

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

By CHRISTIANE SZERMAN*

This paper studies an anticorruption policy—corporate debarment, or blacklisting—to understand how disclosing illicit corporate practices and the sanctions for these practices affect firm and worker outcomes. Exploiting a policy change in Brazil that imposed stricter penalties for corrupt firms, 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 after debarment. The impacts are driven by lost revenues from government contracts. The results shed light on the costs to workers in weighing the consequences of corruption crackdown. (JEL D73, E26, H57, H83, J31, K42, O17)

Corporate debarment, or blacklisting, is an important anticorruption 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 anticorruption strategies. Corporate debarment, in particular, has been increasingly used as a strategy to target corrupt companies and deter corruption in many countries, such as Brazil, China, France, the United Kingdom, and the United States (Zhou, Jin, and Wang 2017; Cerrone, Hermstrüwer, and Robalo 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

* Industrial Relations Section and Economics Department, Princeton University (email: cszerman@princeton.edu). Benjamin Olken was coeditor for this article. I am extremely grateful to Alexandre Mas and Thomas Fujiwara for their constant guidance and support. Ricardo Dahis has kindly shared some data used here and provided helpful suggestions. I have benefited from comments from Dorisz Albrecht, Natalie Bachas, Rebecca Diamond, Hanming Fang, Hank Farber, Mateus Ferraz Dias, Stephanie Hao, David S. Lee, Micaela Sviatschi, two anonymous referees, and seminar participants at Princeton University. I thank Controladoria-Geral da União (CGU) for providing access to data. Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of CGU or Princeton University. All remaining errors are mine.

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¹ Auriol and Søreide (2017) present a theoretical framework to conclude that debarment curbs corruption under certain conditions. Focusing on the United States, Cerrone, Hermstrüwer, and Robalo (2021) empirically study how debarment affects procurement auctions by reducing collusion.

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 anticorruption investigation uncovered an unprecedented and extensive kickback scheme to obtain government contracts. In June 2013, thousands of Brazilians protested against corruption and poor public service provision, prompting the largest protests in two decades. Motivated by general anticorruption sentiment, the National Congress announced several measures to curb corruption two months later. In particular, the Anti-Corruption Law was enacted, coming into effect at the beginning of 2014, and it substantially increased the costs to companies of engaging in illicit practices. 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, 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 fraud, and fraud 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 were debarred between 2014 and 2016, immediately after the enactment of the new law, to the Brazilian matched employer–employee data. The debarment data contain several characteristics of sanctions, such as their start and end dates. The labor market data provide 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.

²Past 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.

³Throughout the paper, I use the terms “earnings” and “wages in the formal sector” interchangeably.

To quantify how debarment affects establishments' performance, summarized by select labor market outcomes, I adopt event-study and matched difference-in-difference 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 are fined, the maximum cap is too small to explain my results entirely. Second, the public nature of debarment suggests there is scope for reputational damage. Excluded establishments may struggle to retain or attract clients and workers or to 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 that are 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-difference 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 decline 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 mitigate these concerns by allowing me to track the same workers over time.

I document that debarment generates significant earnings 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 who remain employed during the analysis period, suggesting that the bulk of earnings 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, highly paid, and more tenured workers experience relatively larger unemployment and earnings 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 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 earnings 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 crackdowns also adversely affect workers, especially those who are highly skilled and highly 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 earnings losses in the long run (Lachowska, Mas, and Woodbury 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 the fight against corruption (Svensson 2005). Anticorruption 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 anticorruption policies. In China, Lin et al. (2016) analyze how anticorruption reform impacts shareholder valuations. Zeume (2017) assesses the causal effects of bribery on firm value by exploiting an antibribery regulation in the United Kingdom. Karpoff, Lee, and Martin (2017) focus on the enforcement of the US Foreign Corrupt Practices Act (FCPA), which prohibits US companies from

paying bribes to foreign government officials. I build on this literature by tracing the impact of debarment, another popular anticorruption 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 anticorruption policy: local government audits (Ferraz and Finan 2011). Colonnelli and Prem (2017); Lagaras, Ponticelli, and Tsoutsoura (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 to foster civil society's 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) 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 US Congress required several government contracts to be awarded to the lowest responsible bidder (West et al. 2006). In 1928, the US 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, Hermstrüwer, and Robalo 2021) and, more broadly, the literature on policy instruments designed to reduce corruption.

