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Preventing forced labor

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PREVENTING FORCED LABOR: CAUSAL EVIDENCE FROM BOTH GOVERNMENT AND NON-GOVERNMENTAL INTERVENTIONS IN BRAZIL^{*}

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Abstract

This article assesses the impact of two types of interventions in curbing forced labor exploitation in Brazil. First, we analyze whether specialized state presence, in the form of Labor Offices and Labor Judicial Courts, diminishes forced labor in Brazilian municipalities. Second, we ask whether the educational interventions of a well-known NGO are effective in decreasing the use of slave-like labor in these localities. We use recently developed difference-in-differences estimators to identify the impact of these policies. Our results show that the detection of slave-like labor diminishes by 6.9% in the treated municipalities following the creation of Labor Courts, and by 6 to 26% following the interventions of the NGO. Due to lack of statistical power, we cannot say anything about the effect of the Labor Offices. Additionally, we provide suggestive evidence that the state presence in question promotes the formalization of workers performing jobs in the economic sectors that are more intensive in slave-like labor.

Key-words: Forced labor; State presence; Non-governmental intervention.

JEL Classification: J28; J47; K31; K42

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1 Introduction

In 1948, the Universal Declaration of Human Rights determined that “slavery and the slave trade shall be prohibited in all their forms.”. Even though, in the 21st century, several countries have registered the now-called “contemporary forms of slavery” and struggle to combat forced labor. The International Labor Organization estimates that around 24.9 million people worldwide have their Human Rights violated and are coerced - often through the use of violence - to carry out forced labor. In this context, understanding policies that can contribute to undermining these perverse practices and helping people submitted to this condition are imperative.

The context of Brazil, where slavery, formally abolished more than 130 years ago, deep-marked the society and culture (Schwarcz and Starling, 2018), is particularly appealing to understand how government and non-governmental organizations can affect the persistence of contemporary forced labor. Although international authorities have praised the Brazilian effort against such misdeed (McGrath, 2013), around 54,000 workers were rescued in slave-like conditions between 1995 and 2019 in the country, and it remains a problem to this day.

The country has a great challenge ahead to honor the global commitment made in Target 8.7 of the Sustainable Development Goals to end all forms of forced labor by 2030 and end the forced labor of children, along with all other forms of child labor, by 2025. Some solutions have been proposed to achieve these goals, as the actions recommended in the 2014 Forced Labor Protocol and Recommendation. However, it is also critical to have evidence of interventions that have a causal impact on reducing forced labor and to quantify that impact.

In an attempt to provide evidence on what policies may curb the exploitation of forced labor, our research question is two-fold: First, we ask how Labor Offices, recently explored by Ponczek and Ulyseia (2021), as well as Labor Judicial courts, may deter the use of forced labor in Brazilian municipalities otherwise neglected by these institutions. We exploit the roll-out of offices and courts across municipalities in different periods and hypothesize that this dimension of state presence enforces labor regulation in otherwise overlooked areas. Using the same identification strategy that exploits staggered implementation, our second question is whether the presence of non-governmental institutions may act as a significant deterrent to the use of forced labor. In this sense, we analyze the impact of the "Escravo, nem pensar!" program (translated as "Slavery, No Way!"), supported by ILO,¹ on the use of forced labor. "Escravo, nem pensar!" is carried out by the well-known Brazilian NGO Repórter Brasil, the most prominent third sector initiative to fight slave-like labor in Brazil.

Using recently developed difference-in-differences estimators and exploiting the

¹<https://escravonempensar.org.br/sobre/quem-apoia/> Last accessed on March 28, 2022.

staggered implementation of the treatment (Callaway and Sant'Anna, 2021), we find that the presence of Labor Courts in a municipality reduces the use of detected slave-like labor, on average, by 6.9%, although results are not significant throughout robustness tests. Regarding the creation of Labor Offices, we do not find any effect in terms of the detection of forced labor, probably due to the small sample size/reduced statistical power (30 treated units). The interventions by the NGO's program, on the other hand, reduce this same outcome by 6.26% depending on the specification. Moreover, we provide suggestive evidence that the state presence in question promotes the formalization of workers performing jobs in the economic sectors that are more intensive in slave-like labor.

In terms of the contributions of our findings, this paper speaks to different strands of the literature. First, our work speaks to the (mostly descriptive) literature on the roots of modern forced labor, its current determinants, and the impacts of preventive initiatives aimed to reduce the prevalence of slave-like labor (Bales, 2004; Sakamoto, 2009). Second, our work relates to previous papers on the impact of state presence on legal violations (Avis, Ferraz and Finan, 2018; Litschig and Zamboni, 2019). Third, we contribute to the literature on the impact of access to justice and increased enforcement of labor regulation on the conditions of local labor markets (Almeida and Carneiro, 2012; Ponczek and Ulysea, 2021; Espinosa, Desrieux and Ferracci, 2018).

The remainder of this paper is as follows. Section 2 provides a background of the use of forced labor in Brazil, as well as the main public and third sector initiatives in curbing its exploitation. Section 3 presents our main data sources and descriptive evidence. Section 4 details the empirical strategy used to estimate the impact of each intervention analyzed in combating the use of forced labor. Section 5 provides the findings of our research. Section 6 provides suggestive evidence of what happens in the local labor markets after each intervention. Finally, section 7 presents our concluding remarks.

2 Institutional Background and Previous Literature

This section provides institutional background on forced labor in Brazil and on each of the interventions analyzed in the paper. Subsection 2.1 describes modern forced labor in Brazil and the legislation to fight it. Subsection 2.2 explains the relevance of Labor Offices in enforcing labor regulations and curbing the exploitation of forced labor. Subsection 2.3 motivates how the increased presence of the judicial system affects labor markets and may also help diminish the prevalence of forced labor. At last, 2.4 describes the program "Escravo, nem pensar!", carried out by Repórter Brasil, which is currently the most prominent third sector initiative to fight slave-like labor in the country.

2.1 Forced labor in Brazil

The ILO Convention No. 29 (1930) defines work analogous to Slavery as “all work or service which is exacted from any person under the menace of any penalty and for which the said person has not offered himself voluntarily”.² Along these lines, the Brazilian Ministry of Labor and Employment (*Ministério do Trabalho e Emprego*, hereafter, MTE) describes how this crime occurs in Brazil in its Manual for Combating Slave Labor. Contractors for estate owners called “gatos” (in English, “cats”) approach vulnerable workers and propose a contract for them to provide services in agricultural establishments generally far from their cities of origin.

The poverty situation usually compels these workers to accept the offer spontaneously, almost always combined with an “advance” of salary to finance expenses with displacement, food, and pensions where they - sometimes with the family - will stay waiting for the job. When starting the activity, the worker is charged for expenses related to tools, accommodation, and food at the place of service, in addition to being forced to buy “on credit” groceries and other products necessary for the subsistence of his family, often at prices higher than the market price. The increasing debt soon exceeds the promised wage – from which it is deducted – and the worker is forced to continue working to pay it off. The employer, then, as a “guarantee” for the debt, starts to retain the worker’s documents and, at times, prevent them from leaving, resorting to armed surveillance.

Table 1 explores data from the Brazilian Ministry of Labor and Employment on rescued forced-labor workers to confirm that these workers come from impoverished backgrounds. Mostly, these workers are relatively young (32 years old, on average), predominantly black (47% of those for which we observe the ethnicity), primarily male (95% of those for which we observe gender), and have not completed elementary schooling (72% of those for which we observe education).

Having noticed the vulnerability of workers in this condition, along with the high prevalence of forced labor, effective policy to combat slave labor in Brazil started to gain momentum in 2003, despite previous legal frameworks that typified it as a crime. Previously, especially before the 1990s, the advances experienced by this policy were practically restricted to initiatives in the legal-normative field, with minimal consequences in terms of creating and consolidating an adequate state capacity to combat slave labor.

The crime of submitting someone to a condition analogous to that of an enslaved person appeared for the first time in the Brazilian Penal Code (1940). In 1957, Brazil ratified the Convention of the International Labor Organization (ILO) n° 29, bringing the international concept of slave labor to the national context. However, the Penal Code was noted for being unspecific and falling short of its goal of criminalizing contemporary slavery. In

²Other than Convention No. 29, ILO also has a statistical guideline to define forced labor. Please refer to page 2, paragraph 5 in https://www.ilo.org/wcmsp5/groups/public/---dgreports/---stat/documents/meetingdocument/wcms_648619.pdf. Last accessed on July 12, 2022.

this context, until the end of the last century, cases of identification, investigation, and punishment associated with the crime of placing a worker in a situation analogous to slavery were rare.

The creation of the Pastoral Land Commission (*Comissão Pastoral da Terra*, hereafter CPT) in 1975 was an important event for the recognition of the existence of contemporary slave labor in the country. Since then, complaints about the existence of such practices have gained public repercussion. With increased protests and public pressure, the government began a more incisive action against this crime.

In March 1994, under the Ministry of Labor and Employment, Normative Instruction No. 1 was published, which provided for the first time the procedures for labor inspection in rural areas. The Ministry of Labor and Employment is responsible for inspecting workers in conditions analogous to Slavery. But, due to the complexity of the crime, it was necessary to create integrated actions with other public agencies.

In 1995, the federal government created the Special Mobile Inspection Group (*Grupo Especial de Fiscalização Móvel*, hereafter GEFM), granting it the special authority to investigate allegations of the exploitation of slave labor in rural areas. This action marks the federalization of the policy to combat slave labor. Operating since 1995, the GEFM is composed of labor inspectors (Ministry of Labor and Employment), Federal Police agents, and prosecutors of the Public Ministry of Labor (MPT), possibly including representatives of the Brazilian Institute for the Environment and Renewable Natural Resources (IBAMA) and the National Institute for Colonization and Agrarian Reform (INCRA).

However, the administrative structure set up for inspections proved to be insufficient. Starting in 2003, the federal government launched the first and instituted the National Commission for the Eradication of Slave Labor (Conatrae). It declared the eradication of slave labor as a priority in Brazil and had as proposals (*i*) the improvement of the logistical structure of mobile inspection groups and (*ii*) the settlement of Labors Offices (which we consider as our main measure of state presence) in locations with frequent reports of practices of slave labor exploitation, notably the countryside of the states of Pará, Mato Grosso, and Maranhão.

