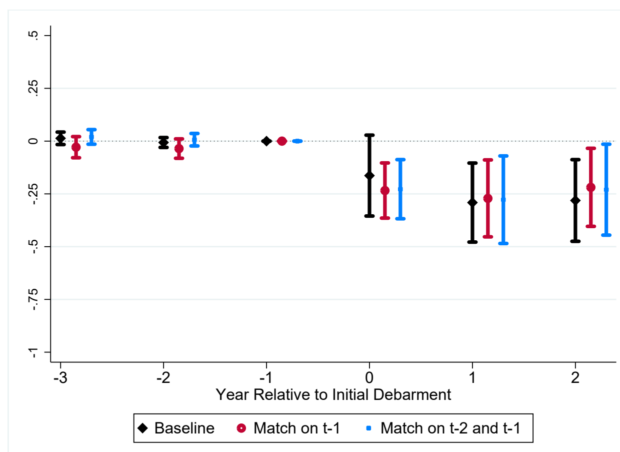
(a) Log Employment - Establishment



(b) Exit - Establishment



(c) Employment - Worker

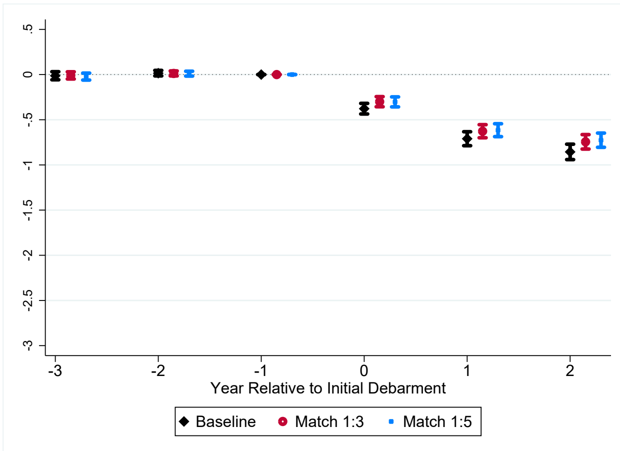


(d) Log (Uncond.) Earnings - Worker

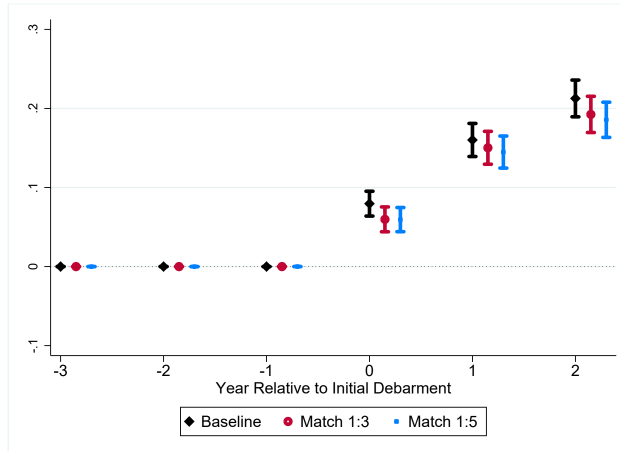
Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering three different matching algorithms: matching on three years before debarment (in black), matching on the year before debarment (in red), and matching on the two years before debarment (in blue). More details can be found in Section 5.4 and in Tables 3, 4, and D4.

D.3.2 Multiple Candidates

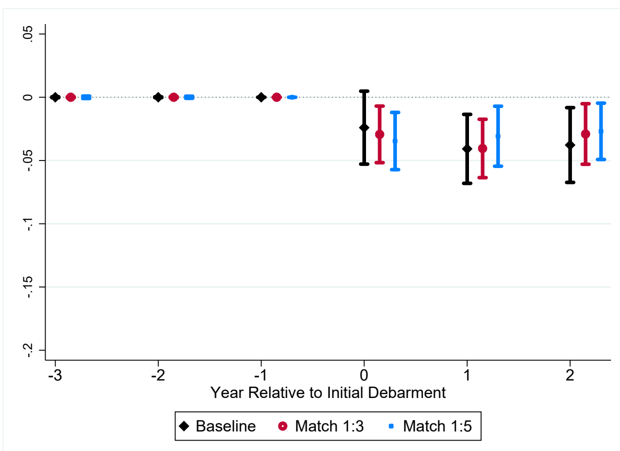
Figure D3: Robustness Check: Multiple Candidates



