

Differential Treatment of Politically Connected Labor Traffickers: Evidence from the Brazilian Dirty List of Slave Labor

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Abstract

Over 40 million people worldwide were victims of modern slavery in 2016, 369,000 in Brazil alone. As part of an ambitious anti-trafficking agenda, the Government of Brazil implemented the Dirty List of Slave Labor, a name-and-shame policy that imposes financial and reputational sanctions on traffickers. Despite its relevance, there have been no attempts to identify sources of bias in the listing criteria nor to evaluate the influence of rent-seeking on the List's outcomes. Using novel data on campaign finance of traffickers from 2004 to 2018, I document compelling evidence that politically connected perpetrators are not less likely to be listed unless they are large donors, but they benefit from early removal and stay in the List 5.5 fewer months than non-connected exploiters. Being linked to ruralist legislators intensifies these effects, long-term connections matter, and there are increasing marginal benefits to donations. Tests that consider variation in the List's visibility support the hypothesis that campaign finance is used as evasion tactics. Money-giving is significantly more generous and frequent in election cycles leading up to inspection and listing years, but it drops afterwards. Although the present results are not causal, they inform a comprehensive research agenda and policy design and evaluation.

Keywords: labor trafficker, political connection, Dirty List, human trafficking, Brazil, campaign finance, corruption.

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1 Introduction

Human trafficking is a pervasive human rights violation. Used as an umbrella term, it takes several forms, including forced labor, sexual exploitation, debt bondage, and domestic servitude¹. The International Labour Organization estimates that over 40 million people worldwide were victims of modern slavery on any day in 2016, 369,000 in Brazil alone, but mere 0.06% survivors were identified (International Labour Organization, 2017). Over the past twenty years, multilateral organizations, NGOs, and numerous countries around the world have come together to raise awareness of human trafficking, improve legal norms and procedures, collect and share high quality data, and design policies that protect and empower survivors (Brunner, 2018). However, the absence of a well-established framework to criminalize trafficking and the meager number of convictions imply that perpetrators enjoy a high degree of impunity (UNODC, 2018).

With a robust anti-trafficking suite of policies implemented in the aftermath of the “Zé Pereira case”, a landmark episode where a 17-year-old boy was shot after trying to escape exploitation in a farm, the Government of Brazil stood out after committing to a series of integrated steps to punish traffickers, compensate survivors, and implement preventive and monitoring measures (Costa, 2009). Specifically, in 2003 the executive branch introduced the Dirty List of Slave Labor (“DL”), an administrative action that aims to increase the costs of human trafficking through two mechanisms: credit restrictions and reputational damage. On the one hand, the suspension of loans from public banks hampers traffickers’ present and future investment possibilities, negatively affecting their production pipeline. On the other hand, the disclosure of their information on a government website brings about shaming effects that impact both their supply chains and consumer market. In this way, keeping a dirty employer on the List is just as important as listing them in

¹Throughout this text, the terms "human trafficking", "trafficking", "slave labor", and "forced labor" will be used interchangeably as per international definition standards.

the first place. Trafficker information must remain public for two years and removal is conditional on full payment of fines and worker compensation.

However, not all *de facto* traffickers are included in the DL. Rather, the decision is made on a case-by-case basis and only one name from the set of identified exploiters in any single inspection is chosen. The lack of standardized criteria to make such selection leaves room for bureaucrat – the agent determining who goes on the List – bias that compromises the policy’s effectiveness. This is partly explained by the nature of coercive markets. For example, most violations of human rights and labor laws happen in the agricultural and charcoal sectors, which are concentrated in remote areas deep into the Amazon rainforest and away from larger urban hubs where Prosecution Offices are located. Coupled with that, the increasing level of sophistication under which perpetrators operate, which involve outsourcing and subcontracting practices, makes it difficult to map complete networks of traffickers and weakens accountability mechanisms. As the number of third-party players multiply, inspectors and prosecutors must deal with diffuse supply chains that hinder their ability to blame all firms and bosses involved in an operation. Yet, anecdotal evidence indicates that these structural difficulties alone do not explain the excess variation in the outcomes of the policy, both prior to and after listing, suggesting that subjective bias at each decision node of the “DL process” may be at play (details of this process are described in Section 2.2).

Additionally, the Dirty List has received significant backlash from political groups in Brazil. Informal interviews with anti-trafficking activists and legislators reveal that there are political interests in protecting traffickers, especially those in economic sectors with historical ties to governing elites and those with significant participation in campaign finance. For instance, government contractors, such as multibillion-dollar companies in the construction industry, have been involved with alleged cases of exploitation in one of Brazil’s largest social housing programs, but their contracts have not been rescinded even after their inclusion in the Dirty List. Criticism towards the

List targets the “ambiguity” of conditions that characterize trafficking and “ill-intended” civil society organizations who advocate for survivors and partner with investigators, such as the Landless Workers’ Movement (*Movimento dos Sem Terra*) and the League of Poor Peasants (*Liga dos Camponeses Pobres*). Most of these political leaders belong to the *bancada ruralista* (“rural caucus”) of Congress, a group that is traditionally aligned with the interests of agricultural elites and that argues that anti-trafficking policies “execrate rural producers” and causes “rural terrorism” (Ferreira, 2012; Uribe, 2021).

Multiple attempts at enacting constitutional amendments that modify the definition of modern slavery and the penal code have taken place both in the Chamber of Deputies and the Senate. These legislative projects advance a less stringent definition of trafficking by requiring that labor inspectors identify restriction of movement and use of explicit coercive practices, which narrows the scope of the Dirty List. A significant setback in the history of the DL took place at the end of 2014, when a Supreme Court Justice suspended its publication on the Ministry of Labor’s website, with endorsement from the ruralist – or pro-trafficker – group. The DL was only issued again in 2017 after another Supreme Court decision reinstated it; in the meantime, prosecutors and inspectors generated “private” lists that, in theory, were made available to banks, but not to the wider public.

Despite the relevance of the DL and the plethora of controversies around it, no studies have quantitatively examined neither how political connections in Brazil lead to differential treatment of traffickers in the “DL process” nor how these connections matter in anti-trafficking policymaking more broadly. In this thesis, I provide the first set of evidence that illustrates how political ties may affect an anti-trafficking policy and document a series of key facts about exploiters and election funding that not only motivate the present analysis but supply insights for future research. I hypothesize that dirty campaign donations are an investment that alleviate the DL effects via higher costs of listing for bureaucrats. Politicians receiving electoral support from these traffickers,

especially ruralists, have an incentive to punish officials' decisions through threat of re-appointment, reduced budget, and veto power. Hence, bureaucrats may become more lenient towards connected perpetrators to insulate their own careers.

There are two moments in which such connections could prove useful: after an inspection and after listing. Traffickers are aware of the risks of inclusion in the DL after an inspection and may leverage their political ties to avoid the moral and economic consequences of the DL altogether, or they may seek a faster removal from the List to attenuate shaming effects and suspend credit restriction. In addition, the period when the DL was not public represents an exogenous variation in the exposure of traffickers that allows me to test the plausibility of connections as evasion tactics. Specifically, the announcement of DL suspension happened in December of 2014 and affected its next publication, which was going to take place in June of 2015. The six-month-gap allowed traffickers to adjust expectations of punishment and, if connections are indeed used as a dodging strategy, then they were not as useful when the List was not publicly available and its reputational impacts were severely compromised.

Exploiting public records of campaign finance from 2004 to 2018, inspection records produced by the Federal Labor Prosecution Office from 2010 to 2019, and Dirty List publications from 2003 to 2019, I construct a novel dataset that measures political ties based on balanced and unbalanced panels of contributions prior to inspection and prior to DL publication. The selection of five- and eight-year tracking windows were determined by the data and facilitated comparability of donation behavior. My data sources allow me to detect employers who practice trafficking, but never go on the List, and I am able to control for economic sector, rural versus urban area, and whether the trafficker is a firm or a boss while I add fixed effects for state of inspection, year of inspection, and year of DL inclusion. I also use a standard definition of ruralist politician and collect data from the Journal of the Chamber of Deputies as well as voting record from the Committee of Agriculture,

Livestock, Supply and Rural Development to identify pro-trafficker incumbents and account for politician heterogeneity.

At first glance, connected traffickers are more likely to be selected to go on the List. Comparing listed and unlisted employers, I calculate that the differential probability of being listed is substantial: making any donation in the five years prior to inspection almost doubles the likelihood of DL selection (significant at the 1% level), but giving money to a ruralist politician shrinks this effect by 78% albeit without statistical significance. In a similar vein, having a long-term connection is associated with an imprecise increase of 11% in the chances of winding up on the DL. This provides strong evidence that bad behaving firms tend to pay rents and is suggestive that being linked to a pro-trafficker may hedge dirty employers against the risk of being listed, but does not completely eliminate it.

Additional tests that account for how many or how much connection a trafficker has paint a different picture and hint at increasing marginal benefits to election funding, a trend that persists across different specifications. Being connected to one additional politician reduces likelihood of going on the DL by 0.5% (significant at 1%), but once this variable is discretized, the data shows that the odds of being listed of traffickers connected to more than 5 candidates is 72.5% lower (significant at 10%). The latter is a large effect that is intensified further, despite without significance, if any of the donation recipients is a ruralist, in which case the chances of being listed decreases 78.5%. When estimating the influence of cumulative donation in the 5 years leading up to an inspection with a quadratic model, a similar pattern arises with high degree of significance, even though the effect becomes only slightly negative even after accounting for links to pro-traffickers. I run a similar set of specifications with an unbalanced panel of donations in the 8 years prior to inspection, indicating whether any of the traffickers' contribution in that timeframe is above a given percentile, and find that only large donors located above the 90th percentile enjoy the benefits of rents.

Whereas my analyses of the likelihood of being listed conditional on an inspection delivered some mixed results, I obtain strong evidence that, once listed, connected traffickers face differential treatment. In estimations with unconditional balanced donation prior to year of listing where I use a broad donation dummy, making an electoral contribution to any politician is associated with a 35% raise in the average likelihood of being removed from the List before the two years dictated by law (17.8 pp increase significant at 1% level), and that connected traffickers leave the List 5.5 months before their non-connected counterparts (also significant at 1%). Again, these effects become larger if any of the recipients of electoral funding was a ruralist – 54% greater probability of early removal and 8.7 fewer months on the List –, but they lose significance. Remarkably, having at least one long-term connection more than doubles the odds of getting off the List in less than 2 years and reduces time on the List in 22 months, both significant at the 1% level. Accounting for the period when the List was not visible, I conclude that the majority of these effects come from public lists, a reassuring indication that connections serve as an strategy to evade reputational damage. When the donation window is conditioned on an inspection, these results are amplified, but the ruralist effect is less consistent and some estimates lose power due to significant sample reduction.

Finally, the data shows that the donation behavior of traffickers is not constant over time and substantial changes are observed around the time of inspection and listing. In such years, the frequency of contributions drops for listed and unlisted traffickers alike, suggesting that traffickers may be allocating donation capital to other activities, such as payment of levied fines, labor compensation, and lawyers to fight the inclusion in the List. Prior to inspection, there is a difference between the average donations of connected versus non-connected employers, but such gap disappears afterwards. Relative to the DL, however, the average contribution of traffickers inspected in an election year increases, suggesting fewer employers make larger contributions. This last finding conforms to my econometric estimations that few large donors escape the DL. Together, these

results insinuate that campaign finance may be an investment that reduces variable costs of the List (constrained credit) but is unable to hedge firms against the fixed costs of listing (shame and supposedly long-lasting memory of the List) unless donations are at the tail of the distribution.

My thesis relates to several strands of previous work. First, I contribute to the currently limited literature on the political determinants of anti-trafficking initiatives (Schönhöfer, 2017), state capacity to implement them (Aronowitz, 2019; Blanton et al., 2020), and the impacts of corruption on such policies (DiRienzo, 2018; Sacco Studnicka, 2010). Scholars point out that implementing anti-trafficking policies and complying with international protocols are costly, depend on changes to criminal and immigration laws, and require expensive survivor support. They contend that bureaucratic efficiency and fiscal capacity elevate the costs faced by traffickers while enabling the state to address push factors, such as low educational attainment, poverty, and fragile safety networks, but fail to demonstrate what can compromise efficiency in this context. Polakova (2018), Perez-Truglia and Troiano (2018), and Eggenberger (2018) consider the financial and reputational repercussions of shaming policies on firms and individuals and find that shaming lists decrease firm value, high visibility matters, but scaling a list's effectiveness rely both on its immediate repercussions and perceived legitimacy. Yet, to my knowledge, no studies have evaluated the influence of political connections on outcomes of anti-trafficking name-and-shame policies nor investigated the decision of blacklisting, an important question in the case of subjective listing criteria such as in the Brazilian example.