Within this Plan, a fundamental innovation was instituted for workers rescued from the condition analogous to Slavery since December 2002: Law No. 10,608 granted them the right to receive three installments of unemployment insurance in the amount of the minimum wage each (today, approximately R\$ 1,212/month). This significantly expanded the social protection for rescued workers. After 2003, there were several changes in the internal rules of the Ministry of Labor and Employment. Most of them consisted of adjustments in the inspection procedures by labor auditors. However, it is worth mentioning the creation of the 2nd Plan to Combat Slave Labor in 2008, which incorporated the experience of the previous Plan and stipulated new goals to combat slave labor.

2.2 Enforcement of labor laws

Given the weak compliance with labor regulations that pervades developing and middle-income countries, the State tends to enforce labor laws through physical inspections (Almeida and Carneiro, 2012). In Brazil, this is carried out by the Ministry of Labor and Employment in the figure of Labor Offices located across some municipalities. Hereafter, we will refer to these offices, interchangeably, as “Labor Offices”, “Labor Agencies” or “Labor Unities”, or as “Delegacias”.

Given the country’s vastness, monitoring is first decentralized at the state level by State Labor Offices, popularly known as *Delegacias do Trabalho*³, and then at the municipal level by Local Labor Offices, known as *Subdelegacias do Trabalho*. While the delegacias are always located in state capitals, the subdelegacias are spread out across municipalities. The catchment area of the subdelegacias is usually larger than one municipality, and the number of these offices within a state is generally determined by the size and economic importance of each area.

The local provision of enforcement occurs through in locus inspections of selected establishments. Auditors assigned to each local labor office select firms to be inspected based on anonymous reports of labor violations, and they travel by car to inspect these sites (Almeida and Carneiro, 2012; Ponczek and Ulyssea, 2021).⁴ This implies that cities that are further away from the nearest enforcement office are less likely to be visited by a labor inspector, which is what we explore as our main measure of enforcement. The dimension we exploit is enforcement capacity across local economies, which is defined as the municipality receiving a local labor office.

The importance of these subdelegacias in promoting compliance with labor laws is well documented in the literature. For example, previous studies show that the presence of local labor offices tends to reduce informal labor (Almeida and Carneiro, 2012; Ponczek and Ulyssea, 2021). Although the primary concern of these offices need not be fighting forced labor, the media also illustrates anecdotal evidence in this direction. For instance, in 2014, thirty-seven workers in situations analogous to slave labor were rescued in São Paulo while working for a supplier of the major clothing chain Renner.⁵ More recently, in 2020, twenty workers were rescued in the state of Pernambuco after one of them died working on an irregular construction site.⁶

³Formally, the “Delegacias Regionais do Trabalho” became the “Superintendências Regionais do Trabalho e Emprego” following changes in the structure of the Ministry. As previous literature studying these offices still refer to them as Delegacias, we adopt this terminology for simplicity.

⁴Reports can be made by employees, unions, the public prosecutor’s office, or even the police. In practice, because the number of labor inspectors is small in comparison to the number of noncompliance complaints, these anonymous reports prompt the majority of inspections.

⁵G1 “Ministério confirma trabalho escravo em oficina que costurava para lojas” - November 27, 2014: <https://glo.bo/3FMwVkc>. Last accessed on March 28, 2022.

⁶G1 “Morte de operário em canteiro de obras provocou ação de resgate de 20 trabalhadores em situação análoga à escravidão” - September 14, 2021 <https://glo.bo/3p0NvHx>. Last accessed on March 28, 2022.

2.3 Access to labor justice

When we discuss forced labor in Brazil, the Executive Branch plays a more significant role in the implementation and inspection of public policies related to the subject, although there is considerable legislation aimed at combating work analogous to Slavery. As discussed in section 2.1, however, the issue of forced labor in Brazil is the object of legal norms that flow between the different branches of law, besides being supported by the Federal Constitution and, also, infra-constitutional legislation. In this way, when the Judiciary Branch receives demands from society, including those involving forced labor, it must seek the best possible solution within what the legislation authorizes.

In Labor Law, several principles are considered relevant to guide Brazilian labor relations, such as the principle of Protection, which consists of establishing legal equality between employee and employer, in virtue of the manifest economic superiority of the latter over that (Leite, 2021). Nevertheless, the physical presence of the labor courts in Brazil does not occur in all municipalities, so that in the last 20 years there has been an expansion in the number of existing labor courts. This event will be investigated in this study as a possible form of action by the state to combat forced labor, which will be our second measure of state presence, following the presence of Labor Offices.

Many papers have studied the consequences of judicial presence empirically, mainly trying to understand how this presence affects firms or the government itself. For instance, Ponticelli and Alencar (2016) studied how court enforcement could impact firm outcomes as finance, investment, and size in Brazil. Also looking at effects on firms, Pezone (2020) studied the impact of judicial enforcement on firm employment in Italy. Still investigating impacts of State presence but now on State outcomes, Litschig and Zamboni (2019) showed that the presence of the state judiciary in Brazil affects irregularities or corruption by the central government.

Different from previous studies, we investigate how the State presence via Labor Courts could deter the use of forced labor in Brazilian municipalities. Although previous papers have shown the impact of access to justice on labor market conditions (e.g., Espinosa, Desrieux and Ferracci, 2018), to the best of our knowledge, ours is the first paper to address the issue of forced labor. In line with anecdotal evidence,⁷ we expect to find a negative impact of judicial presence on forced labor because the probability of a firm being caught rises, and the firm would behave more appropriately, putting fewer workers in the situation of forced labor.

⁷Folha de São Paulo "Empresa contratada pela Aeronáutica é condenada por trabalho escravo em construção de hangar" <https://bit.ly/3K0JJZC>. Last accessed on March 28, 2022.

2.4 NGO's actions in preventing forced labor

There are some NGO's in Brazil aiming to help eradicate forced labor, such as the prominent Comissão Pastoral da Terra (CPT).⁸ As discussed earlier, we focus in analyzing the impact of the Program Escravo, nem pensar! ("Slavery, No Way!"), from the well-known NGO Repórter Brasil, due to its active nature in proposing and implementing preventive actions. This subsection provides an introduction to its origins and initiatives.

The Repórter Brasil NGO was founded in 2001 by journalists, social scientists, and educators to promote reflection and action on the violation of the fundamental rights of peoples and workers in Brazil. Their reports, journalistic investigations, research, and educational methodologies have been used by public sector leaders, industry, and civil society as instruments to fight contemporary slavery. Coordinated by the Repórter Brasil in partnership with the ILO-Brazil and more than 30 institutions, the program Escravo, nem pensar! is the first third-sector nationwide program to prevent slave labor. The program's objective is to reduce the number of workers recruited into slave labor by raising awareness about this violence⁹

The program has as its objective the training of community leaders, educators, and social workers on how to detect and prevent contemporary slave labor. The training participants are expected to disseminate the knowledge acquired to their students, families, and networks, spreading information about this severe violation of human rights. The NGO's actions can be summarized in four areas: (i) training of community leaders, professionals in the public education system, social assistance, and health networks; (ii) production of specialized content on the subject of slave labor and related matters; (iii) development of educational methodologies for human rights education; and (iv) political influence through participation in forums for the elaboration of public policies to fight slave labor, and the articulation between public authorities and civil society entities.

The topics addressed in the training cover the structural causes of slave labor and the consequences of this practice, putting into perspective its social, political, economic, and environmental dimensions. In addition to the problem of slave labor itself, the following subjects are usually discussed: work in our society, agrarian issues, and movements in the struggle for the rights to land; environment; migration and human trafficking; child labor and sexual exploitation of children and adolescents. To better understand the topics during the training, the courses receive guests from civil society and public authorities that work in the fight against slave labor.

⁸Comissão Pastoral da Terra - <https://www.cptnacional.org.br>. Last accessed on March 28, 2022.

⁹The ILO recognized this NGO as a successful case study in the fight against slave labor. https://www.ilo.org/wcmsp5/groups/public/---ed_emp/---emp_ent/documents/publication/wcms_762728.pdf. Last accessed on March 28, 2022

3 Data Sources and Descriptive Statistics

This section presents and describes the main datasets used in this paper. The following subsections provide context, sources, and descriptive statistics for the data on forced labor (subsection 3.1), on the enforcement of labor laws through the presence of Labor Offices (subsection 3.2), on the access to justice through the presence of Labor Courts (subsection 3.3), and on the *Escravo, nem pensar!* actions (subsection 3.4).

3.1 Forced labor in Brazil

As discussed in section 2, the main branch of the federal government responsible for inspections on violations of labor laws is the Ministry of Labor and Employment. Data on labor inspections since 1995 were obtained through the Law of Access to Information, directly from the Ministry. The data has detailed information on labor inspections, such as the date of the inspection; the municipality; its nature (i.e., if it is based on anonymous complaints or on intelligence work from the Ministry); the number of workers freed in each inspection; their respective occupations; and information on the firms that were investigated. This data is our primary measure of forced labor in Brazilian municipalities.

As an alternative measure of forced labor, to validate that from the Ministry of Labor and Employment, we use data from the Comissão Pastoral da Terra (CPT), a Brazilian NGO that documents and gathers data on forced labor and land conflicts over the national territory. The spatial distribution of both these measures is shown in Figure 1. CPT data is based on denounces and fieldwork information and, therefore, tends to slightly underestimate the total of freed workers, as can be seen in Figure 3, which shows the trends in both variables.

As shown in Figure 1, there is no evidence of systematic differences between CPT and Ministry of Labor data-sets. Most of the freed workers were found in the North and Midwest regions of Brazil, an expected pattern since, as shown in Figure 2, the main sector of activities of the companies where forced labor was found is agriculture, specifically livestock, and activities related to the extraction of timber. Finally, from the Ministry of Labor and Employment, we were also able to get data on individual characteristics of the freed workers, as discussed in section 2. For this reason, we use it to build a panel containing the quantity of freed workers by year in each municipality, which we use to construct the primary outcome in our analysis.