⁴There is also another strand of literature focused on empirical evidence that negative reputational costs can be costly to firms (Karpoff, Lee, and Martin 2008; Murphy, Shrieves, and Tibbs 2009; Armour, Mayer, and Polo 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.

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

Third, this paper speaks to a strand of literature related to the earnings losses of displaced workers (Jacobson, LaLonde, and Sullivan 1993; Sullivan and Von Wachter 2009; Von Wachter, Handwerker, and Hildreth 2009; Couch and Placzek 2010; Von Wachter, Song, and Manchester 2011; Lachowska, Mas, and Woodbury 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.

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

I. Institutional Context

A. *The Creation of the CEIS Database*

Corruption has long been a concern in Brazil. In light of this problem, several anticorruption 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 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 that have been sanctioned and debarred for being engaged in illicit practices in bidding procedures, fiscal fraud, or fraud 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

⁶The database was launched in December 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 2010 (Portaria CGU 516).

⁷These illicit practices are typically characterized as noncompliance 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.

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 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.

B. 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 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 2013, imposing new measures to combat corruption, inspired by the international trend of anticorruption efforts.^{9,10} The new law came into effect in January 2014.

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 including the 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.

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

⁹For instance, the Anti-Corruption Law holds individuals giving out bribes equally as 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.

¹⁰International examples include stricter enforcement of the Foreign Corrupt Practices Acts (FCPA) in the United States as well as countries like China and the United Kingdom enacting new anticorruption measures or requiring more stringent enforcement of existing regulations.

¹¹The CEIS database contains the start date of debarment along with states, 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 changes in patterns after the Anti-Corruption Law for other states.

The administrative process, on the other hand, is conducted in a different way. The first step 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 R\$60 million (about US\$15 million) when calculating revenue is not possible, and the sanctions must be disseminated by 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.

C. 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 has 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 Correios¹³ procurements” (O Globo 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” (Frias 2016). In March 2016, another article announced that “CGU published the decision to ban another contractor [...] from contracting with public administration [...]. It was proven that Y coordinated actions with other competitors to reduce competitiveness in several bidding processes by combining prices and winners [...]. It was demonstrated that the company paid

¹²Despite the general requirement that administrative sanctions be disseminated by 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 report whether media were used to reveal the sanctions levied on punished companies and, if so, which media. Official government gazettes (e.g., *Diários Oficiais*) and relevant newspapers are some examples of media sources used to announce the sanctions.

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

bribes to public agents amounting to R\$3 million (equivalent to US\$0.75 million) to sign a contract with Petrobras [...]” (Frias 2016).¹⁴

These articles from relevant media outlets suggest that information about 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 visits to the CEIS platform jumped from 82,837 in January 2011 to 459,161 in December 2017, reaching a record high of nearly 600,000 visits in August 2017.

II. 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.

A. *The Debarment Data*

The confidential CEIS dataset (*brazilceis*) includes all establishments that have been debarred since its inception. It contains detailed information including names and tax identifiers, types of sanctions, start and end dates of sanctions, 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 canceled sanctions. Figure 1 shows the evolution of the number of debarred establishments. There is a clear increasing 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 the 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, in online Appendix A, displays summary statistics using information from CEIS data and

¹⁴ Petrobras is a semipublic Brazilian multinational corporation in the petroleum industry.

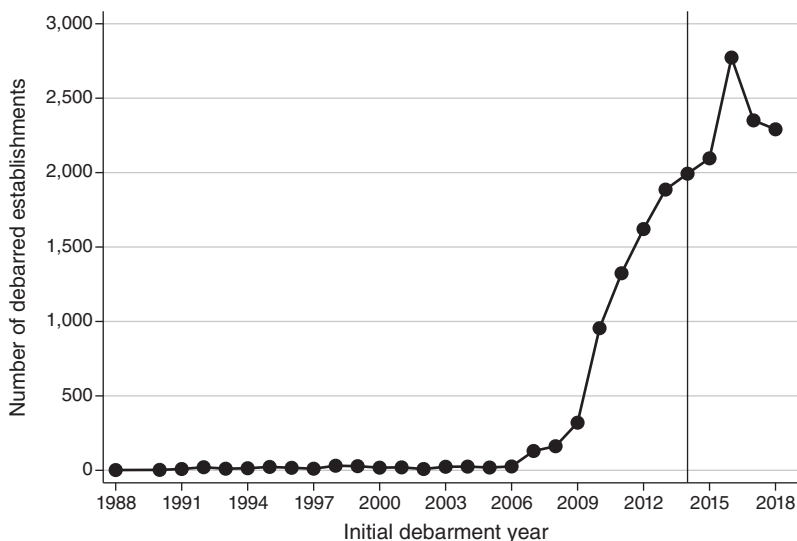


FIGURE 1. EVOLUTION OF DEBARRED ESTABLISHMENTS

Notes: The graph shows how the total number of debarred establishments has rapidly evolved since 1988. Information is obtained from CEIS data, carried out by CGU.