More broadly, the evidence I provide is associated with publications on coercive labor markets (Acemoglu and Wolitzky, 2011; Alston et al., 2008; Feigenbaum et al., 2020;) and adds to a rich body of research that has examined the value of political connections of firms. Authors argue that connected firms have larger market power (Faccio, 2010), preferential access to credit, bailouts, and government contracts (Boas et al., 2014; Boland and Godsell, 2021; Brown and Huang, 2017; Faccio

et al., 2006; Khwaja and Mian, 2005), and higher rate of survival (Akcigit et al., 2017); are sensitive to political shocks (Acemoglu et al., 2016; Faccio, 2006; Fisman, 2001); and can evade or delay enforcement and decrease penalties (Correia, 2014; Yu and Yu, 2011). They also demonstrate that long-term connections matter (Snyder, 1992; Correia 2014). Specifically, Claessens et al. (2008) showed that firms in Brazil experience positive stock returns and increased access to bank finance after a successful electoral outcome for the politician to which they are connected. Conversely, these companies have poorer accountability standards (Faccio, 2010), show no gains in productivity or innovation (Akcigit et al., 2017; Cingano and Pinotti, 2013), and these phenomena are more significant in, though not exclusive to, high corruption countries (Faccio, 2006). Although most of these papers employ direct measures of political connection – ownership relations, connection through top employees (CEO’s or board members), electoral contributions, family ties, or lobbying –, recent works, including Bussolo et al. (2018), apply network methods to investigate the effect of direct and indirect political connections, concluding that network characteristics are positively and significantly correlated with firm attributes, such as sales, wages, capital, leverage, assets, output, and employees. Due to time constraints, I am not able to fully examine trafficking networks, but the relevance of money and power flows through traffickers’ supply chains and political ties suggests that this approach may lead to richer knowledge about how dirty employers escape law enforcement.

Of these works, Correia (2014) is closely related to my thesis. The author explores whether politically connected firms and their executives incur lower costs from enforcement actions by the Securities and Exchange Commission. The study draws on the literature of political control of bureaucracies where politicians use different tools to influence bureaucrats’ decisions, the reelection incentive of politicians to protect their constituencies and allies, and the idea that breaking long-term relationships can be costly for political support. The author asks whether firms with long-term connections face lower probability of being prosecuted and if, conditional on being prosecuted, their

penalties decrease when they have greater political sway. Measuring political connectedness based on the amount of PAC contributions and expenditures with lobbying, Correia runs a logit regression of probability of enforcement on these measures and controls for a set of firm characteristics. The paper finds that a one million dollar increase in PAC contributions made over the previous five years reduces the probability of an enforcement action from 8.58% to 3.43%, and that an increase of four million dollars in long-term lobbying reduces the probability of enforcement from 8.12% to 4.01%. In a set of cross-sectional analyses, the author explores heterogeneity in relationships with politicians that have greater influence on SEC enforcement and finds that long-term relationships with high-ranking politicians from majority parties and politicians serving in a relevant committee were more effective at reducing the probability of enforcement.

Lastly, my thesis is based on a series of assumptions drawn from the literature on the political control of bureaucracies and state capture. I postulate that traffickers anticipate politician interest in re-election and build rapport to reap benefits when needed, that politicians exert control of bureaucracies, and that risk-averse bureaucrats avoid shaming a connected firm to insulate their own careers. A diverse set of economic and political science studies has addressed these claims, taking into account information asymmetries (Banks and Weingast, 1992), term limits (Valsecchi, 2016), reassignment of bureaucrats after legislative renewal (Park and Somanathan, 2004), accountability mechanisms (Alesina and Tabellini, 2007), political uncertainty (Canen et al., 2021), corruption (Ferraz and Finan, 2011), and the influence of interest groups (Grossman and Helpman, 1994). More aligned with my work, Gordon and Hafer (2005) evaluate inspection activities of the Nuclear Regulatory Commission to understand how political expenditures of firms influence regulatory activity. In their model, regulatory agencies have discretionary power to impose costs to firms through enforcement of rules and statutes, and firms minimize the costs of contesting an agency's decision by choosing actions from a portfolio of tactics, which includes pre-enforcement (e.g. strengthen

political expenditures) and post-enforcement strategies (e.g. appeal in court). Authors argue that corporations use political expenditures not only to secure the support of political allies, but also to credibly signal to bureaucratic agencies that they are willing to fight an enforcement decision. They conclude that although larger donors commit more infractions, regulators monitor them less, but firms with observable problems reduce political spending as bureaucrats must employ a minimum amount of monitoring regardless of expenditure size. In this context, the expenditure itself instead of connections to politicians is enough to extract policy concessions and decrease agency enforcement, which contrasts Grossman and Helpman (1994) findings that special interest groups increase political expenditures to directly influence the incumbent’s policy choice.

The rest of the thesis is organized as follows: Section 2 provides institutional background; section 3 describes data sources, preparation, and the construction of political connection measurements; section 4 presents descriptive statistics; section 5 discusses the empirical framework; section 6 contains main results, and the last part concludes.

2 Institutional Background

2.1 Human Trafficking in Brazil

Human trafficking was recognized in Brazil in 2003 with the signature of an amicable settlement in the “Zé Pereira case” where the Brazilian Government accepted responsibility for omission and inefficiency in addressing allegations of human rights violations and modern slavery practices. The act resulted in the development of an ambitious agenda to fight human trafficking that included (i) improvement of state capacity to attend to complains of trafficking, (ii) support to rescued workers via labor compensation, priority access to cash transfer programs, and free legal counseling, (iii) punishment of traffickers, and (iv) implementation of monitoring and preventive measures as well

as awareness campaigns (Costa, 2009). Multiple stakeholders participate in the combat against forced labor, including government agencies, NGOs, academic institutions, the media, unions, and employer associations.

In particular, the creation of Special Mobile Inspection Groups (GEFM in Portuguese) under the Ministry of Labor has been paramount to centralize, document, and attend to anonymous exploitation complaints and prosecute perpetrators. From 2003 to 2018, GEFM has organized a total of 2,977 operations, carried out 4,168 inspections, rescued more than 40,000 workers, and identified over two thousand traffickers (Brazil, 2021; Brazil, 2022; SmartLab, 2020; Stanford Human Trafficking Data Lab, 2020). However, several barriers prevent the eradication of human trafficking in the country, and the Global Slavery Index estimated that nearly 2 in every 1,000 people were victims of trafficking in 2016 (Global Slavery Index, 2018).

Task forces within GEFM face increasing budget, staff, and time constraints in addition to entrenched systemic and operational inefficiencies that hamper labor inspectors' ability to target potential traffickers. Furthermore, human trafficking and modern slavery are seen as distinct problems. Indeed, the Brazilian forced labor definition is broader than the one adopted by the International Labour Organization and "Section 149 of the Brazilian Penal Code allows for prosecution of employers who subject their workers to particularly degrading conditions, whether or not there is clear evidence of coercion in the employment relationship" (Costa, 2009, pp. 16). However, modern slavery encompasses "forced labor, debt bondage, degrading work conditions, [harmful] long [work] hours, and any work that violates human dignity" (Murray and Lopez, 2020) whereas trafficking is treated differently in practice and covers only transnational forced labor and the sex industry. Even though recent amendments of the Penal Code represent a move towards the end of the differentiation between human trafficking and compulsory labor (for example Ordinance No 13.344/2016), the present conceptual gap fragments interagency efforts and narrows the scope of

state interventions, which end up focusing mostly on forced labor complaints.

Meanwhile, the dynamics of coercive labor markets have become more sophisticated and networks of traffickers, harder to identify. On the one hand, this is due to the nature of the economic sectors in which forced labor happens. In rural areas, for instance, violations of labor have been prevalent in the agricultural and charcoal production sectors, which tend to attract poorer workers, usually men with low educational attainment who migrate from other rural regions. One of the central obstacles in identifying these labor camps is that they are located in remote areas that are hard to access due to lack of proper roads and away from larger urban hubs where Labor Prosecution Offices are generally situated. On the other hand, supply chains have grown more complex. Specifically, outsourcing and subcontracting practices weaken accountability mechanisms for involved firms and individuals, and transform the criminalization of perpetrators into a black box.

Albeit clandestine, forced labor in Brazil happens in important export sectors that are labor-intensive, such as agriculture, cattle ranching, forestry, and charcoal production. These activities stimulate demand for both cheaper labor and land clearing, which have been noticeably linked to deforestation in the Amazon region. In the urban sector, modern slavery is prevalent in construction, domestic work, garment, and the sex industry and exploit men, women, and undocumented immigrants, most of whom come from neighboring Latin America countries.

2.2 The Dirty List Process

Organized by the Division of Inspection to Eradicate Slave Labor and published on the website of the Ministry of Labor twice a year, the Dirty List of Slave Labor is a punishing instrument of immediate effect while employers face civil and criminal prosecution. Albeit having more serious

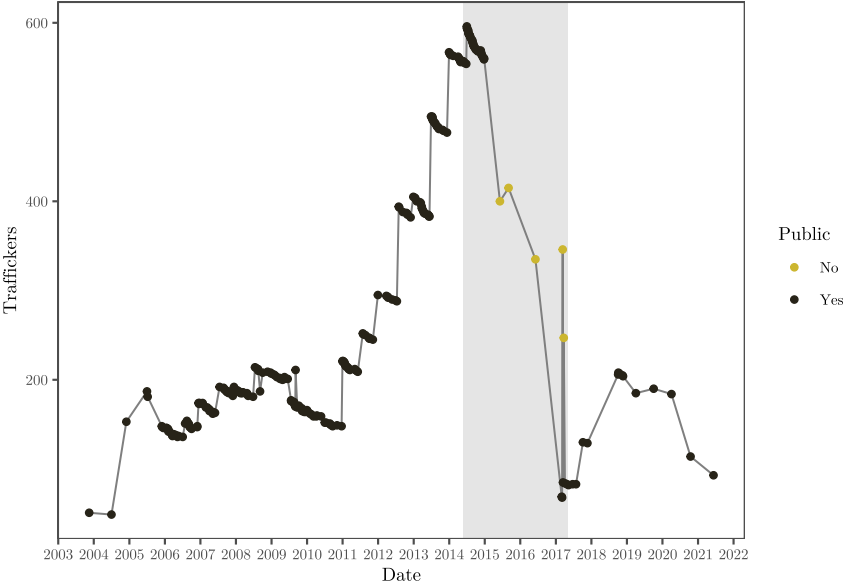
and long-lasting consequences, such as conviction, the legal route is subject to slow proceedings and lack of harmonization around the interpretation of what constitutes trafficking. The List circumvents this problem by penalizing employers on the administrative front, undermining the economic structures that sustain trafficking. Violators lose access to loans from public banks for the duration of time that they are listed and suffer from the stigma of the labor exploitation label that comes with the inclusion on the DL. More than 400 national and international companies have agreed to refrain from trading with listed employers by signing the National Pact for the Eradication of Slave Labor, and consumers are increasingly demanding of sustainable, dignified, and equitable labor standards.

This policy is one of the central initiatives adopted by the government, and the process for inclusion in the List follows a pipeline initiated either by a legal case or labor inspection. In instances where a legal case takes place, the listing decision is the outcome of a judicial resolution; otherwise, GEFM carries out an inspection in cooperation with the Labor Prosecution Office and police departments to analyze whether conditions in inspected establishments typify trafficking. In this thesis, I focus on the latter.

As mentioned, GEFM operations are triggered by anonymous labor complaints, often sent by organizations that support survivors such as the *Comissão Pastoral da Terra* (CPT in Portuguese). Due to the identified constraints, labor inspectors make a decision about whether the complaint describes critical indicators of trafficking, for example high-prevalence economic sectors and restriction of movement, and assemble a team to carry out several inspections. After an *in situ* assessment, inspectors gather evidence that allow them to characterize the labor practices as trafficking, free workers and provide resources for further assistance, and refer perpetrators both to administrative and judicial authorities. All of these activities are described in inspection reports.

Administrative officials, then, make a secondary decision about whether to add the employer in the Dirty List based on inspector recommendation. Once traffickers are listed, regulation determines that their information remains publicly available for two years and removal is conditional on payment of fines and compensation to rescued workers. However, shortly after the DL goes public, traffickers may start a legal case against it and ask for a revision. Their pleas are usually accepted by a first judge just to be nullified in another court, and these temporary removals are reflected in several List updates. Figure 1 demonstrates this phenomenon, showing small but recurrent reductions in the number of listed entities every time a new List is re-issued. The so-called “DL process” and its decision nodes are represented in details in Figure A.1, and a snapshot of the most recent DL publication is displayed in Figure A.2.

Figure 1: Employers in a Dirty List Publication



Subjective decision-making criteria are employed in several steps of the DL process. Because labor camps are highly dynamic and itinerant, the delay between receiving the tip and a task force arriving at the destination might be too long. Here, I focus on the outcomes of listed and unlisted traffickers who have been inspected and, more broadly, on the outcomes of all listed entities.

Nevertheless, differential treatment of politically connected perpetrators could be at play in earlier stages of the DL process, such as the decision to inspect and typify the case as modern slavery. Currently, data on complaints and granular labor camp conditions is absent, and additional research efforts should focus on these steps.

2.3 Unconstitutionality Claims and Suspension of DL Publication (2015-2017)

GEFM inspections result in a legal case that determines the penalties to be applied to employers, but the inclusion in the Dirty List is independent of a court's decision. In fact, the List is an administrative determination rather than criminal prosecution, and various sectors of the industry contend that it violates the principle of presumption of innocence, offends private property, and lacks constitutional legitimacy because it was instituted via an executive ordinance.