3.2 Enforcement of labor laws

Given the continental dimension of Brazil, the Ministry of Labor and Employment has decentralized units that are responsible for monitoring and enforcing labor laws. These units are divided into Regional Labor Superintendencies (*Superintendências Regionais do*

Trabalho e Emprego - SRTE), Regional Labor Offices (Gerências Regionais do Trabalho e Emprego - GRTE) and Regional Labor Agencies (Agências Regionais - AR). We refer to them generically as "Labor Offices" or "Delegacias do Trabalho", following previous literature (Almeida and Carneiro, 2012; Ponczek and Ulyssea, 2021).

The information on the current presence of these Labor Unities across Brazilian municipalities was obtained through the Law of Access to Information, directly from the Ministry of Labor and Employment. In order to be able to exploit the roll-out of these institutions in a panel data framework to be employed as described in section 4, we collect the starting date of each of these Unities in each municipality. This process is done by analyzing the contents of more than 280 documents in Portable Document Format (PDF) sent by the Federal Government in response to our request through the Law of Access to Information. We attempted several strategies to facilitate the data processing, one of them being the use of `tabula-py` to generate data frames.¹⁰ The files, however, follow different structures, and the full automation proved impossible. So, we hand-code part of the information, mainly from the oldest documents received.

Figure 4 shows Municipalities with at least one Labor Unity (i.e., SRTE, GRTE, and AR) in 2021. Currently, there are 416 Brazilian municipalities with at least one Labor Unit (marked in either blue or red in Figure 4), while 5,154 municipalities lack this dimension of state presence. Of the former, 307 of the municipalities served by Labor Units have at least one Regional Labor Agencies (AR), 89 have at least one Regional Labor Offices (GRTE), while 27 have a Regional Labor Superintendencies (SRTE). Nevertheless, to exploit the roll-out of these offices throughout Brazilian municipalities in our econometric framework, we had to exclude from the sample those cities that where: (a) already treated before 2000 or that (b) received and office that closed during the analyzed period. This left us with only 30 (marked in blue in Figure 4) treated and 4789 control municipalities.

3.3 Access to labor justice

To study the roll-out of new labor courts across Brazilian municipalities, we exploit data from the Superior Labor Court (Tribunal Superior do Trabalho, hereafter TST), provided by the Statistics and Research Coordination Department (Coordenação de Estatística e Pesquisa). This data covers the period between 2004 and 2019 and indicates municipalities that received new courts during this period. We complement this information by scraping and analyzing documents indicating the existence of labor courts in some municipalities before 2004.¹¹ This is important because it guarantees that our treatment group is composed of municipalities that received their first labor court while our control group is composed of municipalities that do not have one. With these documents transformed into a dataset,

¹⁰<https://pypi.org/project/tabula-py/>

¹¹Sources: http://www.planalto.gov.br/ccivil_03/leis/2003/L10.770.htm and http://www.planalto.gov.br/ccivil_03/leis/L8432.htm. Last accessed on March 28, 2022.

we identify 4,936 municipalities, of which 4,423 did not have a labor court while 513 had at least one labor court before 2004. Therefore, our analysis set comprises 4,423 municipalities, where 4,305 didn't receive a labor court during our analysis period (i.e., our control group), and 118 received direct access to labor justice (i.e., our treatment group).

Looking at the expansion of municipalities receiving labor courts, Figure 5 shows when the 118 municipalities received the labor courts through the period. It displays two main periods of expansion, the first one between 2004 and 2009 and the second one between 2011 and 2014. Those two moments are due to Brazilian legislation that legally authorized the creation of new labor courts.

Figure 6 shows the municipalities that received new labor courts between 2004 and 2019. Those municipalities are spatially distributed all over the country, with a presence in every Brazilian region. Indeed, of the 27 Brazilian states, 20 received at least one labor court in our period of analysis.

3.4 NGO actions in preventing forced labor

The main data-set on the *Escravo, nem pensar!* interventions was scrapped from the website of the program.¹² The data have a detailed description of the *Escravo, nem pensar!* actions, in which it is possible to know the dates and locations of each activity. It also includes details on the sort of intervention (for instance, workshops, training of community leaders, and education programs), and the targeted groups (e.g., workers, educators, public servants). Figure 7 shows the spatial distribution of the NGO interventions over the Brazilian territory.

As detailed in section 4, our identification strategy will exploit the staggered roll-out of the NGO interventions in Brazilian municipalities. To shed light on how the interventions occurred over time, in Figure 8 we present the number of municipalities that received at least one intervention by year. The number of beneficiary municipalities evolves smoothly over time, with a spike in 2018, due to the start of an NGO partnership with state governments.

4 Empirical Strategy

To perform the intended analysis, we use the data-sets described in section 3 to build a municipality-by-year panel data-set. Our final data, therefore, contains (a) a measure of the number of workers rescued divided by the number of rescue operations carried out by the MTE in each municipality-year and (b) an indicator of the year from which the

¹²<https://escravonempensar.org.br/nossas-acoefinalizadas/>. Last accessed on March 28, 2022.

municipality received one of the interventions (i.e., Labor Office *or* Labor courts *or* Escravo, nem pensar! actions).

In three separate exercises, to estimate the impact of the presence of the state or non-governmental organization on the incidence of forced labor, we exploit the variation in the timing of these interventions throughout Brazilian municipalities. Since the opening of new labor offices, new labor judicial courts, and new NGO operations in Brazilian municipalities occurred in a staggered fashion, our baseline specification is a two-way-fixed-effect (TWFE) model as described in equation 1:

$$(1) \quad Y_{it} = \beta INT_{it} + \gamma_i + \theta_t + \varepsilon_{it}$$

Where Y_{it} is our measure of forced labor in municipality i in year t - more specifically, Y_{it} is the natural logarithm of the number of workers found in slave-like labor divided by the number of rescue operations carried out by the MTE in municipality i in year t .^{13,14} γ_i and θ_t are, respectively, municipality and year fixed-effects, which control for idiosyncratic characteristics of municipalities that are fixed over time and for common time shocks in the outcome Y . INT_{it} is a dummy variable equal to one if the municipality i in year t received an intervention. Following Bertrand, Duflo and Mullainathan (2004), the error term ε_{it} is clustered at the municipality level. Under the assumptions that treated and non-treated municipalities had, before treatment, similar trends in forced labor, and that the treatment effect does not vary over time, β is the impact of the intervention on our outcome Y_{it} .

It is important to note that violations of these assumptions in a TWFE model could lead to biased estimates of β , as highlighted by the new Difference-in-Differences literature (de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021). We, therefore, pursue two different strategies to overcome this concern. First, following Goodman-Bacon (2021), to be able to formally test the parallel trends assumption and also test if the treatment effect varies over time, we will estimate event study-like regressions, as described in equation 2.

$$(2) \quad Y_{it} = \gamma_i + \theta_t + T_i \left[\sum_m^{-2} \beta_m I(t - t^*) + \sum_{k=0}^K \beta_k I(t - t^*) \right] + \varepsilon_{it}$$

¹³We divide the number of freed workers by the number of rescue operations to accommodate the fact that a decrease in former might be explained by a decrease in the intensity of investigations and operations, and not by the existence of forced labor per se.

¹⁴To deal with cases of zero workers found in a given municipality-year, we follow the literature and apply the logarithm on the transformation of the number of workers plus one (for examples, see Doleac and Sanders, 2015; Barreto, da Mota Silveira Neto and Carazza, 2021)

Where the third term in the equation corresponds to a set of interactions of a treatment indicator variable T_i , equal to one if municipality i received an intervention in any year, with relative yearly dummies. The relative yearly dummies are defined as the interval, in years, from the intervention date in each municipality ($t^* = eventtime$). The dummy for the year just before the year of the intervention is omitted, which normalizes estimates of β_m and β_k to zero in that event year. If there is no difference in trends between our treatment and control groups before treatment, we expect that all β_m not to be statistically different from zero. Finally, we can interpret β_k as the average treatment effect for each year relative to the intervention. If the treatment varies over time, we should expect all β_k to be different from each other.

New developments in the Difference-in-Differences (DiD) literature tell a cautionary tale when interpreting TWFE estimates, like the ones provided by equation 1, as causal (de Chaisemartin and D’Haultfœuille, 2020; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021). Goodman-Bacon (2021) shows that when treatment occurs in a staggered fashion, as is the case in our research, estimates from TWFE might be biased. This may happen because the two-way fixed effects models estimator is the weighted average of all possible canonical (two-group/two period) DiD estimators (i.e., early treated vs. never treated, later treated vs. never treated, early treated vs. later treated, and later treated vs. early treated) in the data. As treatment effects may put treated units in a different trend, treatment effect variation over time turns biased the estimate for comparisons between later treated units (as treated) vs. early treated units (as control). This leads to biased estimates for the general two-way fixed effects model.

To overcome the potential limitations of the TWFE model, our next strategy is to estimate both equations 1 and 2 using the doubly robust estimator proposed by Callaway and Sant’Anna (2021). This new estimator is robust to staggered treatments, treatment effect heterogeneity, and when the sample doesn’t include never-treated units. Therefore, it does not suffer from the drawbacks of the Difference-in-Difference with treatment timing variation highlighted by recent literature (de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021).

4.1 Increasing comparability through balancing

Based on our data, previous literature, and conversations with stakeholders from both the government and the NGO, we understand that interventions such as establishing Labor Offices, Labor Courts, and, especially, receiving a *Escravo, nem pensar!* action are not randomly targeted. In the case of the state presence measured by the labor offices and labor courts, they tend to occur in municipalities that are better off in terms of baseline socioeconomic outcomes. On the other hand, NGO interventions tend to focus on worse-off localities with a more significant number of baseline rescued workers, as we show later on.

To avoid comparing "incomparable" localities and to circumvent further omitted variable bias, researchers may exploit simple two-step strategies that can be used to estimate the average treatment effect even in the absence of balance between groups. The most popular body of balancing methods is the one known as propensity score weighting (Hirano, Imbens and Ridder, 2003). The propensity score methods, however, require researchers to manually evaluate and test if the estimated weights are suitable, and it often fails to jointly balance out all of the covariates at once (Diamond and Sekhon, 2013).

As an alternative to the propensity score, we exploit entropy balancing, a multivariate reweighting method described in Hainmueller (2012) and Hainmueller and Xu (2013), which allows the user to reweight a sample so the covariate distributions in the reweighted data satisfy a set of pre-specified sample conditions (e.g. mean, variance, and skewness). In practice, this means that we can find a group of untreated municipalities with the same average predetermined characteristics as the group of municipalities that received an intervention.