highlights several patterns. 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 a selection of more severe sanctions being reported to the CEIS platform before the Anti-Corruption Law. Second, the institutional context discussed in Section IC suggests a higher public awareness of corruption after 2014.¹⁵ Third, data on online bids and winners of federal procurement contracts, essential to probing 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 IVD, 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 after initial debarment, and these data are only available until 2018, I drop establishments that have been debarred since 2017. These restrictions leave me

¹⁵In a simple Becker (1968) 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 for 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.

with a sample of 6,862 establishments to be matched with employer–employee data through tax identifiers.¹⁶

B. 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 the period three years before debarment into the main analysis, I use annual 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, and admission date, along with other variables. Establishment-level information includes, for instance, industry, establishment size (in terms of number of employees), and the 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 have 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 or nonparticipation or due to 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

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

¹⁷Section IVD uses RAIS data for the period between 2002 and 2018.

¹⁸Columns 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 IID (columns 9 and 10).

C. *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 the 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).

D. *The Matching Procedure*

I construct two samples for the main analysis, which I refer to 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 nondebarred 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 industry²⁰ and the state with the closest propensity score, 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 the size of the 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 D, I describe the data and the

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

²⁰Appendix D includes a list of 17 industries generated for the analysis.

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

²²I 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).

TABLE 1—DESCRIPTIVE STATISTICS: ESTABLISHMENTS

| | Debarred establishments | | Control establishments | |
|--|-------------------------|--------------|------------------------|--------------|
| | Mean (1) | SD (2) | Mean (3) | SD (4) |
| <i>Main variables</i> | | | | |
| Number employees | 164.49 | 729.59 | 113.66 | 533.50 |
| log employees | 3.19 | 1.76 | 3.14 | 1.56 |
| Monthly payroll (\$R) | 367,709.51 | 2,543,142.81 | 247,625.94 | 1,294,277.79 |
| log monthly payroll (\$R) | 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 |
| <i>Location</i> | | | | |
| 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 |
| <i>Sector</i> | | | | |
| Construction | 0.16 | 0.36 | 0.16 | 0.36 |
| Commerce | 0.41 | 0.49 | 0.41 | 0.49 |
| Transportation, storage, and communication | 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 |
| Observations | 3,179 | | 3,179 | |

Notes: 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 IID. 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 the Central-West, North, Northeast, South, or Southeast regions; average population of the municipality in which the establishment is located; and indicator variables for the economic sector the establishment belongs to (administration, construction, commerce, transportation, storage and communication, transformation industry, or other sectors).

matching procedure in detail. In Section IVD, I show that the main results are robust to alternative ways of constructing the matching algorithm. In particular, I document that 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, King, and Porro 2012) instead of a propensity score matching, and to allowing multiple control units for each treated establishment.²³

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 with more than one debarred establishment. In Section V, 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.

The matching strategy is similar to other papers using a matched difference-in-difference 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 who worked in an establishment for the three years ($[t - 3, t - 1]$) before it was debarred. I apply a tenure restriction to extract workers who are more attached to companies, since Brazil has substantial turnover in the labor market. I also find similar results when alternatively considering individuals who have worked in a establishment in the year prior to debarment. The tenure restriction, however, offers the additional advantage of following the displacement literature (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010; Lachowsk, Mas, and Woodbury 2020) and allowing me to benchmark my results to this literature. Second, to keep the analysis comparable, the set of debarred establishments with 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 who 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 (five-year age bins), indicator for male, indicator for disability, indicator for White, educational level, two-digit industry category, and occupation category.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 yield a sample with 81,333 workers in the treated group.