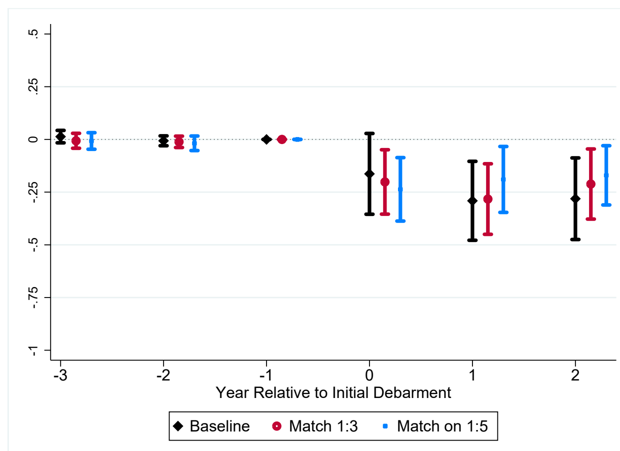
(a) Log Employment - Establishment



(b) Exit - Establishment



(c) Employment - Worker

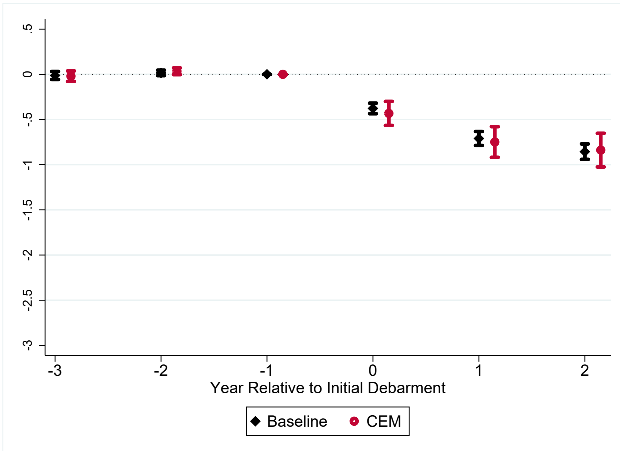


(d) Log (Uncond.) Earnings - Worker

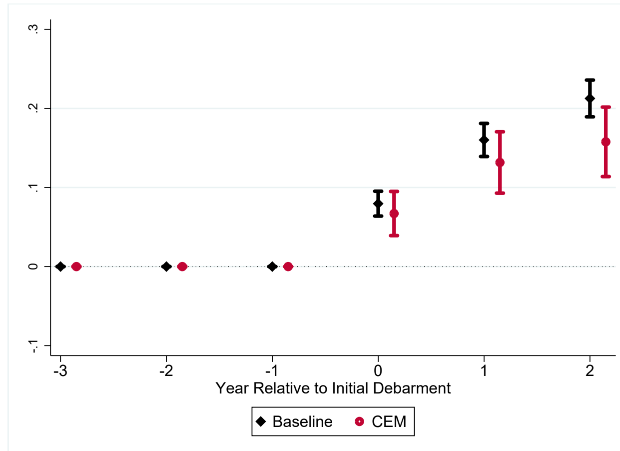
Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering three different matching algorithms: one-to-one matching (in black), one-to-three matching (in red), and one-to-five matching (in blue). More details can be found in Section 5.4 and in Tables 3, 4, and D4.