More specifically, as we will show in section 5, this method helps us find an untreated group of municipalities that is, on average, statistically indistinguishable from the treated ones in terms of baseline characteristics such as the number of workers freed before the intervention; illiteracy rate; per capita income; the proportion of agricultural workers (those more frequently trapped into forced labor); population; and share of the urban population. After running the maximum entropy reweighting scheme, the entropy balancing process asserts that the reweighted sample of control municipalities are statistically similar to the treated ones (for a perspective, visit tables 2, 3, and 4). Once validated, the weights generated by entropy balancing are then used to weight the regressions of equations 1 and 2.

5 Results

In this section, we present the results of the impact of increased access to labor justice and access to the *Escravo, nem pensar!* interventions on the prevalence of slave labor in Brazilian municipalities. As explained in section 3, results on the impact of Labor Offices are still pending due to the difficulty in transforming the information from unstructured PDFs into readable data. For this specific intervention, results will be provided in the third deliverable.

5.1 The effect of increased access to labor justice

As discussed in section 4, we weight all regressions with entropy weights as suggested by Hainmueller (2012). Because we examine different treatments in this study, the weights for each intervention analyzed will be different. Table 2 presents balance Panels comparing

differences in the selected covariates used to generate the entropy weights between treated and untreated municipalities, i.e., municipalities that received a labor court and municipalities that never received one.

As can be seen in Panel (B) of Table 2, the entropy weights successfully made the treatment and control groups more comparable. Without weights (i.e., Panel (A)), there are statistically significant differences between the two groups in almost all variables. When the entropy weights are used, the differences decrease and become statistically insignificant even at conventional levels for all variables. Having discussed the comparability between groups in terms of observable variables, the next step in the analysis is to estimate the causal effect of the new labor courts on our measure of the prevalence of slave labor.

Figure 9 shows the results of estimating the event study model described in equation 2, where the dependent variable is the natural logarithm of the number of workers found in slave-like labor divided by the number of rescue operations carried out by the MTE in municipality i in year t . The figure shows the coefficients of relative years to intervention dummies and the 90% (darker shaded region) and 95% confidence intervals (lighter shaded region). Panel (a) displays the estimates for the standard event study estimator while Panel (b) displays it following the method proposed in Callaway and Sant'Anna (Forthcoming).

The results in Figure 9 Panel (a) show that new labor courts in otherwise neglected Brazilian municipalities reduced the prevalence of slave labor compared to the control group. Combining the coefficients for the post-treatment period, we find that the presence of state labor courts reduces the prevalence of slave labor by 6.9% relative to the mean, significant at 5% level. The dynamics of the effect suggest that it occurs immediately after treatment staying flat after five years since the start of the new labor court. We also found no evidence of a violation of the parallel trends assumption, as the coefficients were not statistically different from zero at the usual significance levels before the intervention. Note, however, that in a staggered difference-in-differences design, TWFE estimates such as those shown Panel (a) may be biased even if the control group includes several never-treated units, as discussed in section 4.

Applying Callaway and Sant'Anna (2021) estimator, we obtain the results in Panel (b), which follow the same negative direction as those in the baseline specification. However, the effect appears lose statistical significance immediately after the court's establishment. Between the second and fifth years after the intervention, the estimates are not statistically different from zero, although the negative effect remains virtually the same in all periods. The grouped coefficient for the post-treatment period also indicates a lower treatment effect compared to the baseline specification (-0.049 versus -0.069).

5.2 The effect of Delegacias do Trabalho (Labor Offices)

The results of the interventions by the Labor Offices follow a similar structure to that presented in the previous section and the methodology reported in section 4. Table 3 displays balance panels comparing differences in the chosen covariates used to produce the entropy weights between treated and untreated municipalities, i.e., municipalities that received a labor office and municipalities that did not. The entropy weights successfully increased the similarity between the treatment and control groups, as demonstrated in Panel (B). Without weights (i.e., Panel (A)), practically all variables show statistically significant differences between the two groups.

Considering that the municipalities are now comparable, the next step is to analyze the effect of opening a Labor Office on the levels of forced labor found in the municipalities. Results for this intervention are shown in Figure 10. Again, Panel (A) displays the estimates obtained through the TWFE model while Panel (B) shows estimates corrected using the Callaway and Sant'Anna (2021) method. In both models, the dependent variable is the natural logarithm of the number of workers found in slave-like labor divided by the number of rescue operations carried out by the MTE in municipality i in year t .

Surprisingly, none of the specifications render statistically significant results of the impact of receiving a Labor Office. In the TWFE model from Panel (A), we find that the presence of Labor Offices increases the prevalence of detected forced labor by 4.8% relative to the mean, but the higher variance (s.e. = 0.046) does not allow us to reject the null hypothesis. Since the TWFE estimates may be biased in a staggered difference-in-differences design, we also exploit the approach proposed by Callaway and Sant'Anna (2021) and do not find statistically significant results as well. In the latter setting, the aggregated effect for the whole period increases to 11.2%, but also does the variance (s.e., 0.078). Although the magnitudes are in line with what one would expect (i.e., more supervisory state presence should lead to more detection), nothing can be said about the impact of that policy itself.

We highlight that the analysis of the effect of the opening of the Labor Office on the levels of forced labor was hampered, possibly due to the lack of statistical power. Several treated municipalities already had an office before 1995, the first year of our sample, or had to be removed because of attrition. In 2021, 416 Brazilian municipalities had at least one Labor Office. However, when we analyzed the opening date through the PDFs sent by the federal government, 386 municipalities either already had a labor office before the beginning of the database or had an office closed during the period. Thus, by reducing the treated units to only 30, it was impossible to infer the effect of opening Labor Offices on the levels of forced labor.

5.3 The effect of *Escravo, nem pensar!* program

In the same spirit of the previous sections, Table 4 shows balance exercises between treated and untreated municipalities, comparing differences in the pre-treatment level of the selected covariates used to build the entropy weights. As can be seen in Panel (B), for the evaluation of the *Escravo, nem pensar!* actions, The entropy weights successfully increased the similarity between the treatment and control groups. Without the weights, there are statistically significant differences between the groups for almost all variables previous to the first NGO intervention (i.e., Panel (A)). On the other hand, after using the entropy weights, the differences reduce in magnitude and also become statistically insignificant at conventional levels.

The baseline event-study estimates on the effect of the NGO program on the prevalence of slave labor are presented in Figure 11. Once again, the dependent variable is the natural logarithm of the number of workers found in slave-like labor divided by the number of rescue operations carried out by the MTE in municipality i in year t . We see that *Escravo, nem pensar!*'s interventions reduced the prevalence of detected slave labor when comparing treated to non-treated municipalities.

The dynamics of the effect suggest that the impact increases with time. This pattern is consistent with a learning effect, given the fact that most NGO interventions are focused on training educators and public servants, and considering that almost 25% of the actions take more than a year to be completed in field. Translating these findings to an overall impact for the post-intervention period, we discover that the program lowers the prevalence of slave labor by 5.9% relative to the mean. Also, we found no evidence of violation of the parallel trends assumption, since all pre-intervention coefficients are not statistically different from zero at conventional significance levels.

Using Callaway and Sant'Anna (2021) new difference-in-difference estimator (Panel (b)), we find qualitatively similar results, reinforcing the robustness of the main results. The grouped coefficient for the post-treatment period indicates a higher treatment effect relative to the baseline specification (26.8%), indicating that the bias on TWFE attenuates the estimate of the treatment effect. Finally, pre-treatment dummies in Figure 11 are not statistically different from zero, reducing concerns about violations of the parallel trends assumption.

The estimated impact of the NGO actions seems to be the most prominent of the three interventions analyzed in this paper. Our agnostic approach shows that, following "*Escravo, nem pensar!*" actions, treated municipalities display a lower level of detected forced labor when compared to untreated ones. The difference in the magnitude of this impact when compared to that of labor offices and courts is relatively high, and conversations with researchers from "*Escravo, nem pensar!*" lead us to understand that the Program may be coupled with other interventions and further coordination with the

local public authorities (municipal and state governments). If this is the case, it will render our estimates to refer to compound treatment effects. That said, we advert readers that these results are not to be taken by their face value but by their direction and consistency (i.e., short and long-term decrease in detected forced labor).

6 Suggestive labor market consequences

Although the previous section provides compelling evidence on the impact of each intervention on the number of rescued workers, one question still needs an answer: do these interventions exert any impact on the formal labor market? Two non-exhaustive hypotheses are: (*Hypothesis 1*) The number of detected enslaved workers might be affected not because the prevalence of forced labor is changing, but because after the intervention, firms start hiding better their workers kept in irregular conditions. Or it could be that (*Hypothesis 2*) changes in the number of detected workers in slave-like conditions is caused by the formalization of work relations and better compliance with labor laws after the intervention.

In trying to provide suggestive evidence in favor of either of these hypotheses, we exploit the *Relação Anual de Informações Sociais* dataset (RAIS, hereafter). RAIS is a linked employer-employee dataset covering the universe of formal workers and firms in Brazil (approximately 65 million per year). It is made available also by the MTE and one of its most important features is that its completion and submission are mandatory for all firms. It contains the firm identifier (CNPJ or Cadastro Nacional da Pessoa Jurídica); location; employer's industry sector (hereafter, CNAE - Classificação Nacional de Atividades Econômicas); and the occupation held by each worker in each contract (hereafter, CBO - Classificação Brasileira de Ocupação).¹⁵

In an effort to investigate whether these interventions increase the number of workers formally contracted in the sectors and occupations most intensive in forced labor, we first map the industries and professions in which the rescued workers were found. Then we resort to RAIS and count the number of jobs in each municipality-year for which the worker is formally employed to perform tasks in these sectors and occupations. With these data at hand, we obtain results estimating equation 2.