As a consequence, the Brazilian Association of Real Estate Developers (Abrainc in Portuguese) brought a constitutional challenge against the DL before the Brazilian Supreme Court alleging infringement of due process in addition to the above claims. A preliminary injunction that determined the List's suspension was granted in December of 2014, but the Federal Administration and the Brazilian Federal Public Ministry (also known as Public Prosecution Office) appealed the decision, emphasizing the importance of the policy for the country's anti-trafficking agenda.

Several other organizations also opposed this ruling. The NGO Repórter Brasil took advantage of the Right to Information Law – the Brazilian Freedom of Information Act – to access undisclosed lists in the years of DL suspension and posted them on their website. Yet, it is unclear whether such action had a similar effect to the official publication. Figure 1 displays the number of listed employers in unpublished lists in yellow, but it does not show any temporary removals as described in the previous section, suggesting that regulatory and non-governmental shaming are

not substitutable from the trafficker perspective. I exploit this variation in visibility of the List by testing whether traffickers seek to evade reputational damage via connections in the same way across published and unpublished DLs.

In May of 2016, the Supreme Court decision was revoked, but the List was only made available on the government portal again in 2017 (“Prohibition of Forced and Child Labor,” 2017).

3 Data

3.1 Sources

To analyze the differential treatment received by politically connected traffickers, I constructed a novel dataset of campaign donation of traffickers from 2004 to 2018 utilizing several sources, some of which were prepared by the Stanford Human Trafficking Data Lab (HTDL or “Lab”) in partnership with the Brazilian Federal Labor Prosecution Office (FLPO). Extensive data cleaning was necessary to produce the final datasets and detailed procedures may be found in the [Data Treatment](#) section of the Appendix.

3.1.1 Dirty List of Slave Labor

The DL is updated semesterly by the Ministry of Labor and posted online. It contains identification information of trafficker, type of ID (which allows me to distinguish between firms and bosses), and publication date. HTDL compiled all lists from 2003 to 2021, including unpublished lists in the 2015-2017 period that were obtained via the FLPO. I collapsed the merged lists to the trafficker level and created variables indicating date of listing, date a trafficker appears on the List for the last time, date of removal (defined as the first publication where the trafficker information does not

show up), and whether the List was public or not. I then calculated total days on the DL, taking into account temporary removals that result from appeals against the listing decision.

In the period of reference, 270 lists were published, 5 of which were private, and 1,804 uniquely identified traffickers were listed. The breakdown between firms and bosses in published, unpublished, or both types of lists is given in Table 1. Notice that in four instances the identification number of the establishment, rather than the employer ID, was named. I do not consider these in the econometric estimation.

Table 1: Traffickers in Published and Unpublished Lists, by Type of Entity

	Firm	Boss	Establishment (farm)	Total
Published	300	784	4	1088
Unpublished	99	241	0	340
Both	109	267	0	376
Total	508	1292	4	1804

3.1.2 Inspection Reports and Construction of Trafficking Flag

Inspection reports produced by the Secretariat of Labor Inspection within the Ministry of Labor supplied data on entities subject to a labor inspection under suspicion of trafficking. Each report summarizes an investigation and contains details about employers, workers, business location, economic activity, dates of inspection, labor code violations, fees and compensation owed to workers, inspectors and police support, indicators of deforestation, presence of charcoal ovens, and a determination of the leading inspector about whether labor conditions typified trafficking. Reports span years 1996 to 2021, but they are originally stored in non-searchable pdf documents, often with handwritten passages. Recent data extraction efforts by HTDL in partnership with the FLPO made available preliminary tables with a subset of reports from 2010-2021.

During the production of this thesis, a trafficking flag in the inspection dataset was not available, and I resorted to an implicit method to devise a trafficking flag which conveniently identifies

unlisted perpetrators. Several data collection difficulties explain the absence of such an indicator in the dataset and, in addition to inspection findings and recommendations, there are other ways to identify forced labor in the country, for example with records of civil or criminal prosecution and other administrative actions. Here, I chose to use the Dirty List as a proxy of trafficking. Exploiting the fact that multiple companies are involved in any given investigation (Figure 2), if an inspection involved at least one listed trafficker, then all employers in that inspection were flagged as traffickers as well. Figure 3 illustrates this method.

Figure 2: Distribution of Employers per Inspection

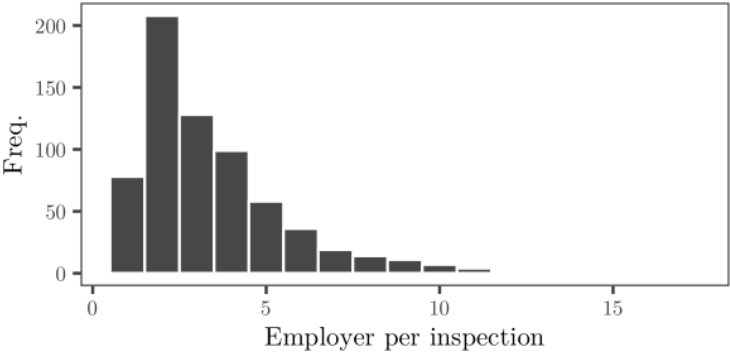
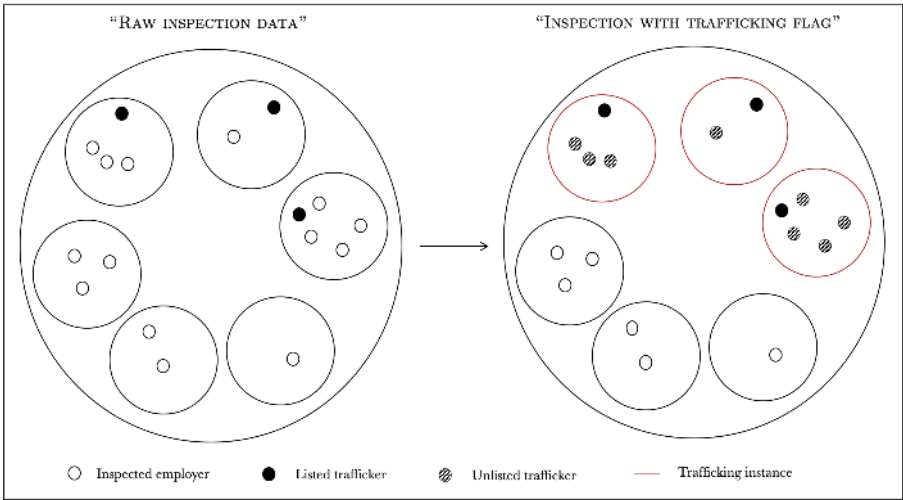


Figure 3: Devising a Trafficking Flag



Notes: Larger circles represent the universe of inspections and each subcircle is an inspection instance. In the raw data, I only observe who was implicated in each case and whether any of the employers were listed. I then use the Dirty List as a proxy for trafficking status of all employers in the same inspection site of the listed entity, arriving at the constructed trafficking flag.

This definition has two limitations. One is that it is possible that inspections where trafficking was confirmed are overlooked if none of its employers were included in the DL. Another drawback is that traffickers might not be inspected in the first place, but were included in the DL by adjudication. In either case, it is highly likely that my approach leads to undersampling of unlisted traffickers.

In addition, it is not unusual that some employers are inspected more than once, given that one of the pillars of the anti-trafficking agenda in Brazil involves monitoring previously targeted employers. The outcomes of these different instances may vary, but the patterns are too heterogeneous to allow for a simple treatment of these cases. For this reason, I exclude 33 employers with multiple inspections from my analysis.

The dataset I obtained included inspection records from 2010 to 2018 specifying inspection identification key, employer identification number and ID type, number of rescued workers, location of trafficking, economic activity, dates of inspection, number of labor code violations, and total compensation owed to rescued workers. Unlike the DL dataset, inspection records provide information at the inspection level and do not specify who paid worker compensation nor what labor violations were associated with which employer. My sample provided information on 669 inspections and 2,256 inspected employers. Of these, 399 inspections found trafficking involving 1,278 listed and unlisted traffickers.

3.1.3 Superior Electoral Court Data Repository

Elections in Brazil happen biennially and alternate between local (mayor and municipal representatives) and national elections (president, governors, federal and state representatives, and senators). The Brazilian Superior Electoral Court (TSE) data repository makes available historical campaign

finance data and candidate metadata. The first registers donations from a donor to a candidate in a given election and has candidate and donor identification (name and ID number); information on candidate's political party and political office; size, type, and date of contribution; state, city, level (national or local), type (ordinary or supplementary), and date of election; and date and amount of donation. I downloaded and treated records from 2004 to 2018, obtaining a raw dataset with 10,186,076 observations. After extensive cleaning, I ended with 3,478,727 observations of annual contributions of each donor-candidate pair (refer to the [Data Treatment](#) in the Appendix for full details).

Candidate metadata provided additional demographic information, such as gender, birth date and place, race, level of education, and marital status, as well as whether they were elected. However, discrepancies across years are common and some of these variables were not available at all times. For example, files of recent years include an indicator for reelection that is absent in earlier records, and candidate personal identification was recorded only after 2006. Regardless, the main use of this metadata was to match the names of ruralist politicians to their unique identification number.

3.1.4 Ruralist Affiliation

“Ruralist” is a broad denomination attributed to legislators who are aligned with and defend the agenda of agricultural producers and farm owners. Its largest bloc is represented by the Agricultural Parliamentary Front (FPA) at the federal level. Starting in 2008, parliamentary fronts were mandated to report the list of affiliated members to the Journal of Chamber of Deputies, which constitutes one of my sources of data. I expanded this definition to include politicians who supported a pro-trafficker bill within the Committee on Agriculture, Livestock, Food Supply and Rural Development of Congress (CAPADR). Specifically, I considered roll call voting records on PL 3.842/2012,

a bill approved in 2015 that proposed the elimination of the terms “exhaustive labor journey” and “degrading working conditions” from the trafficking definition set forth in the Brazilian Penal Code. Even though this Committee discussed other pro-trafficker bills, PL 3.842/2012 was the only one voted, approved, and sent to the floor for consideration of all federal representatives. Note that “ruralist” has been defined in other ways as well, the main alternative methodology including analysis of voting behavior of all politicians in a given legislative period. Due to time constraints, I relied on member affiliation with FPA and supporters of the aforementioned bill within CAPADR.

FPA affiliation lists consist of mandatory registration of parliamentary front members, which were established by the Act of the Board 69 of November 11, 2005. They are published on the Journal of the Chamber of Deputies (*Diário da Câmara dos Deputados* or DCD), and were available for the years 2008, 2011, 2015, and 2019. The second source were reports of activities of CAPADR that are available at the Digital Library of the Chamber of Deputies and contain name, political party, and state of active Committee members as well as roll call votes for bills appraised. The bill of interest is PL 3.842/2012, which proposed a modification in the definition of slave labor to a less stringent one and was voted in 2015. The 2015 CAPADR report states that PL 3.842/2012 was approved almost unanimously by the Committee – only 3 out of 48 permanent members abstained from voting.

I combined the two ruralist datasets using politician name and politician state, and took advantage of the candidate metadata to assign each ruralist to their ID number using several methods to account for duplicates, misspelled names, and homonyms. Lastly, I used the politician identifier to create a ruralist flag in the donation dataset indicating if a candidate had ever been affiliated with FPA from 2008 onward or voted on the pro-trafficking bill in 2015. I was able to recognize 665 politicians who affiliated with FPA and 44 who voted “Yes” – 39 of whom were already FPA members –, getting a total of 670 ruralists. Precise steps in the methods applied to

the merging process may be found in the Appendix.

Three caveats are in order. Although FPA became official in 2008, it already existed as an informal organization within Congress prior to its registration. Arguably, selection bias might have been at play: the public announcement of FPA affiliation on the DCD may have discouraged some politicians from wanting to be publicly or officially associated with the group due to liability and transaction costs, and they might have left the organization after 2008. For this reason, extrapolating FPA's composition back in time, i.e. before 2008, based on future association could lead to an undercount of the number of ruralists, but the same is not necessarily true for those who affiliated after 2008. Hence, ruralist classification covers years 2008 through 2018 only. Second, FPA composition is dynamic and changes during the legislative term, but these modifications are not currently traceable so I considered only static snapshots of FPA's composition at time of registration. Like before, this approach potentially under-represent total members of FPA. This limitation may underestimate the effect of being connected to a ruralist, but this is an inconsequential problem as it does not prevent me from measuring the effects of being connected to politicians more broadly. Third, even though the PL 3.842/2012 bill was voted in 2015, I assume that policy preferences of ruralists are constant over time. For evidence on the strategic alignment of FPA and CAPADR and the influence of groups of interest on their political activity, see Bruno (2017), Coradini (2010), and Cruz (2015).

3.2 Data Preparation

3.2.1 Data cleaning

Extensive data cleaning was necessary to create the final datasets and detailed procedures may be found in the Appendix.

3.2.2 Merging Procedure

After preparing the trafficker, inspection, donation, and ruralist data separately, I merged them as follows. First, I used the trafficker identification number to match observations in the Dirty List and inspection records. Notice that because I have a sample as opposed to the full universe of inspections, only 393 listed entities were matched. Then, I identify ruralist politicians in the donation dataset using candidate identification number. Of the 670 ruralists, 651 were associated with 112,820 records of campaign finance. Next, I use donor identifier to merge inspection records with DL indicator and the donation data, and finally I include all other listed traffickers without an inspection. The final dataset stores observations from 2004 to 2018 at the level of annual donation of trafficker-candidate pairs and I impute zeros for years when a donation did not happen. Because candidate identification number is unavailable in 2004 and 2006, I calculate political connections using candidate name as identification key. Although this is not the preferred approach because the dataset might contain homonyms or misspelled names over the years that may lead to under or overcounting of politicians, I resort to such method in order to expand my sample size which is already small.