D.3.3 Coarsened Exact Matching

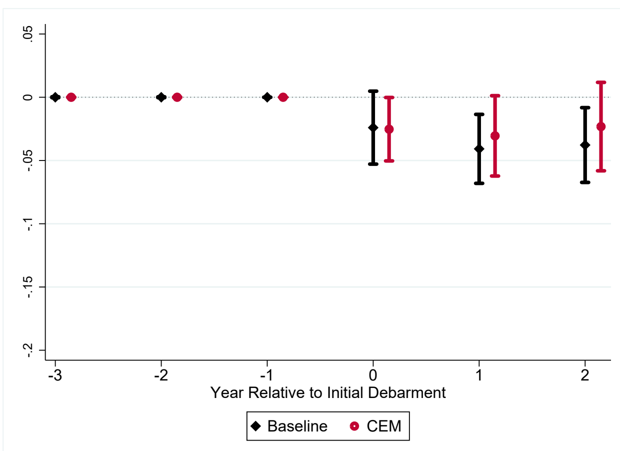
Figure D4: Robustness Check: Coarsened Exact Matching



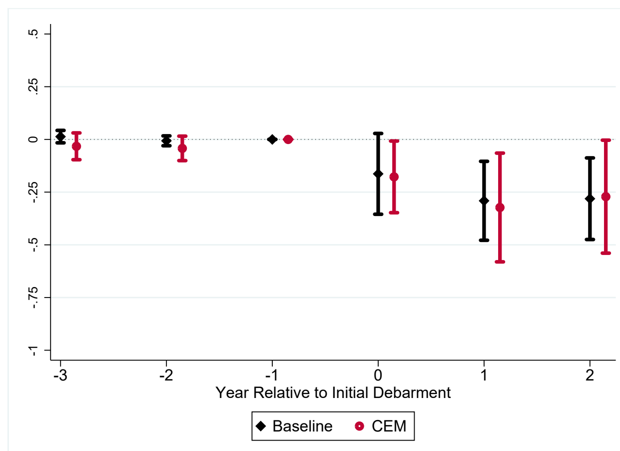
(a) Log Employment - Establishment



(b) Exit - Establishment



(c) Employment - Worker



(d) Log (Uncond.) Earnings - Worker

Note: This figure reports point estimates of the annual effects of debarment on selected outcomes considering two different matching algorithms: the baseline matching described in Section 3.4 (in black) and one-to-one coarsened exact matching (in red). More details can be found in Section 5.4 and in Tables 3, 4, and D4.

Table D4: Robustness Checks

	(1) log employment	(2) exit	(3) employment	(3) log earnings (uncond.)
	Panel A: Baseline			
PostDebarment	-0.649***	0.151***	-0.034**	-0.247**
	(0.035)	(0.009)	(0.014)	(0.096)
	Panel B: Match on t-1			
PostDebarment	-0.700***	0.161***	-0.031***	-0.220***
	(0.034)	(0.008)	(0.011)	(0.080)
	Panel C: Match on t-2 & t-1			
PostDebarment	-0.660***	0.154***	-0.040***	-0.254***
	(0.035)	(0.009)	(0.012)	(0.092)
	Panel D: 1:3 Matching			
PostDebarment	-0.557***	0.134***	-0.033***	-0.226***
	(0.033)	(0.009)	(0.011)	(0.079)
	Panel E: 1:5 Matching			
PostDebarment	-0.542***	0.129***	-0.031***	-0.190***
	(0.033)	(0.008)	(0.011)	(0.071)
	Panel F: Coarsened Exact Matching			
PostDebarment	-0.678***	0.119***	-0.026*	-0.232**
	(0.078)	(0.016)	(0.015)	(0.108)
Establishment FE	✓	✓	×	×
Worker FE	×	×	✓	✓
Year FE	✓	✓	✓	✓
Unit	Establishment	Establishment	Worker	Worker

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on selected establishments' and workers' outcomes: log employment, exit, indicator for employment, and log unconditional earnings. Columns (1) and (2) refer to Equation (1), while Columns (3) and (4) refer to Equation (3). Panel A considers the baseline results after implementing a one-to-one matching on all three years prior to debarment. Panels B and C report estimates after one-to-one matching on the year and on two years prior to debarment. Panels D and E refer to one-to-three and one-to-five matching. Panel F implements a one-to-one coarsened exact matching. Further details can be found in Section 5.4. Standard errors are clustered at the firm level.