Regarding the labor market consequences of Labor Courts, figure 12 shows an upward trend in formalization in the sectors of activity (Panel(a)) and occupations (Panel (b)) of previously rescued workers. However, neither individual coefficients for each year or the aggregate effects for the whole period are statistically significant. Figure 13 replicates the same exercise for the consequences of opening Labor Offices. Now we find a positive and

¹⁵The CBO describes and orders occupations within a hierarchical structure that allows the aggregation of information regarding the workforce. It takes as a reference the latest version of the International Statistical Classification of Occupations - ISCO-88.

significant aggregate impact over the whole period: an increase in formalization by 13.8% in the sectors that are more intensive in forced labor and by 16% in the occupations in which forced labor is more prevalent. These results should be taken with caution, as both panels display some level of pre-trends or anticipatory effects. At last, Figure 14 displays the labor market consequences of the "Escravo, nem pensar!" actions. There is a prominent effect in the sectors of activity of previously rescued workers. This effect represents a decrease by 5.6%, although we are not able to explain why this happens.

7 Concluding Remarks

This paper assesses the impact of three interventions on the prevalence of forced labor in Brazilian municipalities. First, we ask whether specialized state presence, in the form of Labor Offices and Labor Judicial Courts, diminishes the use of forced labor locally. Second, we ask whether the intervention of a well-known NGO is effective in curbing the use of slave-like labor.

Exploiting the roll-out of these interventions, we find that labor courts reduce the number of workers found in slave-like labor, on average, by 6.9% in a TWFE specification. Results are not robust when we account for potential bias through estimation via the Callaway and Sant'Anna (2021) method. Moreover, we do not find any effect in terms of the detection of forced labor for the opening of Delegacias do Trabalho, probably due to the small sample size/reduced statistical power. The interventions of the NGO, on the other hand, reduce this same outcome by 6-26% depending on the specification, although we have reason to believe that other forces are acting concurrently with the "Escravo, nem pensar!" (e.g., municipal and state governments).

Additionally, we offer suggestive evidence that the state presence in question promotes the formalization of people working in industries where slave-like labor is more prevalent. The creation of Labor Offices increases the number of formal employees in these industries by 5.6 percent, but we find no effect for the opening of Labor Courts. While these results should be interpreted with caution, the NGO interventions appear to reduce formalization in these industries by 13.8 percent.

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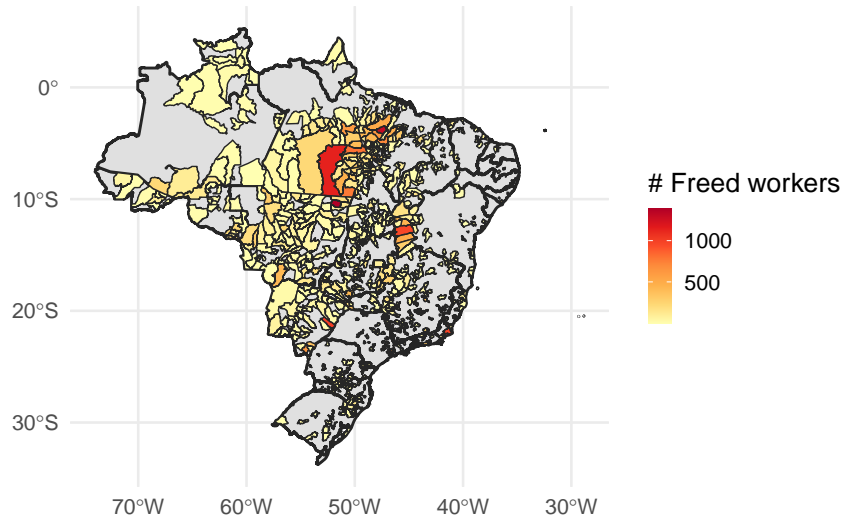
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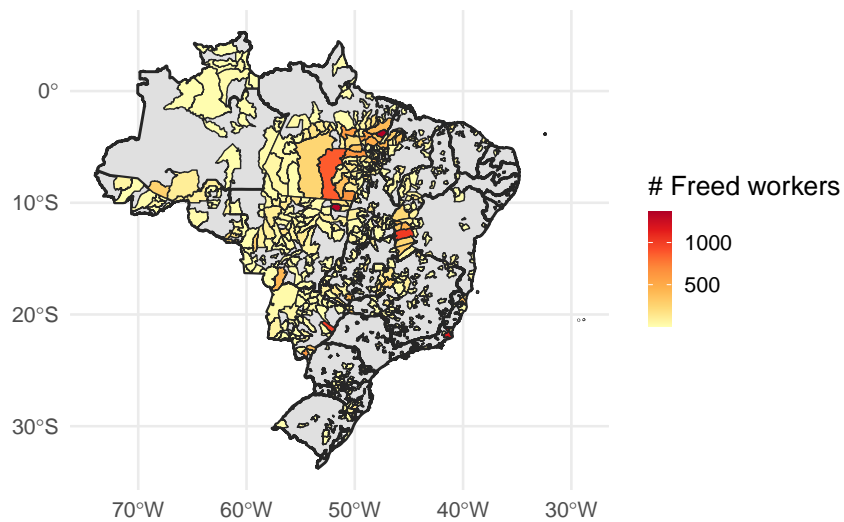
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Figure 1: Spatial distribution of freed workers



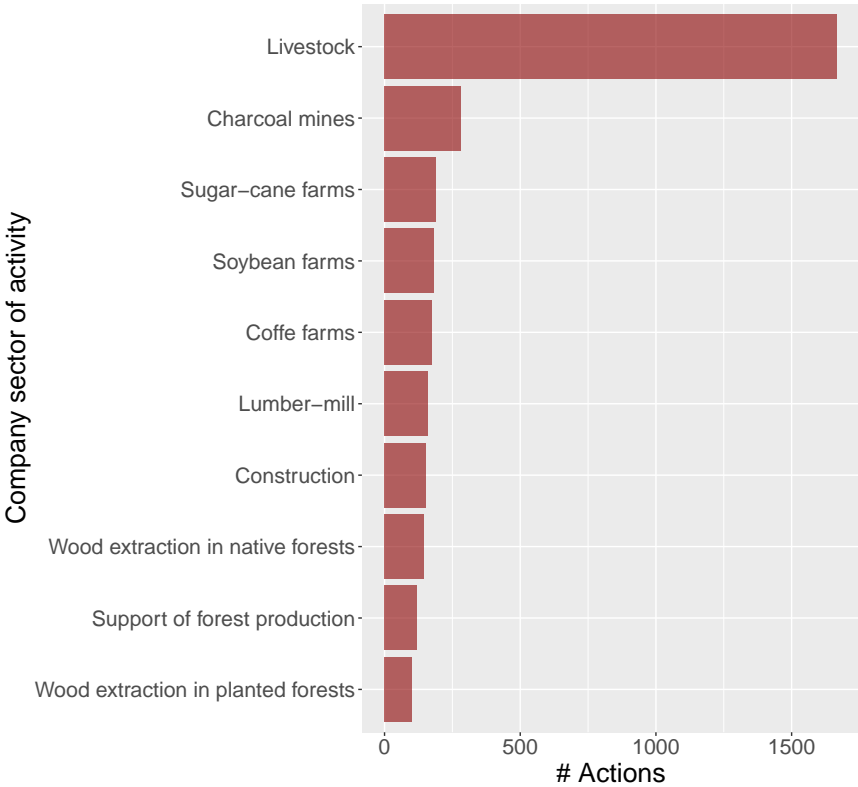
(a) Ministry of Labor and Employment



(b) Comissão Pastoral da Terra

Notes: This figure plots the spatial distribution of freed workers in Brazil from 1995 to 2019.

Figure 2: Top-10 sectors targeted by the Ministry of Labor and Employment



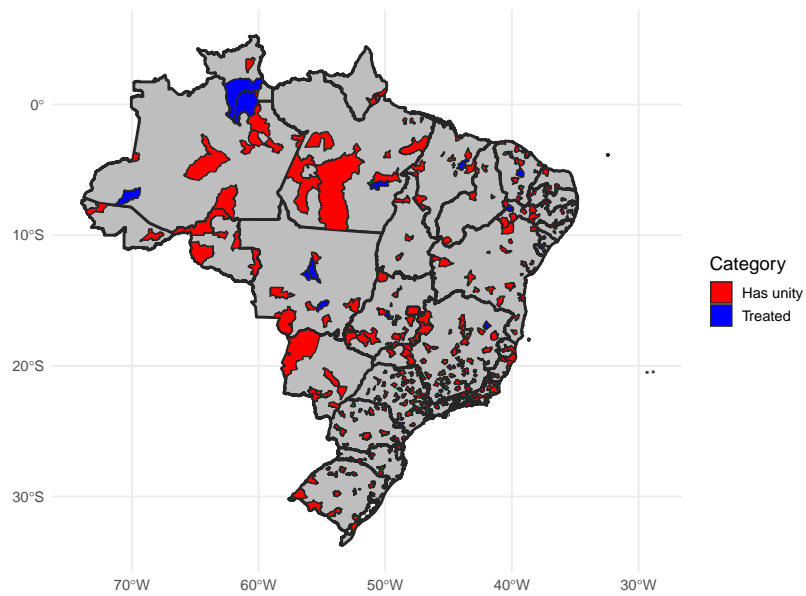
Notes: This figure plots the top-10 sectors that were more frequently targeted by operations of the Ministry of Labor and Employment between 1995 to 2019.