3.2.3 Constructing Datasets for Analysis

To study the impact of political connections in the context of trafficking in Brazil, I consider cumulative campaign funding prior to two events: labor inspection and inclusion in the Dirty List. Recall that I impute zeroes when a contribution in a given election year was not identified, but the relative timing of and inspection and DL publication of each trafficker implies that I observe donation behavior of some perpetrators for a longer period of time. Thus, in order to evaluate trafficker outcomes across listed and unlisted employers in a comparable fashion, I produce two

balanced panels of unconditional and one panel of conditional election funding.

In the first balanced panel, I restrict the contribution window to the 5 years prior to inspection, including the year of inspection, and obtain a dataset with contributions from 1,223 traffickers inspected in the 2010-2019 period, 855 of which remained unlisted. Similarly, the second dataset is derived with the same methodology but relative to time of DL publication, including the year of listing, and I keep only traffickers listed after 2008 and before 2019, which gives me a dataset with 1,286 listed perpetrators among donors and non-donors.

The choice of this 5-year period was driven by the data rather than by a conceptual framework. Indeed, one of the main limitations of my study is low power associated with the meager number of donating traffickers (see Tables 3 and 4) and I aimed to maximize the sample size of dirty donors when choosing the time boundaries to form these balanced panels. Notice that in the former, the lower bound is set by the data while in the latter, I choose to track contributions for three electoral cycles. I also generate a third dataset of donation conditional on inspection that encompasses the period between an inspection and DL publication. Even though the last one is not a proper balanced panel as the delay between the two events varies (see Figure D.7), the same event boundary is applied to all traffickers.

Finally, the trends identified in the raw data and displayed in Figure 5 motivate the creation of a fourth unbalanced panel consisting of the donation records of all listed and unlisted traffickers in the 8 years leading up to an inspection. The choice of the 8-year period was determined by the figure referenced above as it seems to be the time where donating behavior of listed and unlisted exploiters were noticeably different. This dataset is mainly used to reproduce the relationship of such graph econometrically and contains 1,253 listed and unlisted traffickers.

The dirty employers in these four tables constitute the samples in my econometric estimation.

Traffickers on the List with balanced unconditional donation compose sample 1; traffickers on the List with balanced conditional donation form sample 2; listed and unlisted traffickers with balanced unconditional donation are represented in sample 3, and listed and unlisted traffickers with unbalanced unconditional donation are represented in sample 4.

3.3 Measuring Political Connections

In my analysis, I exploit several definitions of political connections based on election funding as described above. My preferred measure is a broad dummy indicating if the trafficker has ever made any donation. Next, I consider the cumulative number of candidates and cumulative donation. Because the distribution of contributions is extremely right-skewed and contains many zeroes, I apply an inverse hyperbolic sine transformation, which can be interpreted similarly to a log-transformation (Aihounton and Henningsen, 2021). Likewise, the distribution of candidates is right-skewed, and most of its mass lies under 5. For this reason, in addition to estimating the effects of cumulative number of candidates on the outcomes of interest, I discretize this variable to individually show the effects of donating to exactly 1, 2 to 5, and more than 5 candidates (refer to Figures B.1 and B.2 for density plots of raw and transformed cumulative donations and distribution of candidates.)

To explore heterogeneities between ruralist and non-ruralist politician, I add an indicator that takes value 1 if any of the candidates was ruralist. Furthermore, I build on the idea that relationships between traffickers and politicians grow stronger if they “interact” more than once. To account for long-term links consisting of recurrent donations prior to inspection or inclusion in the Dirty List, I develop a dummy for whether each trafficker i donated more than once to any given politician.

Exclusively in Sample 4, I consider where donors are situated with respect to the full distribution of donations. In this way, I differentiate traffickers who always donates more than the median value of donations, for example. To calculate the indicator that informs whether any of a trafficker's donation was above a given percentile score, I proceed as follows. With the unbalanced panel of contributions from listed and unlisted inspected traffickers, I calculate the percentile scores of non-zero donations and create a dummy *Above X^{th}* that takes value 1 if the money transfer from the trafficker i in year t was strictly above the X^{th} percentile score. Next, I keep only the donations made from year -8 to 0 relative to an inspection, where 0 represents the inspection year, and collapse the dataset from trafficker-year to trafficker level by performing two tasks: I keep the maximum value of *Above X^{th}* , which generates the new variable *Any above $X^{th}_{[-8,0]}$* , and add up all donations ever made in the time window of reference. Notice that *Any above $X^{th}_{[-8,0]}$* refers specifically to the 8 years leading up to an inspection such that if a trafficker makes small contributions prior to inspection and a large election payment afterwards, the indicator still takes value 0. Lastly, I calculate the square of the transformed cumulative donation transformed.

Two aspects of my approach must be highlighted. First, I do not differentiate between elected and non-elected candidates. I assume candidates who run for office accumulate political capital and obtain privileged access and information even when they do not win a race such that they would still be able to offer support to friendly traffickers. Second, the level of a firm's effort, i.e. donation relative to total assets, is irrelevant for the construction of my measures. Reasonably, larger firms/richer bosses donate more in absolute terms but possibly less in proportion to their size compared to smaller firms/poorer bosses, but I posit that candidates neglect firm's effort and care only about absolute values. For this reason, I do not adjust donation to firm characteristics.

Table 2 describes all of these variables and specifies to which samples they belong.

Table 2: Description of Political Connection Measurements

Variable	Description	Samples
Any donation	Dummy for whether trafficker made any donation	1, 2, 3
N candidates	Cumulative number of candidates	1, 2, 3
N candidates = 1	Dummy for cum. candidates is exactly 1	1, 2, 3
N candidates between 2-5	Dummy for cum. candidates is between 2 and 5	1, 2, 3
N candidates > 5	Dummy for cum. candidates is greater than 5	1, 2, 3
Cum. donation ₅ , arcsinh	Cum. donation from balanced panel in R\$ with IHS transformation	1, 2, 3
Cum. donation ₈ , arcsinh	Cum. donation from unbalanced panel in R\$ with IHS transformation	4
(Cum. donation ₈ , arcsinh) ²	Cum. donation from unbalanced panel in R\$ with IHS transformation, squared	4
Long-term connection	Dummy for whether trafficker has any long-term connection	1, 2, 3
Any ruralist	Dummy for whether any of the candidates is ruralist	1, 2, 3
Any above $X_{[-8,0]}^{th}$	Any donation above Xth percentile	4

Notes: R\$ = reais. IHS = inverse hyperbolic sine.

4 Descriptive Statistics

Although I have access to rich data spanning several years, one of the main limitations of my study is that my data contain few donating exploiters. Table 3 show the breakdown of all listed, listed and identified in the inspection dataset, and unlisted traffickers who made a donation, where the largest number of dirty donors with an inspection record is 36 in 2008. Overall, merely 4.5% of unlisted traffickers donated, which compromises the power of estimates. The same is not true for listed exploiters whose post-DL outcomes are compared across donors – roughly 18% – and non-donors (see Table 4.)

Table 3: Annualized Frequency Table of Donors, Raw Data

Year	Donating Trafficker			
	Listed (All)	Listed (with an inspection record)	Unlisted	Total
2004	97	16	15	112
2006	80	8	14	94
2008	124	21	15	139
2010	78	10	10	88
2012	90	18	17	107
2014	71	9	14	85
2016	53	7	8	61
2018	29	0	4	33

Notes: The same trafficker might have donated in multiple elections.

Table 4: Cumulative Frequency Table, Raw Data

	Trafficker		
	Listed	Unlisted	Total
Donor	332	40	372
Non-donor	1472	845	2317
Total	1804	885	2689

After making a remark about the difficulty around the number of dirty donors in the raw data, Table 5 presents statistics for each variable in Samples 1 through 4. Notice how even though my samples have different number of observations and slightly distinct donation windows, they are similar within their events of reference (Samples 1 and 2 and Samples 3 and 4).

In Samples 1 and 2, just over 50% of listed traffickers gets off the List in less than 2 years, the average length of stay on the List is 25 and 22 months, and standard deviations are sizable, signaling considerable variation in the outcomes of traffickers. In Sample 1, 11% of traffickers donated, but the average number of candidates and average cumulative donation is low. Long-term and ruralist connections are verified for a small subset of these employers. In Sample 2, all of these connectedness measures decrease by more than a half and there are no long-term connections, arguably because the donation window is considerably smaller. Observations for which the ruralist indicator is present contain 9 fewer traffickers across both samples because I do not code ruralist in election years earlier than 2008. For example, employers listed in 2010 will be tracked in 2006, 2008, and 2010 in the balanced panel, but I cannot tell whether or not there was a ruralist link in 2006. Hence, I code the ruralist variable as a missing value for these traffickers and exclude them in estimations with such indicator.

Samples 3 and 4 are also quite alike with 30% of listed traffickers and similar standard deviations. Cumulative donation is slightly higher in Sample 4, which could be associated with the longer donation window, but this gap is not statistically significant (p-value of two sample t-test

= .323). Distribution indicators *Any above X^{th}* confirm that very few traffickers are large donors, with 27 donating above the median and 3 above the 95th percentile. Although these are small values in absolute terms, they still comprise 32% and 3.5% of the donating set. For the sake of brevity, I include only the 50th, 75th, 90th, and 95th percentiles in Table 5, and all other percentiles are included in Table B.1 in the Appendix. Finally, the difference in the number of observations between Samples 3 and 4 is caused by the construction of balanced panel in Sample 3, where I exclude traffickers whose donations I cannot track for 5 years prior to inspection.

The share of traffickers in rural areas and their distribution across economic sector is almost the same, although the inclusion of unlisted traffickers in Samples 3 and 4 indicate that there are more bosses than firms (16%) relative to Samples 1 and 2 (31% and 24%). These basic statistics align with qualitative studies that point out the disproportionate concentration of trafficking in rural areas and in agribusiness: together, cattle ranching, agriculture, and extraction comprise more than 65% of traffickers in my data.

Additional descriptive plots that explore other dimensions in the data are displayed in Section D of the Appendix and help validate data quality.

Table 5: Descriptive Statistics

Variable	Sample 1			Sample 2			Sample 3			Sample 4		
	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs
<i>Outcomes</i>												
Listed	-	-	-	-	-	-	0.3	0.46	1223	0.31	0.46	1253
Off DL before 2 years	0.51	0.5	1286	0.53	0.5	351	-	-	-	-	-	-
Total Days on DL	763.34	434.19	1286	656.22	338.69	351	-	-	-	-	-	-
<i>Political Connection Measurements</i>												
Any donation	0.11	0.32	1286	0.034	0.18	351	0.045	0.21	1223	-	-	-
N candidates	0.5	4.64	1286	0.091	0.75	351	0.71	12.14	1223	-	-	-
N candidates = 1	0.066	0.25	1286	0.02	0.14	351	0.024	0.15	1223	-	-	-
N candidates between 2-5	0.03	0.17	1286	0.011	0.11	351	0.016	0.12	1223	-	-	-
N candidates > 5	0.016	0.13	1286	0.0028	0.053	351	0.0057	0.075	1223	-	-	-
Cum. donation ₅ , arcsinh	1.15	3.35	1286	0.34	1.9	351	0.45	2.18	1223	-	-	-
Cum. donation ₈ , arcsinh	-	-	-	-	-	-	-	-	-	0.54	2.35	1253
(Cum. donations ₈ , arcsinh) ²	-	-	-	-	-	-	-	-	-	5.8	28.07	1253
Long term connection	0.0047	0.068	1286	0	0	351	0.0082	0.09	1223	-	-	-
Any ruralist	0.022	0.15	1277	0.0085	0.092	351	0.009	0.095	1221	-	-	-
Any above 50 _[-8,0]	-	-	-	-	-	-	-	-	-	0.022	0.15	1253
Any above 75 _[-8,0]	-	-	-	-	-	-	-	-	-	0.011	0.11	1253
Any above 90 _[-8,0]	-	-	-	-	-	-	-	-	-	0.004	0.063	1253
Any above 95 _[-8,0]	-	-	-	-	-	-	-	-	-	0.0024	0.049	1253
<i>Controls</i>												
Firm	0.31	0.46	1286	0.24	0.43	351	0.16	0.37	1223	0.16	0.37	1253
Rural	-	-	-	-	-	-	0.78	0.41	1223	0.78	0.41	1253
<i>Economic Sector</i>												
Agriculture	-	-	-	-	-	-	0.18	0.38	1223	0.18	0.38	1253
Commerce or services	-	-	-	-	-	-	0.18	0.39	1223	0.18	0.39	1253
Construction	-	-	-	-	-	-	0.11	0.31	1223	0.11	0.31	1253
Domestic work	-	-	-	-	-	-	0.0033	0.057	1223	0.0032	0.056	1253
Charcoal	-	-	-	-	-	-	0.095	0.29	1223	0.097	0.3	1253
Timber	-	-	-	-	-	-	0.087	0.28	1223	0.088	0.28	1253
Food industry	-	-	-	-	-	-	0.0041	0.064	1223	0.004	0.063	1253
Livestock	-	-	-	-	-	-	0.29	0.46	1223	0.29	0.46	1253
<i>Trafficker</i>												
Listed		✓			✓			✓			✓	
Unlisted								✓			✓	
<i>Donation</i>												
Unconditional pre-list		✓										
Conditional post-inspection, pre-list					✓							
Unconditional pre-inspection								✓			✓	
Balanced Panel		✓			✓			✓				