E. 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 (R\$1,823, equivalent to US\$347) are similar

²⁴Educational 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's degree, and doctorate. For occupation, I use the 2002 Brazilian Classification of Occupations (CBO), which classifies jobs based on their skill and task content to construct four occupational categories: managerial, professional, blue-collar, and lower-level white-collar positions.

to the comparison group (R\$1,958, or US\$373). The two samples are also similar in terms of having bid for a procurement contract from the federal government, their locations, and their industries. We notice that the matching algorithm does not fully eliminate differences between debarred and nondebarred 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 of tenure in the year immediately prior to debarment for both the 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 of finding a balanced control group. The main difference comes from earnings. On average, in the year before debarment, workers in punished establishments earn about R\$2,256 (US\$430) per month, which is less than control workers' earnings (R\$2,572, or US\$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.

III. Empirical Strategy

A. 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:

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

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

TABLE 2—DESCRIPTIVE STATISTICS: WORKERS

| | Treated workers | | Control workers | |
|---------------------------|-----------------|-----------|-----------------|-----------|
| | Mean (1) | SD (2) | Mean (3) | SD (4) |
| <i>Earnings</i> | | | | |
| Earnings (\$R) | 2,256.96 | 2,996.49 | 2,572.38 | 3,585.70 |
| log earnings | 7.29 | 1.32 | 7.35 | 1.44 |
| <i>Gender</i> | | | | |
| Male | 0.62 | 0.49 | 0.63 | 0.48 |
| <i>Disability</i> | | | | |
| Disabled | 0.02 | 0.13 | 0.01 | 0.11 |
| <i>Race</i> | | | | |
| White | 0.46 | 0.50 | 0.46 | 0.50 |
| <i>Firm size</i> | | | | |
| 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 |
| <i>Education</i> | | | | |
| 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 |
| <i>Age</i> | | | | |
| Less than 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 |
| <i>Occupation</i> | | | | |
| 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 |
| <i>Tenure</i> | | | | |
| 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 |
| Number unique workers | 81,333 | | 81,333 | |

Notes: 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 IID. The total number of unique workers is reported in the last row. The variables are average and log earnings (\$R); indicator variable for male worker; indicator variable for disability; indicator variable for whether the worker is White; indicator variables for establishment size; indicator variables for whether the worker has basic education, high school education, or college education; indicator variables for different age groups (less than or equal to 25 years old, 26–35 years old, 36–45 years old, above 45 years old); indicator variables for whether the worker holds a managerial, professional, white-collar (lower-level), or blue-collar position; and indicator variables for different tenure lengths (three years or less, four years, five years, and six years or more).

their inclusion in the CEIS database. Standard errors are clustered at the firm level.²⁵

B. Worker-Level Analysis

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

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

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 who 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 dynamic 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.

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 of political connections during election years and thereby affect 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 coincides exactly with the timing of debarment, mitigating concerns related to unobservable shocks affecting the coefficients of interest.