Figure 3: Number of freed workers in Brazil by year and data source



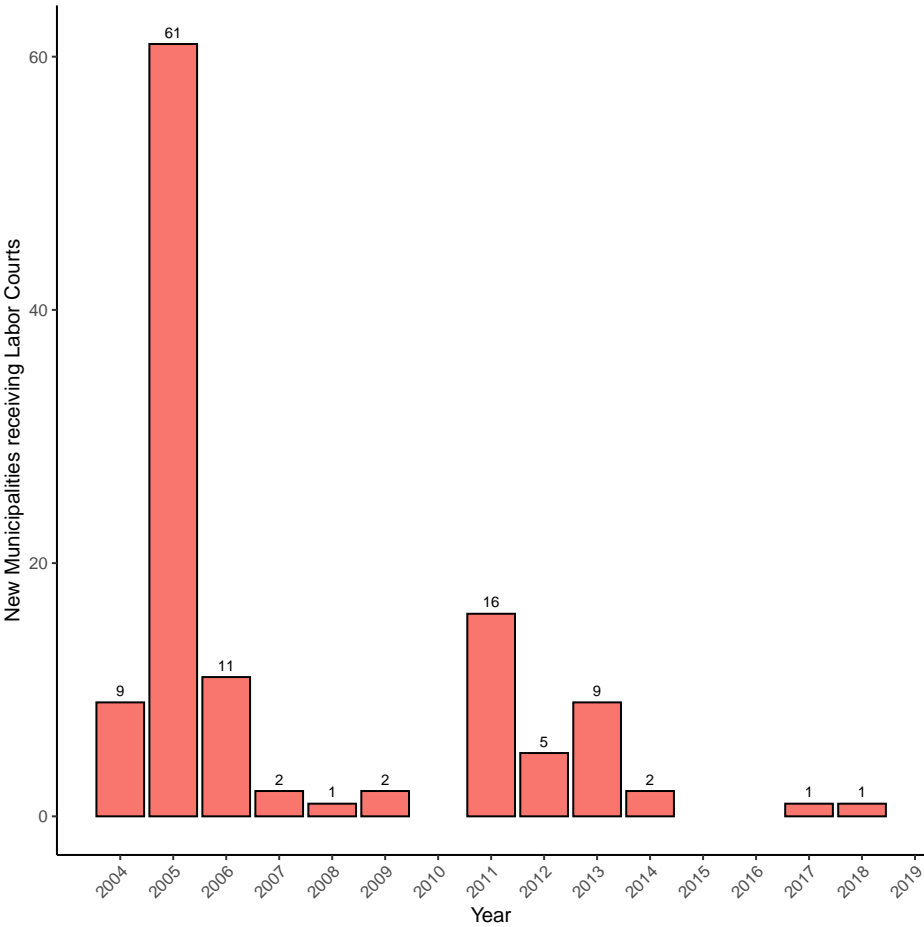
Notes: This figure plots the trends in the number freed workers in Brazil between 1995 to 2019 for both data sources on freed workers used in this paper.

Figure 4: Municipalities with at least one Labor Unity - SRTE, GRTE, and AR



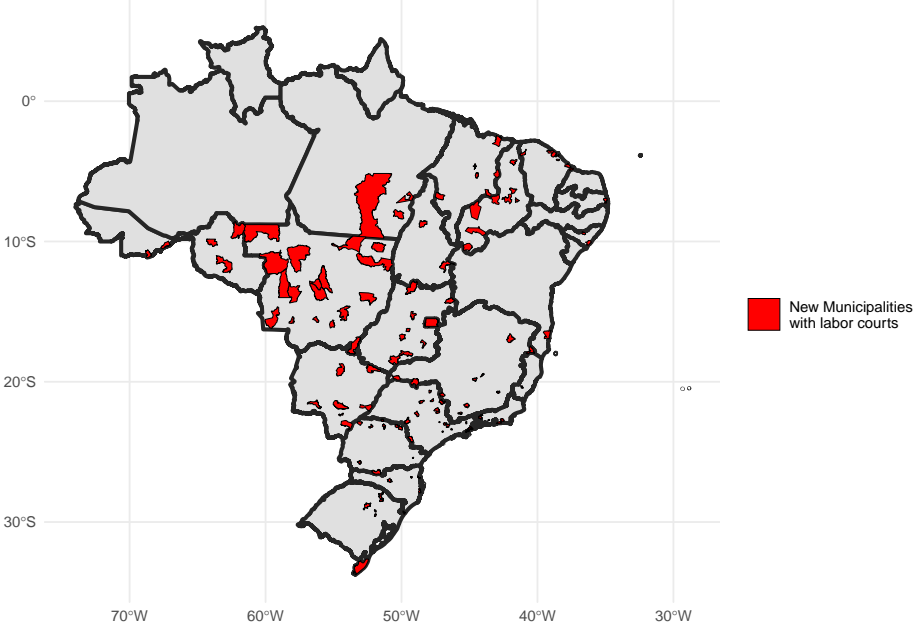
Notes: This figure plots the spatial distribution of Labor Unities for the whole country. The municipalities that have at least one Office are highlighted in red.

Figure 5: Number of municipalities that received Labor Courts during the period analyzed



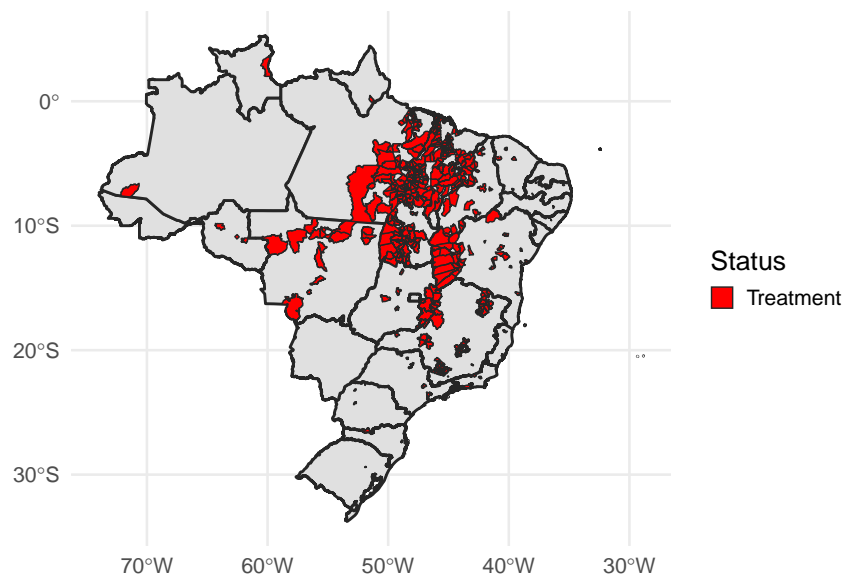
Notes: This figure presents the trends in the number of municipalities that received Labor Courts from 2004 to 2019.

Figure 6: Spatial distribution of municipalities that received Labor Courts during the period analyzed



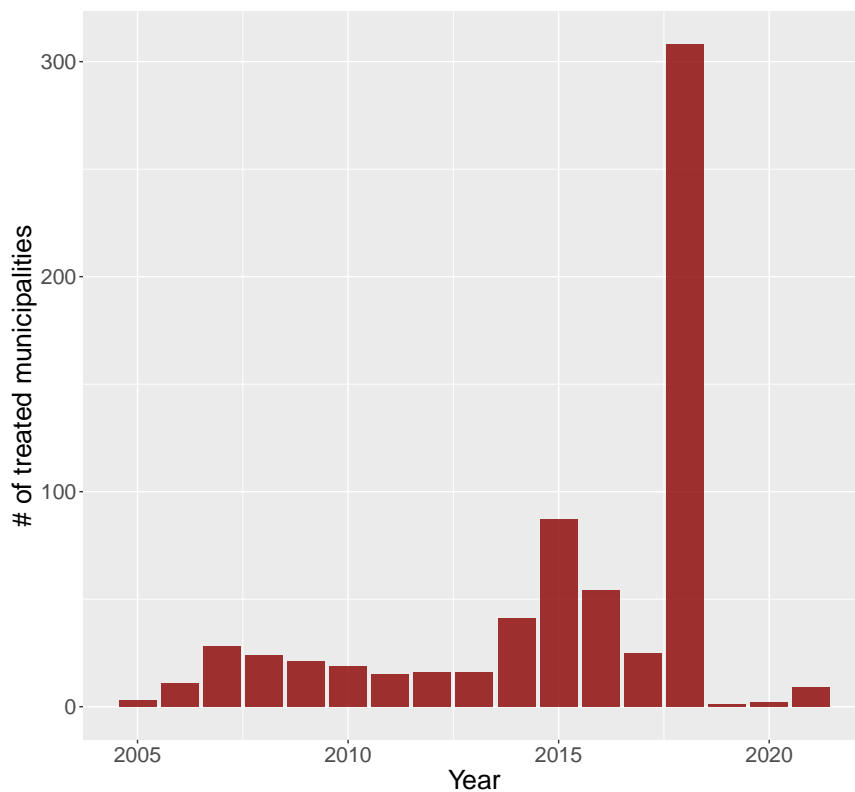
Notes: This figure presents the spatial distribution of Labor Courts, highlighting municipalities that received at least one new court from 2004 to 2019.

Figure 7: Municipalities that received at least one *Escravo, nem pensar!* intervention



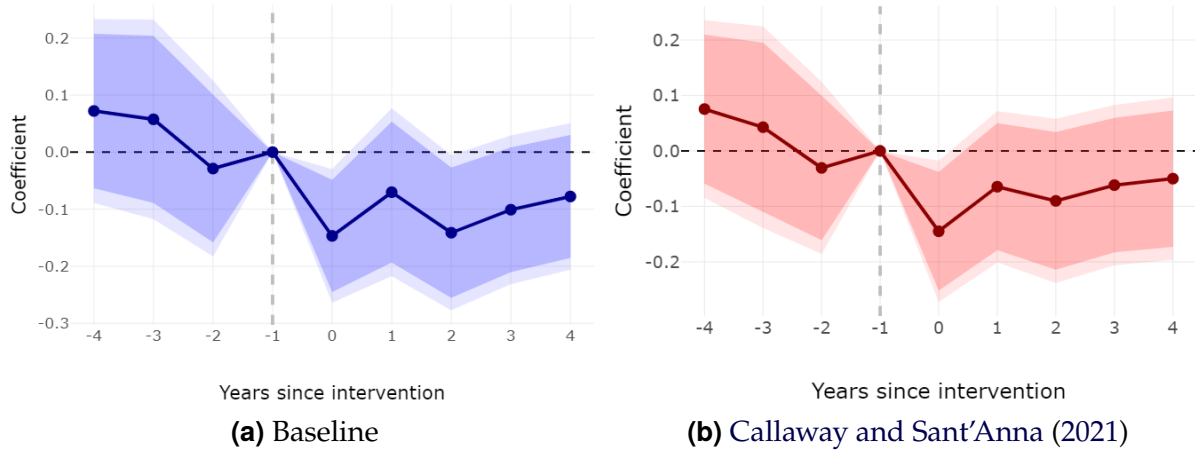
Notes: This figure plots the spatial distribution of the *Escravo, nem pensar!* interventions for the whole country between 2005 to 2020. The municipalities that were benefited with at least one action are highlighted in red.