5 Empirical Framework

To analyze the effect of political connection on the outcomes of interest, I estimate the following base model:

$$Y_i = \alpha + \beta \textit{Political Connection}_i + \phi \textit{Any Ruralist}_i + \gamma \mathbf{M}_i + \eta_{DL} + \varepsilon \quad (1)$$

where Y_i represents each of the three outcomes considered: $\textit{Prob}(\textit{Listed} | \textit{Inspection} = 1)$ refers to the probability of being included in the Dirty List conditional on being inspected; $\textit{Off DL in less than 2 years}$ is a dummy indicating whether the trafficker left the DL earlier than allowed by regulation conditional on being put on the List, and $\textit{Total Days on DL}$ is self-explanatory and also conditioned on being listed. $\textit{Political Connection}_i$ and $\textit{Any Ruralist}_i$ are as described in Subsection 3.3 and \mathbf{M}_i is a matrix of three employer characteristics: economic sector proxied by inspection records, rural or urban area, and whether the trafficker is a firm. At the moment this thesis was finalized, Sample 1 with all listed traffickers contained only the firm variable. η_{DL} represents DL year fixed effects that are included to account not only for macroeconomic shocks but also unobservable year-to-year variation in resource availability within regulatory agencies. Note that when the outcome is $\textit{Prob}(\textit{Listed} | \textit{Inspection} = 1)$, I substitute η_{DL} for state of inspection (δ_s) and year of inspection fixed effects (θ_y), which account for trafficking prevalence and institutional peculiarities in different geographic locations and temporal shocks. The main effect of interest is the β of all specifications and ϕ is utilized to capture politician heterogeneity.

A plausible contention against the story behind political connections is that traffickers may exhibit unobservable characteristics correlated with their decision to participate in campaign finance such that donation is not a production factor that mitigates likelihood of reputational damage. To test for this argument, I take advantage of the fact that the Supreme Federal Court suspended

publications of the Dirty List from 2015 to 2017 and, although loan restrictions were still in place, the reputational damage no longer harmed newly named traffickers. To compare employers in Lists with different visibility levels, I modify the base estimation to a fully interacted model as follows:

$$\begin{aligned}
Y_i = & \alpha + \beta_1 \textit{Political Connection}_i + \phi_1 \textit{Any Ruralist}_i + \gamma_1 \mathbf{M}_i + \rho \textit{Public List}_i + \\
& + \beta_2 \textit{Public List}_i * \textit{Political Connection}_i + \phi_2 \textit{Public List}_i * \textit{Any Ruralist}_i \\
& + \gamma_2 \textit{Public List}_i * \mathbf{M}_i + \eta_{DL} + \varepsilon
\end{aligned} \tag{2}$$

In equation 2, I am mostly interested in the interaction between *Public List_i* and *Political Connection_i*, and focus on the coefficient β_2 . Notice that this model is only applicable to the outcomes *Off DL in less than 2 years* and *Total Days on DL* given that the visibility of the List does not affect unlisted traffickers in the way I conduct my analysis. Arguably, the likelihood of being listed is also correlated with expectations of visible punishment, but I do not attempt to evaluate this channel. Models 1 and 2 are employed to study Samples 1 through 3.

The preliminary results I obtain with the specifications above prompt me to consider non-constant returns to donation and I run the following equations using donations from the unbalanced panel used in Sample 4:

$$\textit{Listed}_i = \alpha + v \textit{Any above } X_{[-8,0]_i}^{th} + \gamma \mathbf{M}_i + \delta_s + \theta_y + \varepsilon \tag{3}$$

$$\begin{aligned}
\textit{Listed}_i = & \alpha + \mu_1 \textit{Cum. Donation}_{8_i} + \mu_2 (\textit{Cum. Donation}_{8_i})^2 + v \textit{Any above } X_{[-8,0]_i}^{th} \\
& + \gamma \mathbf{M}_i + \delta_s + \theta_y + \varepsilon
\end{aligned} \tag{4}$$

where *Cum. Donation_{8_i}* and *Any above X_{[-8,0]_ith}* are as described in Subsection 3.3, \mathbf{M}_i is as in previous models, and δ_s and θ_y are state and year of inspection fixed effects. Specifically, model

3 is a direct attempt to obtain an econometric representation of Figure 5 and model 4 assesses whether there are diminishing or increasing returns to total amount donated. Thus, the central estimates are v and the marginal effects of $Cum. Donation_{8_i}$ can be described by the expression:

$$\text{Marginal Effects of Cum. Donation}_8 = \mu_1 + 2\mu_2 Cum. Donation_{8_i}. \quad (5)$$

The scant literature on my research topic implies that a well-established functional form is missing. Hence, the specifications above offer ways to explore the relationships observed in the raw data, but they do not exhaustively explain all trends observed nor are the last word on the topic. All equations are estimated with a linear probability model.²

6 Results

6.1 Graphical Analysis

Before presenting econometric results, I offer a graphical analysis of the raw data that set expectations for the key coefficients estimated. Figures 4 through 7 describes the main outcomes of the Dirty List for donating and non-donating trafficker.

Figure 4(a) shows that the share of listed traffickers over time is higher for donating exploiters in any inspection year, an unsurprising indication that bad behaving employers give more money. However, looking more closely at the evolution of average campaign contribution relative to year of inspection, a different pattern arises. Panels (a) and (b) of Figure 5 hint at the possibility that traffickers with larger rent payments are listed less often than others, a common tendency across those inspected in electoral and non-electoral years. However, when traffickers are subdivided by

²Logit regressions have been calculated, but estimates and standard errors were too similar and I omit them for conciseness.

those who donated above and below a given percentile of contribution, I verify that that is not necessarily true and, instead, there seems to exist a threshold above which donations payoff. In my data, that threshold seems to be the 90th percentile (for additional percentile breakdowns, refer to Figure C.1 in Section C.1 of the Appendix.) What seems to be consistent across all plots, nevertheless, is that post-inspection donation becomes virtually inexistent across listed and unlisted exploiters, signaling that being inspected is a shock that affects the level of connectedness of traffickers. The sample of those connected to ruralist is too small and it is hard to conclude a clear pattern from Figure 4(b).

Figure 4: Share of Listed Traffickers, by Year of Inspection

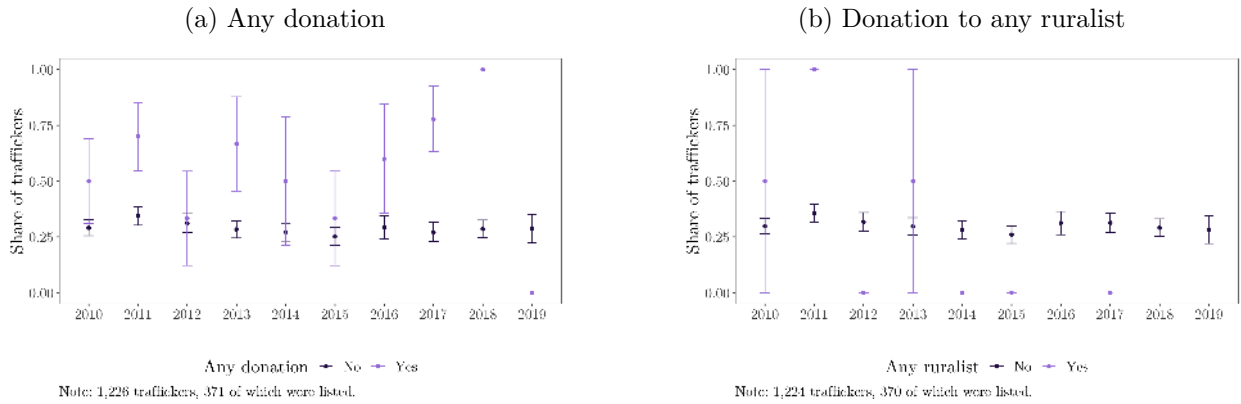


Figure 5: Average Donation Relative to Inspection

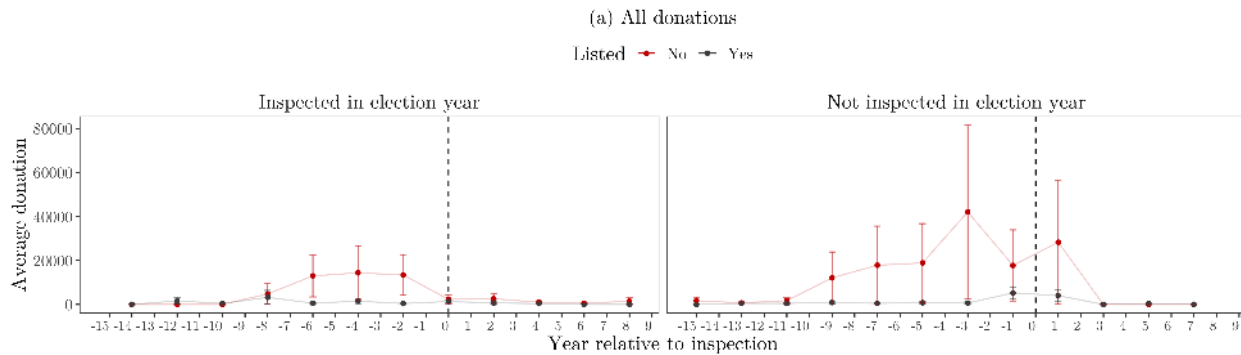
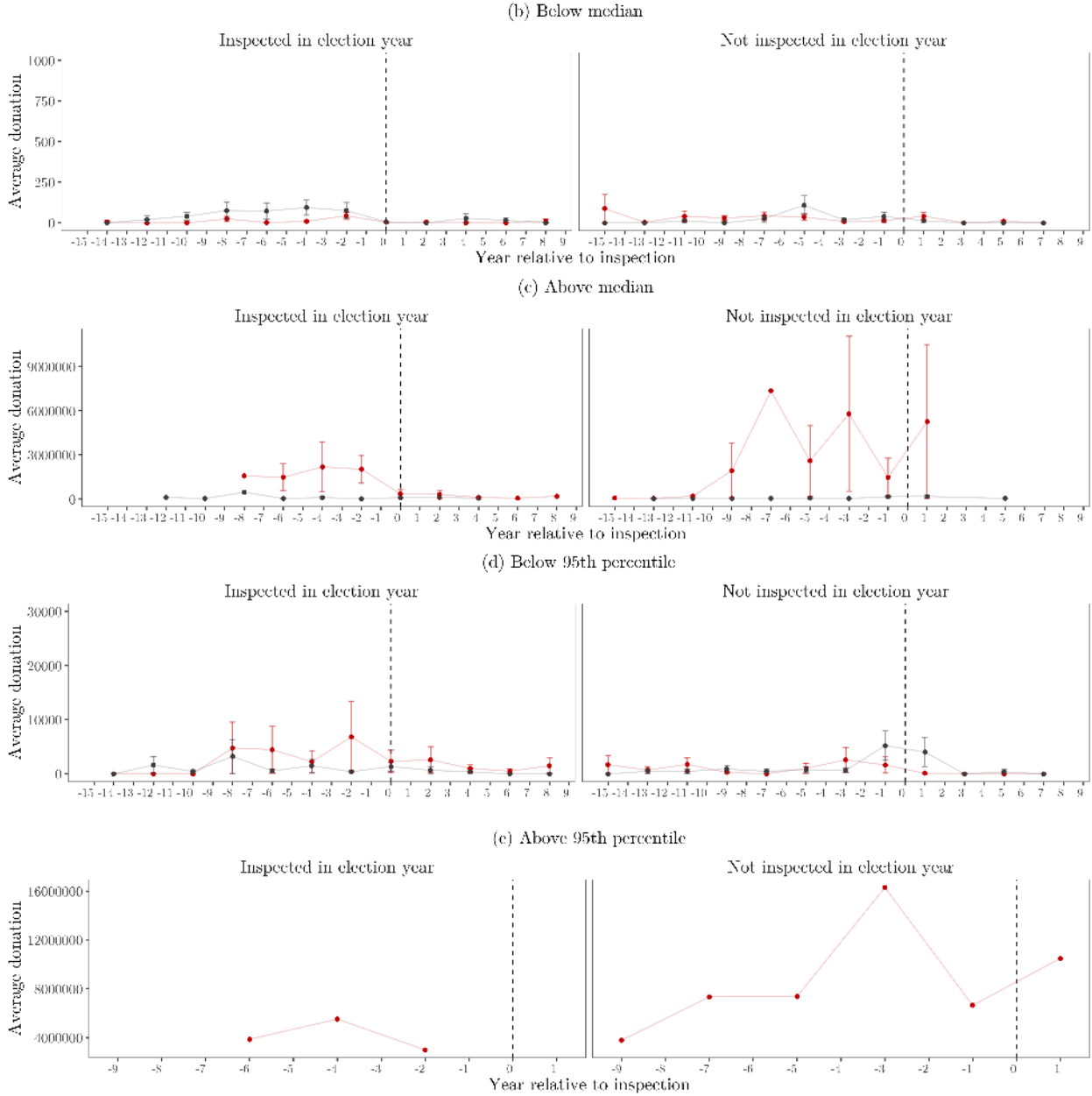


Figure 5: Average Donation Relative to Inspection



On the other hand, the relationship between donations and DL outcome is more straightforward. First, the proportion of traffickers removed from the List in less than 2 years is larger for donors everywhere, except 2019, and such trend persists once I consider those connected to ruralists as depicted in Figure 6. Additionally, it seems that the overall share of traffickers removed early from the DL started decreasing when the DL was suspended. A similar movement

is observed amongst those connected or not to ruralists in panel (b), although there seems to be higher variability within ruralist-connected exploiters. Figure 7 indicates that the average duration a trafficker stays on the List is shorter if there exists a connection to any politician prior to listing, and even shorter if the politician is a ruralist. Notice how the average of days on the List declined over time for donor and non-donors alike and was particularly lower when the List was private.