IV. Results

A. 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

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

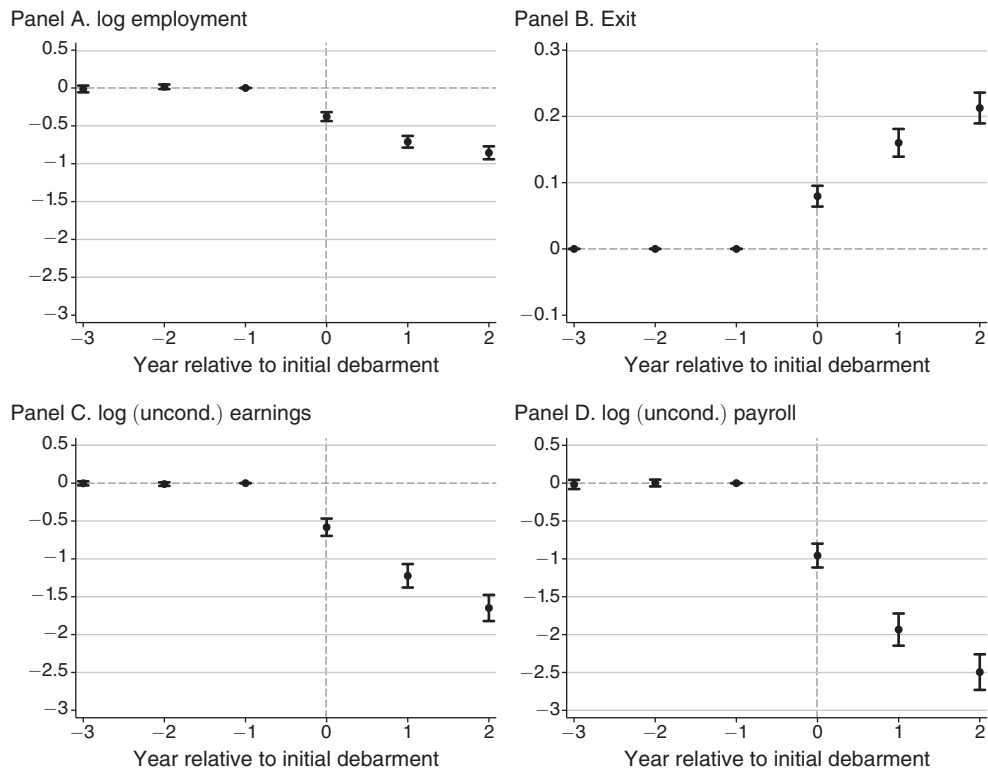


FIGURE 2. THE EFFECTS OF DEBARMENT ON ESTABLISHMENTS' OUTCOMES

Notes: 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.

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 2, panel 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 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

²⁶To 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.

TABLE 3—EFFECTS OF DEBARMENT ON ESTABLISHMENTS' OUTCOMES

| | log employment (1) | Exit (2) | log earnings (unconditional) (3) | log payroll (unconditional) (4) |
|-----------------------------------|--------------------------|------------------|--|---------------------------------------|
| Post-debarment | −0.649 (0.035) | 0.151 (0.009) | −1.147 (0.064) | −1.790 (0.092) |
| Sample size | 38,148 | 38,148 | 38,148 | 38,148 |
| Establishment fixed effects | ✓ | ✓ | ✓ | ✓ |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ |
| Establishments | 6,358 | 6,358 | 6,358 | 6,358 |
| Debarred establishments | 3,179 | 3,179 | 3,179 | 3,179 |
| Mean dependent variable (control) | 3.14 | 0 | 7.42 | 10.44 |

Notes: 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.

three years after debarment is -0.649 ($SE = 0.035$), equivalent to a 47.7 percent ($(100 \times [\exp(-0.649) - 1])$) decline in employment.²⁷

Figure 2, panel 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 more likely to exit the formal sector following debarment.

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 2, panel 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 2, panel D and column 4 of Table 3 report the findings for log monthly payroll, also suggesting substantial decline.²⁸

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

$$(3) \quad y_{jft} = \alpha_j + \alpha_t + \beta \times PostDebarment_{jft} + \varepsilon_{jft}.$$

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

²⁸The 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.

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 decrease 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 in 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 two-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 in 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 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, have 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 explain the drop in labor market outcomes entirely. 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 to secure credit after being debarred. I partially test for this mechanism in Section IVC 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

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

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 in Appendix C point to a sizable decline of 4.7 percentage points and 6.1 percentage points, respectively, 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 two-digit industry codes 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 the federal government.³² Table C7 in Appendix C indicates that the estimates are slightly larger among establishments more connected to the federal government. 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 literature examining the role of demand shocks in firm growth (Ferraz, Finan, and Szerman 2015; Pozzi and Schivardi 2016; Atkin, Khandelwal, and Osman 2017; Cho 2018; Gugler, Weichselbaumer, and Zulehner 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 to sell a broader set of products in other markets (Ferraz, Finan, and Szerman 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

³⁰The data only cover a subset of government purchases and are 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.

³¹Interestingly, for establishment stayers, columns 2 and 4 of Table C6 in Appendix C indicate smaller effects, suggesting that they are less affected by this shock.

³²The results are not sensitive to this threshold definition.

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

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.

B. 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.

Figure 3 presents 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, panel 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 Table 4 suggests a decline of 3.4 percentage points ($SE = 0.014$).