Figure 8: Number of municipalities that received at least one Escravo, nem pensar! intervention by year



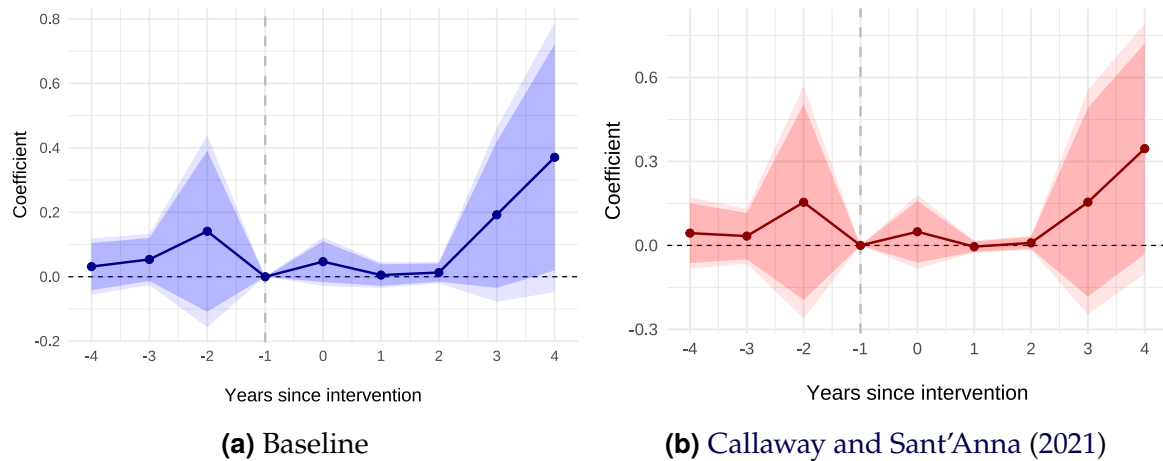
Notes: This figure plots the number of municipalities that were benefited with at least one Escravo, nem pensar! action by year, from 2005 to 2020.

Figure 9: The effect of opening Labor Courts on detected of slave labor



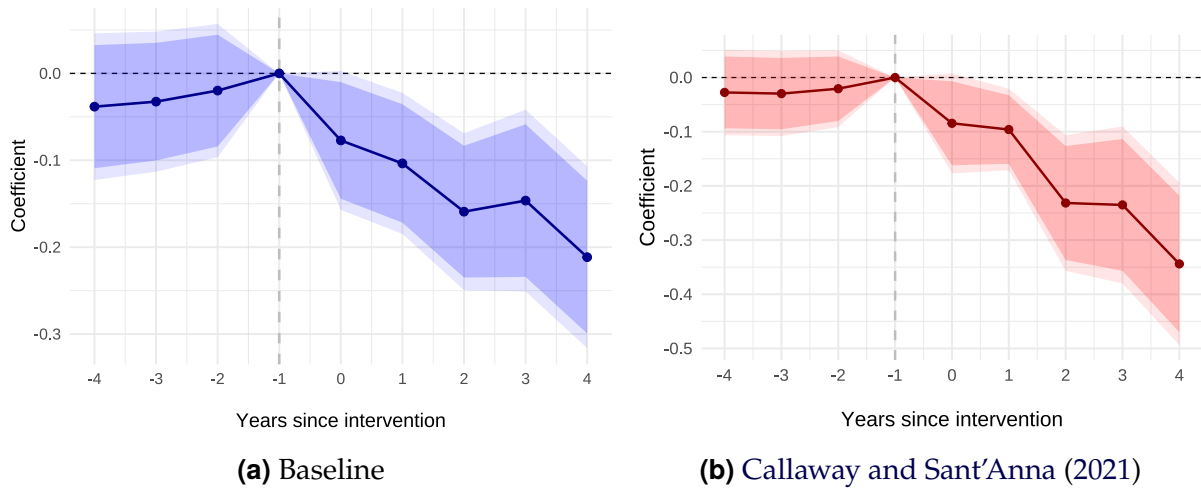
Notes: The figure plots the coefficients, 95% and 90% confidence intervals of the interaction term of the treatment indicator and relative yearly dummies from the regression specified 2 and also using the estimator proposed by Callaway and Sant'Anna (2021) for staggered difference-in-differences designs. Relative yearly dummies are defined as the interval, in years, from the start of the first intervention. The regression is weighted using entropy weights proposed by Hainmueller (2012). The dependent variable is the log of the number of freed workers plus one. Standard errors are clustered at the municipality level. The grouped post-treatment coefficient for panel (a) equals -0.069^{**} and the standard error is 0.028. The grouped post-treatment coefficient for panel (b) is -0.049 , and the standard error is 0.066.

Figure 10: The effect of opening Delegacias do Trabalho on detected of slave labor



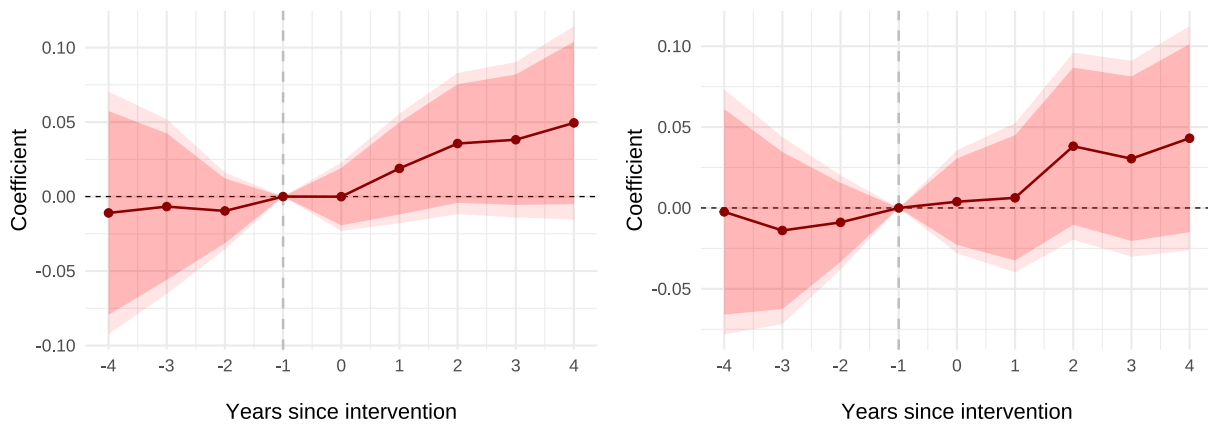
Notes: The figure plots the coefficients, 95% and 90% confidence intervals of the interaction term of the treatment indicator and relative yearly dummies from the regression specified 2 and also using the estimator proposed by Callaway and Sant'Anna (2021) for staggered difference-in-differences designs. Relative yearly dummies are defined as the interval, in years, from the start of the first intervention. The regression is weighted using entropy weights proposed by Hainmueller (2012). The dependent variable is the log of the number of freed workers plus one. Standard errors are clustered at the municipality level. The grouped post-treatment coefficient for panel (a) equals 0.048 and the standard error is 0.046. The grouped post-treatment coefficient for panel (b) is 0.112, and the standard error is 0.078.

Figure 11: The effect of "Escravo, nem pensar!" on detected of slave labor



Notes: The figure plots the coefficients, 95% and 90% confidence intervals of the interaction term of the treatment indicator and relative yearly dummies from the regression specified 2 and also using the estimator proposed by Callaway and Sant'Anna (2021) for staggered difference-in-differences designs. Relative yearly dummies are defined as the interval, in years, from the start of the first intervention. The regression is weighted using entropy weights proposed by Hainmueller (2012). The dependent variable is the log of the number of freed workers plus one. Standard errors are clustered at the municipality level. The grouped post-treatment coefficient for panel (a) equals -0.059^{**} and the standard error is 0.026. The grouped post-treatment coefficient for panel (b) is -0.268^{***} , and the standard error is 0.063.

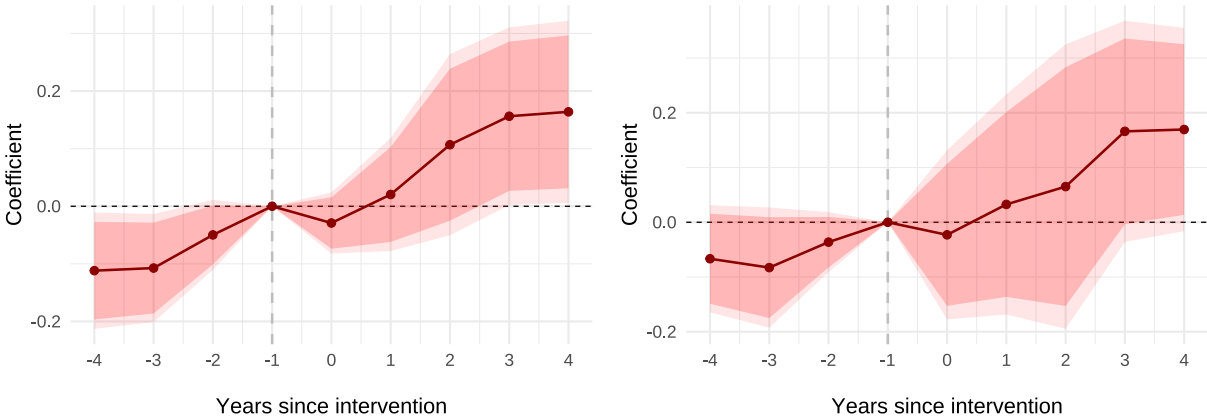
Figure 12: The effect of Labor Courts on labor formalization – key sectors and occupations



(a) Sector of rescued workers - using CNAE **(b)** Occupation of rescued workers- using CBO

Notes: The figure plots the coefficients, 95% and 90% confidence intervals of the interaction term of the treatment indicator and relative yearly dummies from the regression specified in Equation 2 using the estimator proposed by Callaway and Sant’Anna (2021) for staggered difference-in-differences designs. Relative yearly dummies are defined as the interval, in years, from the start of the first intervention. The regression is weighted using entropy weights proposed by Hainmueller (2012). The dependent variables are the log of one plus the number of formal workers in each sector. Standard errors are clustered at the municipality level. The grouped post-treatment coefficient for panel (a) equals 0.034, and the standard error is 0.031. The grouped post-treatment coefficient for panel (b) equals 0.028, and the standard error is 0.034.