Figure 6: Off DL in Less than 2 years, by Year of Listing

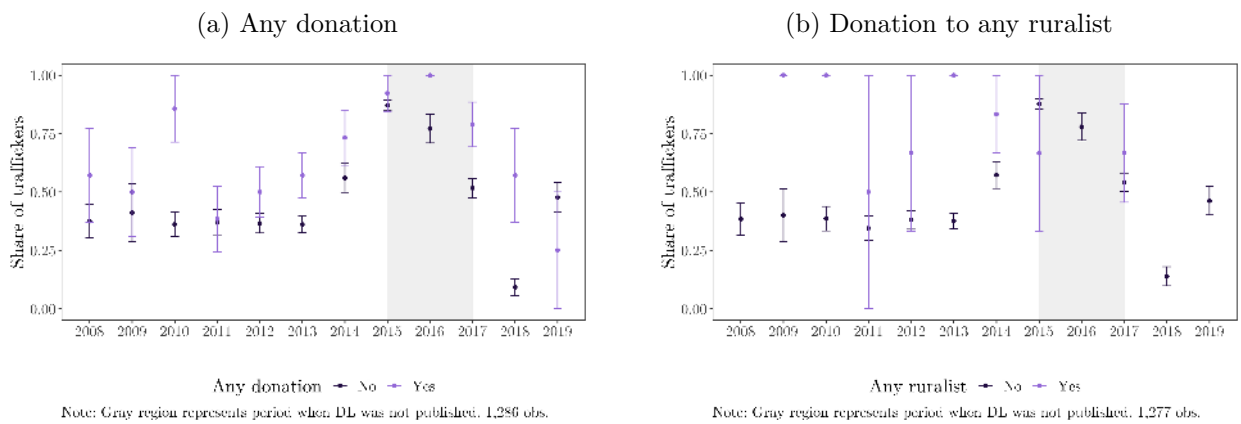
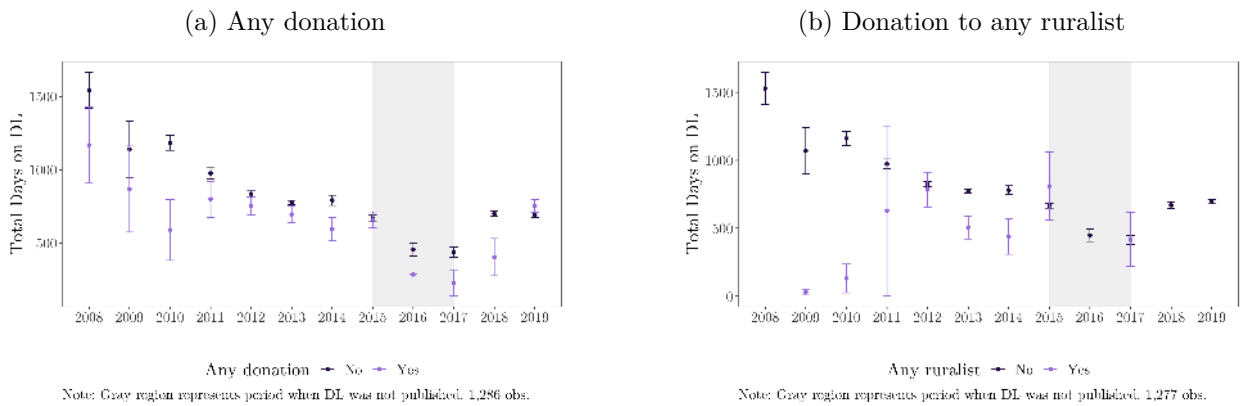


Figure 7: Total Days on the List, by Year of Listing



6.2 Econometric Estimations

Tables 6 through 8 contain the main OLS estimates discussed in this section. For complete results, refer to Tables C.1 through C.5 in Section C.2 of the Appendix.

6.2.1 Extensive Margin: Probability of Being Listed Conditional on Inspection

The influence of a political connection on DL selection is shown in Table 6. Initially, the results do not seem to indicate any benefits of connecting to politicians: column (1) of the preferred specification (broad dummy) indicates that making any contribution is associated with a 90% magnification in the probability of being selected to go on the List (1% significance) and although being linked to a ruralist substantially attenuates this effect, it does not reverse it (24% imprecise increase). Likewise, having a long-term connection is correlated with a raise in the odds of being listed, but to a lesser extent and with high imprecision. These estimates contradict my hypothesis and, more specifically, the role that ruralists play is not entirely consistent across models. For instance, Specification II suggests that giving to one additional candidate leads to a modest but statistically relevant reduction in the chances of winding up listed, but those connected to pro-trafficker legislators are still highly susceptible to being shamed (74% increase).

Table 6: Effect of Political Connection on Likelihood of Listing, Balanced Unconditional Donation

	Listed	
	(1)	(2)
Specification I		
Any donation	0.2709*** (0.0624)	0.3276*** (0.0528)
Any ruralist		-0.2549 (0.2017)
R ²	0.08268	0.08450
RMSE	0.43996	0.43939
F-test	0.02318	0.10548
Specification III		
N candidates = 1	0.3082*** (0.0916)	0.3204*** (0.0490)

N candidates between 2 and 5	0.3882*** (0.0921)	0.4061*** (0.0921)
N candidates > 5	-0.2194* (0.1096)	-0.1707 (0.1375)
Any ruralist		-0.0665 (0.1800)
R ²	0.09010	0.08995
RMSE	0.43817	0.43808
F-test	0.02408	0.10314
Specification IV		
Cum. donation ₅ , arcsinh	0.1036*** (0.0148)	0.1046*** (0.0124)
Cum. donation ₅ , arcsinh square	-0.0070*** (0.0013)	-0.0069*** (0.0010)
Any ruralist		-0.0482 (0.1599)
R ²	0.08814	0.08819
RMSE	0.43865	0.43850
F-test	0.02416	0.10551
Specification V		
Long-term connection	0.0341 (0.1183)	-0.0029 (0.1577)
Any ruralist		0.0617 (0.1976)
R ²	0.06827	0.06814
RMSE	0.44340	0.44330
F-test	0.01884	0.08356
<i>Fixed-effects</i>		
Year of inspection	✓	✓
State of inspection	✓	✓
<i>Controls</i>		
Observations	1,223	1,221
Dependent variable mean	0.30253	0.30221

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Clustered (Year of inspection) standard-errors in parentheses.

Campaign finance in the 5 years prior to inspection, including the year of inspection.

On the other hand, Specifications III and IV suggest the existence of non-constant returns of connections. Giving to more than 5 candidates is associated with a decline of 72.5% in the odds of being put on the DL (21.94 pp significant at the 10% level) and each additional *real* (R\$) donated has increasing marginal benefits (1% significance), but the odds of escaping the DL becomes slightly negative only for donations in the high end of the range of cumulative donation (see Figure 8.) Here, I obtain suggestive evidence that ruralist-connected traffickers experience a 40% advantage in the odds of escaping shaming. Drawing on these results and on the pattern observed in Figure 5, I further investigate the possibility of non-linear benefits to donation with models 3 and 4.

Figure 8: Predicted Values of Prob(Listed|Inspection = 1)

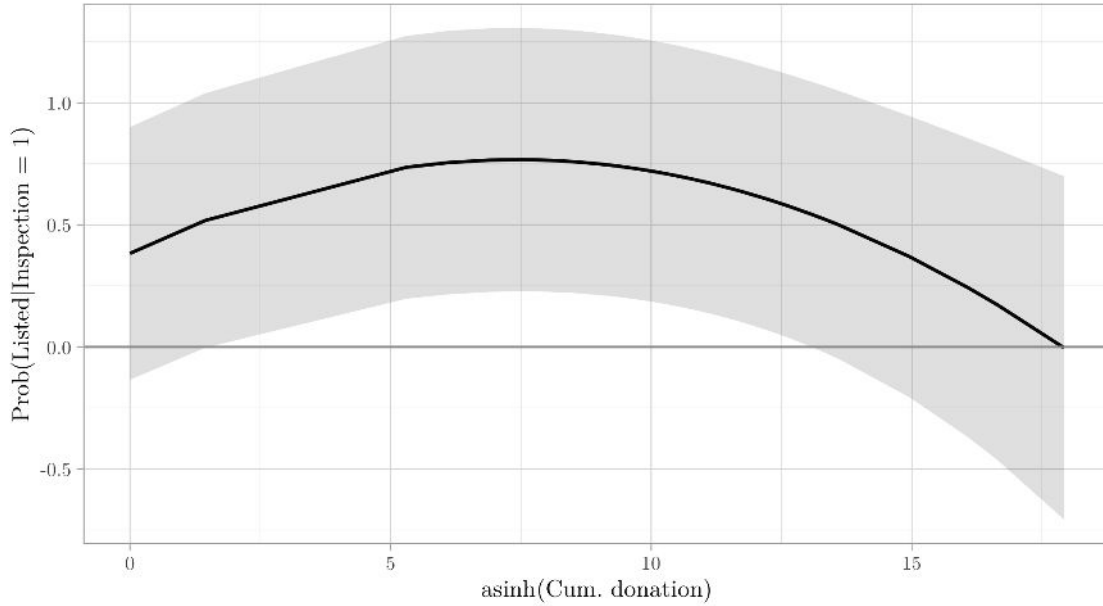
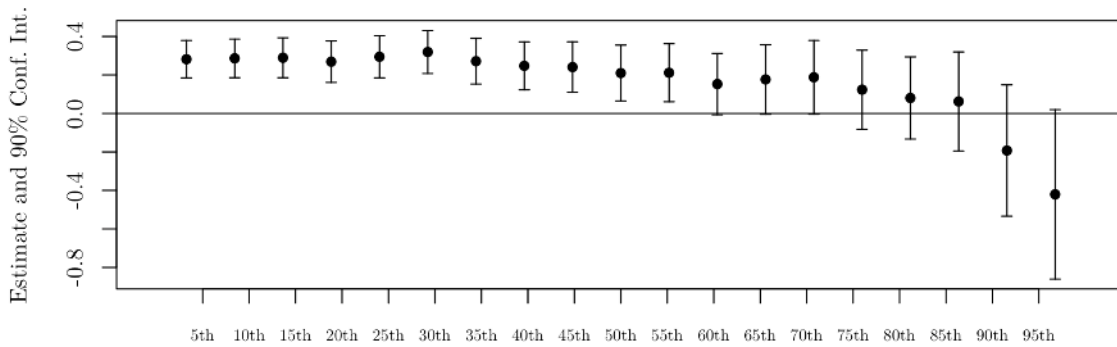


Figure 9 displays the probability of DL selection of donors who give above the X^{th} percentile of contributions. Although critical estimates lose precision (90th and 95th percentiles), the stark trend reversal suggest that only large donors are able to extract benefits from politicians.

Figure 9: Likelihood of Listing per Donation Percentile



My last evaluation of this phenomenon employs a quadratic specification with the percentile dummy using the unbalanced cumulative donation made 8 years leading up to inspection. The results shown in Table 7 corroborate the idea of increasing marginal benefits of cumulative donation

to traffickers without and with controls (columns (1) and (2)) and are qualitatively very similar to Specification IV of Table 6, an indication of robustness to different donation windows. They also remain fairly stable with different percentile dummies, but lose significance when higher percentiles are included.

Table 7: Likelihood of Listing with Quadratic Model and Percentile Dummy, Unbalanced Unconditional Donation

	Listed					
	(1)	(2)	(3)	(4)	(5)	(6)
Cum. donations, arcsinh	0.0884*** (0.0213)	0.0973*** (0.0215)	0.0993*** (0.0222)	0.1087*** (0.0275)	0.0847*** (0.0291)	0.0765*** (0.0286)
Cum. donations, arcsinh square	-0.0054*** (0.0018)	-0.0063*** (0.0018)	-0.0068*** (0.0022)	-0.0077*** (0.0027)	-0.0050* (0.0027)	-0.0042 (0.0026)
Any above 50th _[-8,0]			0.0610 (0.1645)			
Any above 75th _[-8,0]				0.1581 (0.2379)		
Any above 90th _[-8,0]					-0.2173 (0.3394)	
Any above 95th _[-8,0]						-0.4472 (0.4088)
R ²	0.02507	0.09224	0.09235	0.09258	0.09255	0.09314
RMSE	0.45554	0.43957	0.43954	0.43949	0.43949	0.43935
F-test	16.069	2.7256	2.6674	2.6747	2.6739	2.6928
<i>Fixed-effects</i>						
Year of inspection		✓	✓	✓	✓	✓
State of inspection		✓	✓	✓	✓	✓
<i>Fit statistics</i>						
Controls		✓	✓	✓	✓	✓
Observations	1,253	1,253	1,253	1,253	1,253	1,253
Dependent variable mean	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Unbalanced campaign finance in the 8 years prior to inspection, including the year of inspection.