Figure 3, panel 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 decline in log earnings of -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 less likely to be employed in the formal sector and are therefore 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 earnings losses of 22 percent, I compare it to the worker displacement literature. For example, Jacobson, LaLonde, and Sullivan (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, Mas, and Woodbury (2020) find lost earnings of 33 and 45 percent at the time of displacement in Connecticut and Washington State. During the recession in the United States, 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

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

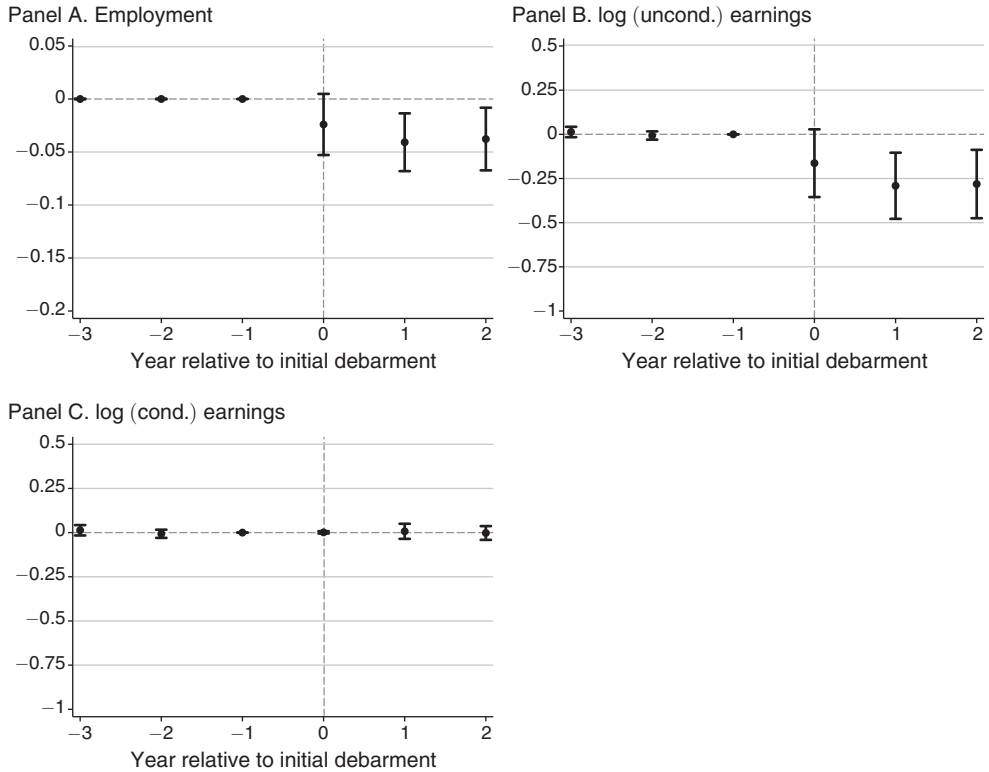


FIGURE 3. THE EFFECTS OF DEBARMENT ON WORKERS' OUTCOMES

Notes: 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.

years following displacement for workers displaced in mass layoffs between 2012 and 2014. In this paper, I show that the earnings losses of 22 percent from debarment are computed from a sample of workers regardless of whether they are displaced from punished establishments and that debarment leads to a 47.7 percent decline in employment. Therefore, the earnings 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 anticorruption instrument.³⁵ Table C9 in

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

$$(4) \quad y_{it} = \beta_1 \times Post_t + \beta_2 \times Post_t \times Debarment_i + \beta_3 \times Post_t \times Heterogeneity_i + \beta_4 \times Post_t \times Debarment_i \times Heterogeneity_i + \alpha_i + \alpha_t + \varepsilon_{it}$$

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.

TABLE 4—EFFECTS OF DEBARMENT ON WORKERS' OUTCOMES

| | Employment (1) | log earnings (unconditional) (2) | log earnings (conditional) (3) |
|-----------------------------------|-------------------|--|--------------------------------------|
| Post-debarment | −0.034 (0.014) | −0.247 (0.096) | −0.000 (0.011) |
| Sample size | 975,996 | 975,996 | 883,131 |
| Worker fixed effects | ✓ | ✓ | ✓ |
| Year fixed effects | ✓ | ✓ | ✓ |
| Workers | 162,666 | 162,666 | 162,666 |
| Treated workers | 81,333 | 81,333 | 81,333 |
| Mean dependent variable (control) | 1 | 7.35 | 7.35 |

Notes: This table reports the aggregate effects of debarment on workers' outcomes in the formal sector with indicators for employment, log unconditional earnings, and log conditional earnings using information from the 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 IID. All columns refer to equation (2). The numbers of workers, treated workers, and worker-year pairs are 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.

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 earnings losses, bearing higher costs of corruption crackdown.