D.4 Intensity

Table D5: Treatment Intensity: Establishment-Level Analysis

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
Panel A: Less Intense Treatment (N = 20,496)				
PostDebarment	-0.374***	0.104***	-0.793***	-1.154***
	(0.042)	(0.011)	(0.083)	(0.115)
Panel B: More Intense Intense Treatment (N = 16,704)				
PostDebarment	-0.986***	0.208***	-1.585***	-2.575***
	(0.059)	(0.014)	(0.101)	(0.148)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the heterogeneous effects of debarment on establishments' outcomes by treatment intensity using information from both CEIS and RAIS data. Panels A and B show the results after restricting the sample to establishments with debarment length below and above the median (12 months) to represent less and more intense treatments, respectively. The estimation sample consists of annual window $[-3, 2]$ around debarment. All columns refer to Equation (1). Standard errors are clustered at the firm level.

Table D6: Treatment Intensity: Worker-Level Analysis

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
Panel A: Less Intense Treatment			
PostDebarment	-0.016	-0.142	-0.005
	(0.022)	(0.149)	(0.016)
Sample Size	561,372	561,372	510,693
Panel B: More Intense Treatment			
PostDebarment	-0.058***	-0.391***	0.006
	(0.011)	(0.078)	(0.014)
Sample Size	402,636	402,636	361,960
Worker FE	✓	✓	✓
Year FE	✓	✓	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the heterogeneous effects of debarment on workers' outcomes by treatment intensity using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment. Panels A and B show the results after restricting the sample to establishments with debarment length below and above the median (12 months) to represent less and more intense treatments, respectively. All columns refer to Equation (2). Standard errors are two-way clustered at the worker and pre-event firm levels.

Table D7: Treatment Intensity: The Information Shock Channel

	(1) employment	(2) employment	(3) log earnings	(4) log earnings
Panel A: Less Intense Treatment (N = 288,252)				
PostDebarment	-0.008	-0.008	-0.109	-0.110
	(0.015)	(0.013)	(0.109)	(0.100)
Panel B: More Intense Treatment (N = 290,280)				
PostDebarment	-0.023*	-0.021**	-0.181**	-0.170**
	(0.012)	(0.011)	(0.090)	(0.085)
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Worker Controls	×	✓	×	✓

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table tests the information shock channel by estimating the heterogeneous effects of debarment on employment and log unconditional earnings in the formal sector by treatment intensity using information from both CEIS database and RAIS data. The estimation sample consists of annual window [-3,2] around debarment. Panels A and B show the results after restricting the sample to establishments with debarment length below and above the median (12 months) to represent less and more intense treatments, respectively. All columns refer to Equation (2). In Columns (2) and (4), I add time-varying controls, such age and age squared. Standard errors are two-way clustered at the worker and pre-event firm levels.

D.5 Firm-Level Results

Table D8: Effects of Debarment on Firms' Outcomes Using Firm-Level Sample

	(1) log employment	(2) exit	(3) log earnings (uncond.)	(4) log payroll (uncond.)
PostDebarment	-0.635***	0.143***	-1.096***	-1.732***
	(0.035)	(0.009)	(0.064)	(0.091)
Sample Size	38,484	38,484	38,484	38,484
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Dep. Var (Control)	3.21	0	7.13	10.22

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on several firms' outcomes restricted to the formal sector: log employment, likelihood of exiting the formal sector, log (unconditional) earnings and log (unconditional) monthly payroll using information from both CEIS and RAIS data. The firm-level estimation sample consists of annual window $[-3, 2]$ around debarment. Means of dependent variables are computed from pre-event years $[-3, -1]$ of the matched control group. Standard errors are clustered at the firm level.

Table D9: Effects of Debarment on Workers' Outcomes Using Firm-Level Sample

	(1) employment	(2) log earnings (uncond.)	(3) log earnings (cond.)
PostDebarment	-0.029***	-0.214***	-0.008
	(0.011)	(0.073)	(0.011)
Sample Size	1,274,652	1,274,652	1,158,547
Worker FE	✓	✓	✓
Year FE	✓	✓	✓
Mean Dep. Var (Control)	1	7.33	7.33

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of debarment on workers' outcomes in the formal sector: indicator for employment, log unconditional earnings and log conditional earnings using information from both CEIS database and RAIS data. The firm-level estimation sample consists of annual window [-3,2] around debarment. Means of dependent variables are computed from pre-event years [-3,-1] of the matched control group. Standard errors are two-way clustered at the worker and pre-event firm levels.