6.2.2 Intensive Margin: Early Dirty List Removal Conditional on Listing

Columns (1) and (4) of Table 8 show that politically connected traffickers face 35% higher odds of early DL removal and stay listed 5.5 fewer months than non-connected traffickers, a reduction of 21.7% relative to the average trafficker (both significant at the 1% level). When a long-term connection exists, these effects skyrocket: connected traffickers are twice as likely to get off the DL early and are kept in the List 22 fewer months (reduction of 84% compared to the mean).

Moreover, columns (3) and (6) provide strong evidence that these effects come mostly from public Lists and remaining estimates indicate that to whom perpetrators connect matters: a ruralist link is associated with considerably larger effects, especially when the List is public. Specifications III and IV also suggest that even though cumulative donations have a small (statistically significant) effect, donating to more than 5 candidates pays off almost twice as much as donating to exactly 1.

Table C.5 shows the same analysis with donation conditional on inspection. Perhaps surprisingly, even though the sample and timeframe is substantially smaller, the influence of political connection is larger: any donation represents an additional 47.7 pp in the odds of getting off the List before 2 years (89.5% increase) and fewer 274 days spent on the List (41.7% reduction), a strong indication that traffickers may indeed pay rents purposefully. In this case, however, connecting to a ruralist harms traffickers, an intriguing result that is consistent across all specifications and contrasts with previous econometric conclusions. Again, effects come mostly from visible lists, whose coefficients are all significant at least at the 10% level. Because the time window is narrower, long-term connections were not present in the data.

Table 8: Effect of Political Connection on Dirty List Outcomes, Balanced Unconditional Donation

	Off DL before 2 years				Total Days on DL			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification I								
Any donation	0.1780*** (0.0368)	0.1428** (0.0497)	0.0385* (0.0191)	0.0399* (0.0195)	-165.4*** (37.80)	-135.0*** (37.42)	-17.84* (9.922)	-15.90 (11.17)
Any ruralist		0.1338 (0.1039)		-0.0095* (0.0051)		-126.9 (114.2)		-16.84 (19.15)
Public list x Any donation			0.1625*** (0.0523)	0.0911 (0.0591)			-173.2*** (47.63)	-117.4** (41.84)
Public list x Any ruralist				0.2897*** (0.0726)				-227.0** (79.88)
R ²	0.17719	0.17962	0.33983	0.34666	0.33304	0.34037	0.48796	0.50085
RMSE	0.45347	0.45281	0.40618	0.40409	354.45	350.97	310.57	305.30
F-test	1.1844	0.80279	1.1325	0.83379	2.7464	1.8920	2.0966	1.5768
Specification V								
Long-term connection	0.5237*** (0.0448)	0.3697*** (0.1008)	-0.0140 (0.0082)	-0.0599*** (0.0076)	-647.9*** (115.3)	-512.4*** (122.2)	-7.871 (18.53)	33.49* (17.21)
Any ruralist		0.1990* (0.1040)		0.0472*** (0.0024)		-160.4 (104.2)		-42.48*** (2.413)
Public list x Long-term conn.			0.5535*** (0.0514)	0.3349*** (0.1012)			-689.8*** (128.0)	-500.0*** (124.4)
Public list x Any ruralist				0.3092*** (0.0843)				-250.2*** (76.93)
R ²	0.16979	0.17554	0.33110	0.34339	0.32896	0.33846	0.48206	0.49892
RMSE	0.45550	0.45394	0.40886	0.40510	355.53	351.48	312.35	305.89
F-test	1.1248	0.78070	1.0890	0.82182	2.6962	1.8759	2.0476	1.5647
<i>Fixed-effects</i>								
DL year	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,286	1,277	1,286	1,277	1,286	1,277	1,286	1,277
Dependent variable mean	0.50933	0.50822	0.50933	0.50822	763.34	761.77	763.34	761.77

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Clustered (DL year) standard-errors in parentheses.

Campaign finance in the 5 years prior to DL publication, including the year of listing.

6.3 Discussion

Together, my results offer strong evidence that political connections diminish the effectiveness of the Dirty List mostly through reduced variable costs of loan restrictions, but they only suppress fixed reputational costs of being listed for big donors.

Although politically connected traffickers are 27% more likely to be listed, meaning that badly behaving firms give more campaign contribution, allowing for non-constant returns to donation leads to a more nuanced conclusion: there seems to exist increasing marginal benefits to political contributions, implying that large donors, arguably the wealthier or more profitable, escape reputational damage altogether. This exempts them from short and long-term repercussions because the Dirty List not only negatively affects listed exploiters for the duration of time they are exposed, but is also meant to persist on the market's memory to curtail exploitative practices via increased scrutiny from business partners, consumers, and legal authorities in the long run.

Once listed, all politically connected perpetrators are more likely to be removed from the DL quicker and are kept in the List 5.5 fewer months than their non-connected counterparts. This validates my hypothesis that traffickers may have an incentive to connect to politicians early and obtain a drastic reduction in the variable costs of punishment, provided that staying on the List for a longer period implies more time without access to credit from public banks. Furthermore, these benefits are distinctly concentrated in public lists, which reaffirms that connections represent a plausible evasion tactic. Because there is a delay between inspection and Dirty List publication (see Figure D.7(a)), traffickers can develop expectations about being named, but anticipating inclusion in a private list does not seem to constitute strong enough incentive to leverage connections. Recall that labor laws and the anti-trafficking agenda were languishing during the years of DL suspension, and the overall perception of DL threat might have faded at that time. Lastly, the assessment of

donations conditional on inspection supplies strong additional evidence that traffickers intentionally seek the help of politicians to avoid DL punishment right after an inspection.

I also provide suggestive indication that to whom traffickers connect matter. My initial hypothesis stated that politicians may want to protect perpetrators, especially if they belong to the rural caucus of Congress. Ruralist-links are associated with an inconsistent effect on likelihood of listing, but they significantly intensify all benefits of connections from unconditional donation 5 years prior to listing (54% higher chances of early removal and 8.7 fewer months on the List). Even though pro-trafficker connection conditional on an inspection is not helpful, my analyses support the view that political disputes around trafficking are not trivial and call for supplementary studies that investigate the role that politician heterogeneity plays in anti-trafficking policy.

Lastly, I show that long-term connections have a considerable impact on post-listing outcomes: they double the odds of getting off the List in less than 2 years and reduces listing time in 22 months. Beyond accounting for the role of connection intensity, I interpret this set of results as confirmation that corrupt or clientelistic practices in Brazil are deeply rooted in historical struggles. Particularly, Table 8 Specification V columns (4) and (8) indicate with statistical significance that ruling elites, such as the rural caucus, are strongly associated with lenient treatment of traffickers. Knowing that paying rents to politicians, especially ruralists, and for a longer period pays off, it is reasonable to speculate that labor traffickers can strategize in advance how they allocate production resources to diminish or avoid altogether the costs of punishment.

Nevertheless, this thesis is not exempt from limitations. First, the proportion of donors in my data is small and even smaller for those who donate to ruralists. This may have two reasons: the inspection sample lacks pre-2010 data, as mentioned, and trafficker donation can be considered a “rare event” compared to DL publication. Note that, in Brazil, campaign donation only happens

around the time of an election, i.e. every two years alternating between local and national ballots, whereas the DL is updated semesterly.

Second, my trafficking flag may considerably undercount the number of traffickers for mechanical and conceptual reasons. One mechanical reason is that I proxy trafficking with the DL, but it could be the case that an inspection with trafficking never results in the listing of perpetrators. Using other sources of data, such as unemployment insurance records and court documents, could help expand the trafficking definition. On a conceptual level, bureaucrat bias may be identified in the decision to inspect and the decision qualify the case as trafficking, as showed in Figure [A.1](#). This means that any other trafficking flag would also be subject to pre-listing and pre-inspection biases, such that my choice to use the DL as a proxy gives, at best, an undercount of unlisted traffickers.

Moreover, measuring political connections with campaign donation is only one way in which these links may be established. Unfortunately, there are no other sources recording politician connections in Brazil, such as family relations. Data collection efforts that expand the scope of this concept would help understand the nuances and mechanisms of how connections benefit traffickers. Particularly, illegal practices such as *caixa dois*, where politicians receive money for their campaigns but do not report it to the government, and *candidatos laranjas*, where “shell candidates” are used to divert electoral funds, could hide potentially important connections. In a similar vein, the ample number of political parties in Brazil and the similarity between a number of them prevented me from exploring partisan aspects in this thesis. I propose that future studies do not look into traditional ideological rankings (left, center, right) but rather on the relative power of legislators in Congress and local administrations (number of seats in the Chamber of Deputies, for example), thematic commissions and parliamentary fronts, and coalitions over time (see Coradini, 2010).

Exploring the network of traffickers – suppliers, subsidiaries, parent firms, and partners – is also relevant in this context given the intertwined nature of exploitation markets. More closely to the agenda of this thesis, understanding how the prohibition of campaign finance from firms in 2015 affected donation decisions is pertinent and such shock may allow an investigation of how substitutable donations and other evasion tactics are. In addition, I only study trafficker outcomes with relation to the Dirty List, but other sanctions, such as size of levied fines, payment of labor compensation, prosecution, and other penalties, were not evaluated. Understanding why some traffickers are punished along other dimensions or not is equally important for an efficient anti-trafficking agenda.

Lastly, I cannot claim causality over my estimates because the decision to donate to any candidate is possibly endogenous. Yet my results identify important relationships, submit relevant recommendations for future research, and brings attention to an understudied and overlooked topic among academics and policymakers.

7 Conclusion

Albeit an important tool in the combat against human trafficking in Brazil, the Dirty List of Slave Labor had never been subject to analysis. This thesis is the first attempt to study its outcomes and assess the extent to which political connection of traffickers interact with the List. I find sizable effects offering evidence that politically connected traffickers spend less time on the DL, but are not less likely to be listed conditional on an inspection unless they are large donors. I find suggestive evidence that some of these effects are exacerbated when the politician is a ruralist, donations display increasing marginal benefits, and there is strong indication that connections are used as hedging strategy against reputational damage. The data suggest that outcomes post-inspection

and post-inclusion in the List may be subject to clientelism and corruption via campaign financing that deserve policymakers' attention. In other words, traffickers' impunity seem to have a price.

Eliminating decision bias from the DL process is a challenging policy task, but illuminating what factors could be influencing bureaucrats' decision-making may lead to the design of incentives that diminish arbitrariness. The results in this thesis are not negligible and may be used to advance at least three policy and institutional modifications. First, campaign funding eligibility should be amended to eliminate dirty money from the democratic process. Second, early removal decisions should be audited. And third, politician relationship with labor traffickers should be better understood and investigated.

The current scant knowledge of labor traffickers' relationship with government institutions, regulatory agencies, and incumbents suggest that the mechanisms, incentives, and dynamics of human trafficking will remain in force and continue affecting millions of people worldwide. Dedicating research efforts to disentangle the *modus operandi* of perpetrators will enable analysts and decision-makers to better predict their behavior and tackle trafficking more effectively.