C. Isolating the Information Shock Channel

I have shown that debarment is associated with negative effects on employment and unconditional earnings. As discussed in Section IVA, there are at least two reasons that may explain these findings. First, debarment is designed as an anticorruption 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 who are laid off prior to debarment events from the same set of establishments used in the establishment-level analysis. I track them over subsequent years, even they are working in different companies, to assess whether there are differences in labor market outcomes after their original firms

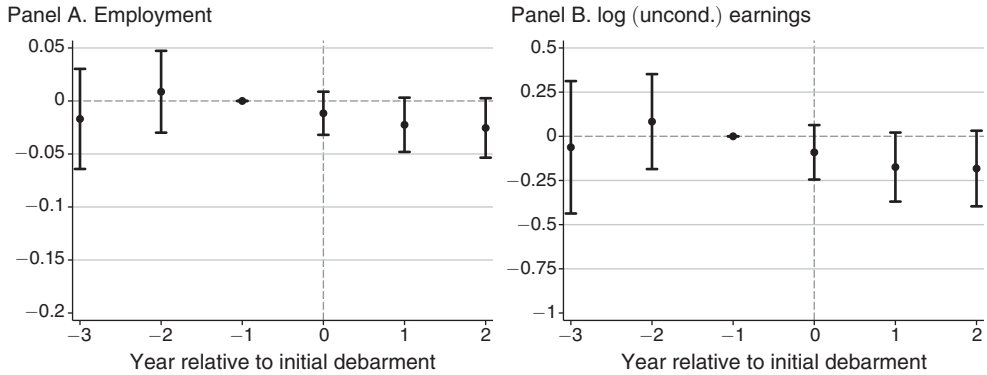


FIGURE 4. THE INFORMATION SHOCK CHANNEL

Notes: 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.

are 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 who have been laid off in any of the three years before debarment. Figure 4, panels A and 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 percentage points ($SE = 0.010$) and by 13.1 percentage points, or a 12.2 percent decrease ($SE = 0.072$), in employment and log earnings, respectively. When controlling for time-varying worker characteristics, such as a worker's age and age squared, the effect becomes smaller, although still negative.

In Table C10 of Appendix C, I consider different 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.

D. 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.

TABLE 5—THE INFORMATION SHOCK CHANNEL

| | Employment (1) | Employment (2) | log earnings (3) | log earnings (4) |
|-----------------------------------|-------------------|-------------------|------------------------|------------------------|
| Post-debarment | −0.014 (0.010) | −0.012 (0.009) | −0.131 (0.072) | −0.119 (0.068) |
| Sample size | 586,908 | 586,908 | 586,908 | 586,908 |
| Worker fixed effects | ✓ | ✓ | ✓ | ✓ |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ |
| Worker controls | | ✓ | | ✓ |
| Mean dependent variable (control) | 0.71 | 0.71 | 5.39 | 5.39 |

Note: 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 the 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 IVC. All columns refer to equation (2). In columns 2 and 4, I add time-varying controls such as 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.

Pre-2014 Debarment Analysis.—For the reasons outlined in Section IIA, the main analysis is restricted to establishments that were 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 in 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 IIB, 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 in 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

³⁶In other words, the establishments need to appear in the RAIS data at least once in the three years before debarment. This condition allows me to ensure that the establishments have been operating before debarment and to 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 IID, 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.

treated establishment. Third, instead of using propensity-score matching to construct a suitable comparison group, I rely on coarsened exact matching procedure (Iacus, King, and Porro 2012). Overall, Figures D2–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 the irregularities that have prompted the sanctions, I utilize the length of 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 Appendix D in Tables D5–D7. 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 in 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 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.

V. Concluding Remarks

Despite the importance of anticorruption 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 increasingly been adopted by many other countries: corporate debarment, or blacklisting. I leverage novel data on debarment combined with matched employer–employee data and adopt a matched difference-in-difference 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 indicate 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 to some extent to the unemployment effects.

An important implication of my analysis is that anticorruption 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 division of a firm), as the main target can vary across countries. In principle, sanctions designed to affect particular agents—such as owners or a division of a firm—may be too weak to generate a credible threat, as they can avoid such punishments or even redistribute their impacts to less influential subgroups 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 nontargeted subgroups 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 subgroups of workers are affected by the policy, I find that highly skilled, highly paid, and more tenured workers experience higher unemployment and earnings 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) have 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) noncorrupt 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 reduced competition in the procurement market, resulting in lower quality or higher prices, especially in oligopolistic markets (Cerrone, Hermstrüwer, and Robalo 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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