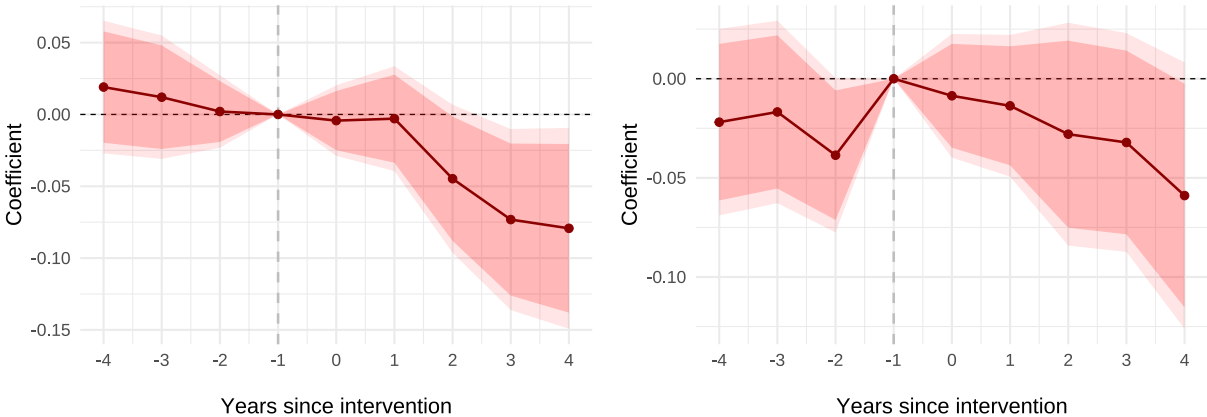
Figure 13: The effect of Delegacias do trabalho on labor formalization – key sectors and occupations



(a) Sector of rescued workers - using CNAE **(b)** Occupation of rescued workers- using CBO

Notes: The figure plots the coefficients, 95% and 90% confidence intervals of the interaction term of the treatment indicator and relative yearly dummies from the regression specified in Equation 2 using the estimator proposed by Callaway and Sant’Anna (2021) for staggered difference-in-differences designs. Relative yearly dummies are defined as the interval, in years, from the start of the first intervention. The regression is weighted using entropy weights proposed by Hainmueller (2012). The dependent variables are the log of one plus the number of formal workers in each sector. Standard errors are clustered at the municipality level. The grouped post-treatment coefficient for panel (a) equals -0.056^{**} , and the standard error is 0.026. The grouped post-treatment coefficient for panel (b) equals -0.035 , and the standard error is 0.022.

Figure 14: The effect of "Escravo, nem pensar!" on labor formalization – key sectors and occupations



(a) Sector of rescued workers - using CNAE **(b)** Occupation of rescued workers- using CBO

Notes: The figure plots the coefficients, 95% and 90% confidence intervals of the interaction term of the treatment indicator and relative yearly dummies from the regression specified in Equation 2 using the estimator proposed by Callaway and Sant’Anna (2021) for staggered difference-in-differences designs. Relative yearly dummies are defined as the interval, in years, from the start of the first intervention. The regression is weighted using entropy weights proposed by Hainmueller (2012). The dependent variables are the log of one plus the number of formal workers in each sector. Standard errors are clustered at the municipality level. The grouped post-treatment coefficient for panel (a) equals 0.138*, and the standard error is 0.073. The grouped post-treatment coefficient for panel (b) equals 0.161*, and the standard error is 0.097.

Table 1: Demographic characteristics of freed workers

Panel A - Characteristics	Obs.	Mean
Age	34,941	32
Black	11,886	0.47
White	11,886	0.25
Indigenous	11,886	0.04
Asian	11,886	0.25
Men	35,084	0.95
Illiterate	34,214	0.32
Elementary school dropout	34,214	0.40
At least elementary school completed	34,214	0.10
Panel B - Occupation	Freq.	Percent
Cook	196	0.56
Coffee farm workers	238	0.68
Chainsaw operator	400	1.14
Charcoal miner	446	1.27
Farm worker	642	1.83
Sugar-cane farm workers	757	2.16
Livestock workers	798	2.27
Construction worker	1,765	5.03
General farm worker	26,465	75.43

Notes: This table presents summary statistics for freed workers' socioeconomic and demographic characteristics from 1995 to 2019. Panel A shows demographic variables, where each variable corresponds to a dummy that equals one if the individual has this characteristic. Panel B presents the frequency of freed workers by sector of activity.

Table 2: Balance table with and without entropy weights (Hainmueller, 2012) - Labor Courts interventions

Pre-treatment characteristics	Panel (A) - no weights			Panel (B) - Entropy weights		
	Control (1)	Treatment (2)	Difference (3)	Control (4)	Treatment (5)	Difference (6)
Freed workers before 2003	1.529 [0.276]	13.160 [5.794]	-11.631***	13.160 [1.079]	13.160 [5.794]	-0.000
Illiteracy rates (18 to 24 years old)	10.120 [0.130]	5.920 [0.496]	4.199***	5.920 [0.083]	5.920 [0.496]	0.000
Prop. income top 10%	43.058 [0.103]	46.532 [0.598]	-3.474***	46.532 [0.120]	46.532 [0.598]	-0.000
Income per capita	314.096 [2.435]	478.722 [16.537]	-164.626***	478.719 [3.457]	478.722 [16.537]	-0.003
Prop. agricultural workers	45.800 [0.267]	23.120 [1.339]	22.679***	23.121 [0.236]	23.120 [1.339]	0.000
Prop. workers with elementary school	26.209 [0.155]	37.433 [0.916]	-11.224***	37.432 [0.169]	37.433 [0.916]	-0.000
Access piped water	64.730 [0.421]	76.791 [2.199]	-12.061***	76.790 [0.370]	76.791 [2.199]	-0.000
Population	13773.034 [448.751]	48246.420 [3860.626]	-3.45e+04***	48245.952 [2465.844]	48246.420 [3860.626]	-0.468
Urban population share	0.551 [0.003]	0.803 [0.014]	-0.252***	0.803 [0.002]	0.803 [0.014]	-0.000
Observations	4933	119		4933	119	

Notes: This Table displays results from difference-in-means tests between treatment and control municipalities (see columns (3) of Panels (A) and (B)). Panel (A) presents the results for the raw sample – i.e., before applying the entropy balance. Panel (B) shows results adjusting the sample through the use of entropy weights (Hainmueller, 2012). ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3: Balance table with and without entropy weights (Hainmueller, 2012) - Delegacias do trabalho

Pre-treatment characteristics	Panel (A) - no weights			Panel (B) - Entropy weights		
	Control (1)	Treatment (2)	Difference (3)	Control (4)	Treatment (5)	Difference (6)
Freed workers before 2003	0.385 [0.110]	7.333 [7.333]	-6.948***	7.329 [0.523]	7.333 [7.333]	-0.004
Illiteracy rates (18 to 24 years old)	10.271 [0.132]	11.076 [1.556]	-0.805	11.076 [0.143]	11.076 [1.556]	0.001
Prop. income top 10%	42.997 [0.106]	48.092 [1.090]	-5.094***	48.090 [0.131]	48.092 [1.090]	-0.002
Income per capita	309.083 [2.421]	384.092 [41.021]	-75.009**	384.007 [3.388]	384.092 [41.021]	-0.085
Prop. agricultural workers	46.479 [0.265]	28.500 [2.572]	17.979***	28.522 [0.274]	28.500 [2.572]	0.022
Prop. workers with elementary school	25.767 [0.153]	32.685 [1.585]	-6.918***	32.676 [0.194]	32.685 [1.585]	-0.009
Access piped water	64.046 [0.427]	65.589 [4.521]	-1.542	65.578 [0.416]	65.589 [4.521]	-0.010
Population	12645.560 [229.928]	42124.933 [5281.000]	-2.95e+04***	42093.514 [1128.381]	42124.933 [5281.000]	-31.419
Urban population share	0.546 [0.003]	0.717 [0.031]	-0.171***	0.716 [0.003]	0.717 [0.031]	-0.000
Observations	4789	30		4789	30	

Notes: This Table displays results from difference-in-means tests between treatment and control municipalities (see columns (3) of Panels (A) and (B)). Panel (A) presents the results for the raw sample – i.e., before applying the entropy balance. Panel (B) shows results adjusting the sample through the use of entropy weights (Hainmueller, 2012). ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 4: Balance table with and without entropy weights (Hainmueller, 2012) - Escravo, nem pensar! interventions

Pre-treatment characteristics	Panel (A) - no weights			Panel (B) - Entropy weights		
	Control (1)	Treatment (2)	Difference (3)	Control (4)	Treatment (5)	Difference (6)
Freed workers before 2003	1.496 [0.247]	23.833 [4.712]	-22.337***	23.828 [1.628]	23.833 [4.712]	-0.004
Illiteracy rates (18 to 24 years old)	9.421 [0.126]	11.776 [0.385]	-2.355***	11.775 [0.114]	11.776 [0.385]	-0.001
Prop. income top 10%	43.128 [0.098]	46.023 [0.354]	-2.895***	46.022 [0.109]	46.023 [0.354]	-0.000
Income per capita	344.439 [2.691]	271.194 [8.441]	73.244***	271.234 [2.472]	271.194 [8.441]	0.040
Prop. agricultural workers	42.172 [0.286]	46.038 [0.942]	-3.866***	46.036 [0.280]	46.038 [0.942]	-0.002
Prop. workers with elementary school	28.522 [0.173]	24.684 [0.562]	3.838***	24.686 [0.171]	24.684 [0.562]	0.002
Access piped water	68.574 [0.397]	44.916 [1.383]	23.658***	44.923 [0.404]	44.916 [1.383]	0.007
Population	25970.412 [1125.241]	82384.915 [28031.841]	-5.64e+04***	82384.038 [4611.645]	82384.915 [28031.841]	-0.878
Urban population share	0.586 [0.003]	0.574 [0.011]	0.012	0.574 [0.003]	0.574 [0.011]	0.000
Observations	5117	448		5117	448	

Notes: This Table displays results from difference-in-means tests between treatment and control municipalities (see columns (3) of Panels (A) and (B)). Panel (A) presents the results for the raw sample – i.e., before applying the entropy balance. Panel (B) shows results adjusting the sample through the use of entropy weights (Hainmueller, 2012). ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.