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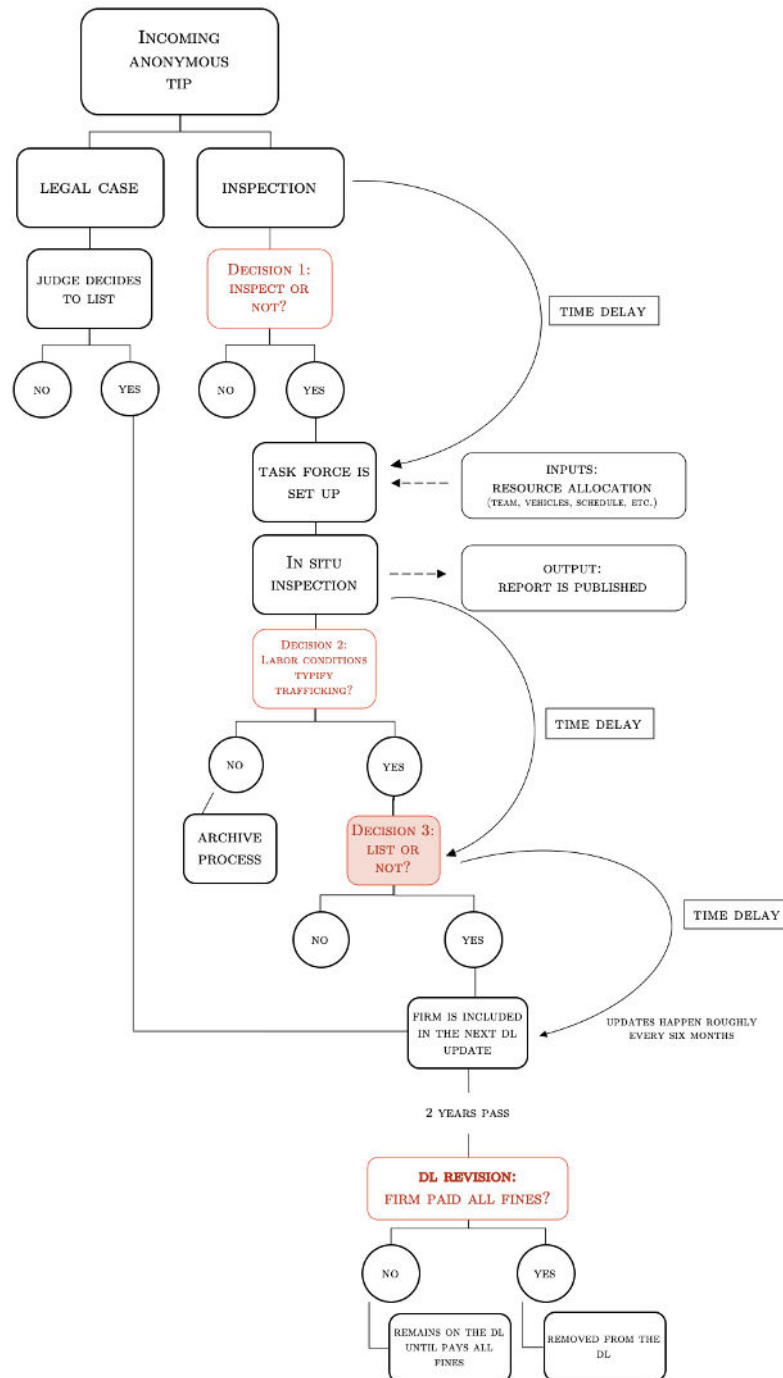
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Appendices

A Figures

Figure A.1: Dirty List Process



Source: Author's production.

Note: Several key decisions in the DL process are subject to selection bias. In this thesis, I focus on Decision 3, where a bureaucrat will decide to blacklist or not a trafficker. Data to analyze Decisions 1 and 2 are not yet available.

Figure A.2: Dirty List: Snapshot

Cadastro de Empregadores que tenham submetido trabalhadores a condições análogas à de escravo

(Portaria Interministerial MTPS/MMIRDH nº 4 de 11/05/2016)



Atualização periódica de 5 de abril 2022. Cadastro atualizado em 5/4/2022.									
I- PUBLICAÇÃO DO CADASTRO DE EMPREGADORES PREVISTA NO ARTIGO 2º, CAPUT, DA PORTARIA INTERMINISTERIAL MTPS/MMIRDH Nº 4, DE 11 DE MAIO DE 2016									
ID	Ano da ação fiscal	UF	Empregador	CNPJ/CPF	Estabelecimento	Trabalhadores envolvidos	CNAE	Decisão administrativa de procedência (irrecorribilidade)	Inclusão no Cadastro de Empregadores
1	2019	PB	A I Maciel Mineração	12.985.350/0001-33	Sítio Olho D'Água da Viração, Zona Rural, Salgadinho/PB	12	2399-199	02/12/2019	05/10/2020
2	2018	BA	Adilson Bona Vieira	088.062.627-50	Fazenda Dois Rios e Dois Rios I - Rodovia BA-283, Km 11, Partido de Caraiva, 16 Km, Zona Rural, Porto Seguro/BA	39	0134-200	27/02/2020	05/04/2021
3	2019	MG	Agrícola Minas Norte Ltda.	02.387.202/0001-63	Fazenda Agrícola Minas Norte, Rod. BR-365, km 214 acesso, 13 km, Zona Rural, Buntzeiro/MG	46	1199-005	08/01/2021	05/10/2021
4	2019	MG	Agrocol - Agropecuária, Comércio e Participações Ltda.	08.691.264/0001-40	Agrocol, Rodovia BR-050, próximo ao km 158, sentido Uberaba-Uberlândia, Uberaba/MG	6	2101-001	06/10/2021	05/04/2022
5	2019	PR	Agroflorestal Schultz Ltda.	26.396.600/0001-04	Linha Santa Terezinha (Papuã), Município de Bituruna/PR	5	0139-302	30/12/2019	05/10/2020
6	2018	BA	Agropecuária Vallas Ltda.	06.202.274/0001-40	Fazenda Agropecuária Vallas, Quadra das Acácias, BR-242, Km 735, Angical/BA	3	0210-107	27/02/2020	05/04/2021
7	2019	MG	Angelo Gualberto de Macedo	681.055.846-20	Fazenda da Onça/Altoleiro, Zona Rural, Piumhi/MG	10	2101-007	24/02/2021	05/10/2021
8	2019	MG	Antonio Lara de Moraes	010.405.536-72	Fazenda Nova Fronteira, Bairro Congonhas, Zona Rural, Jacutinga/MG	4	1342-000	03/11/2021	05/04/2022
9	2019	MT	Antônio Leuar Mascarello	005.878.939-15	Fazenda Santa Rita, Rodovia MT-422, km 100, Zona Rural, Santa Carmem/MT	7	1156-000	15/09/2021	05/04/2022
10	2017	BA	Arlinda Pinheiro de Souza Santos	109.459.975-15	Rua Moisés Santos, nº 44, Centro, Elísio Medrado/BA	1	9700-500	22/11/2021	05/04/2022
11	2018	MG	Alanael dos Santos	853.388.776-00	Sítio Segredo, Estrada Cristais Fernandes, km 4,6 Segredo, Zona Rural de Cristais/MG	2	0134-200	21/10/2021	05/04/2022
12	2020	TO	Augusto Gralão	015.927.421-49	Fazenda Gralão, Rodovia BR-153, sentido Araguaína-Nova Olinda, após o restaurante Sabores de Mel, primeira entrada à direita, Nova Olinda/TO	5	0151-201	13/09/2021	05/04/2022
13	2018	MT	Carlos Alberto dos Reis Dias	174.781.201-30	Galpão com maquinário para produção de tijolos, Rua Benedito Fortes, S/N, Nossa Senhora do Livramento/MT	1	2342-702	22/01/2020	05/04/2022

Source: Ministry of Labor (Brasil, 2022).

Note: Last update of the Dirty List on April 5, 2022.

B Descriptive Statistics

B.1 Plots

Figure B.1: Density of Raw and Transformed Cumulative Donation in Reais

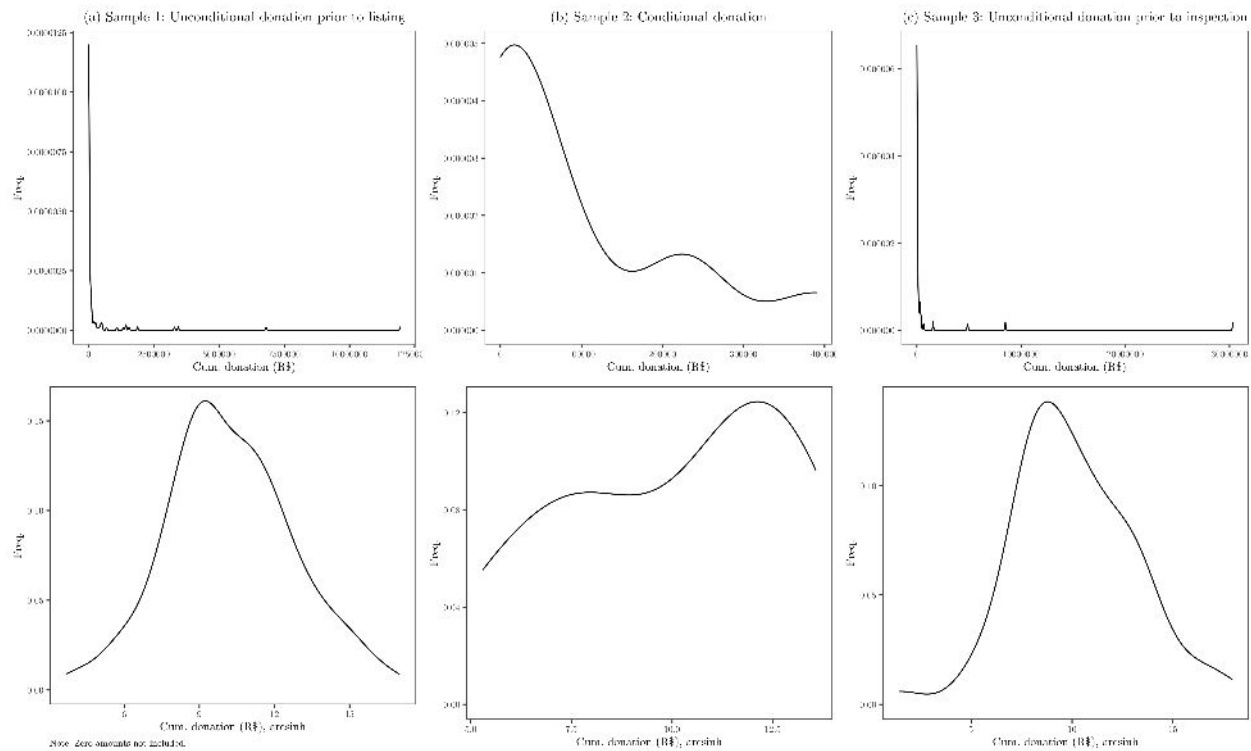
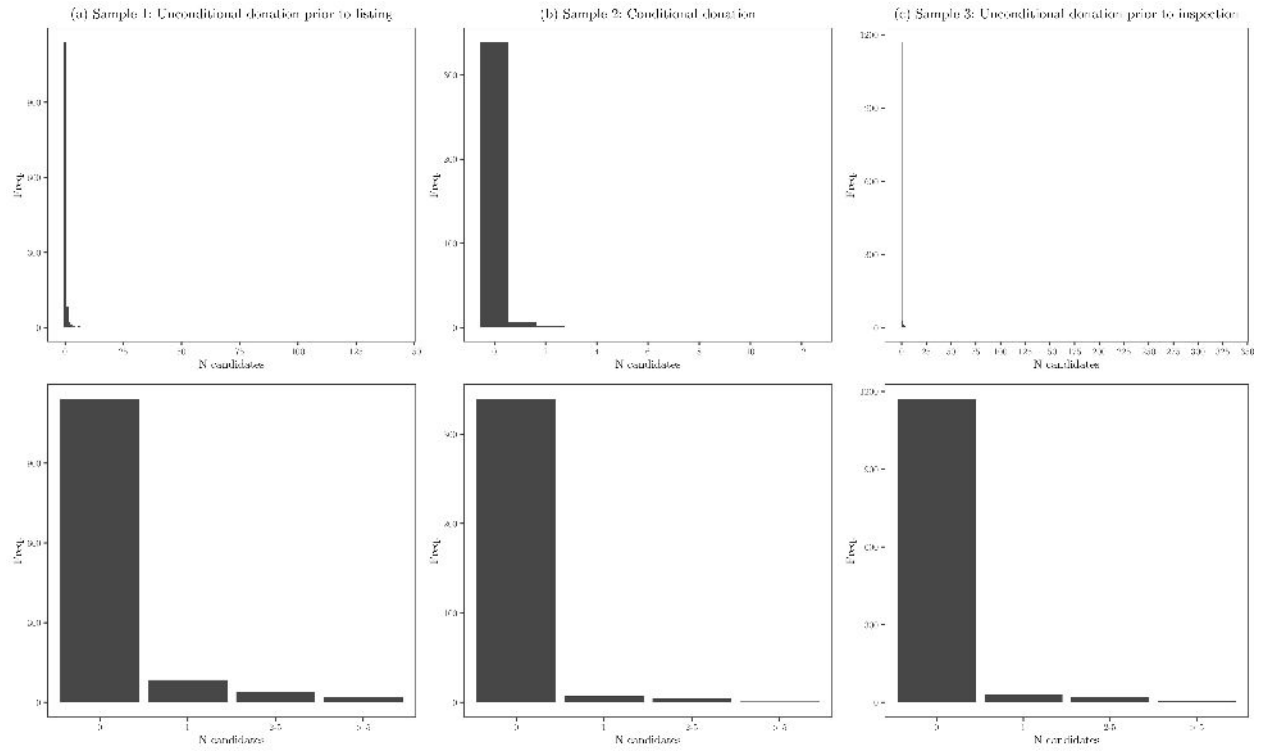


Figure B.2: Distribution of Candidates per Trafficker



B.2 Tables

Table B.1: Descriptive Statistics (Cont.)

Variable	Sample 4		
	Mean	Std. Dev.	Obs
<i>Political Connection Measurements</i>			
Any above 5th _[-8,0]	0.05	0.22	1253
Any above 10th _[-8,0]	0.047	0.21	1253
Any above 15th _[-8,0]	0.044	0.2	1253
Any above 20th _[-8,0]	0.041	0.2	1253
Any above 25th _[-8,0]	0.039	0.19	1253
Any above 30th _[-8,0]	0.038	0.19	1253
Any above 35th _[-8,0]	0.034	0.18	1253
Any above 40th _[-8,0]	0.03	0.17	1253
Any above 45th _[-8,0]	0.027	0.16	1253
Any above 50th _[-8,0]	0.022	0.15	1253
Any above 55th _[-8,0]	0.021	0.14	1253
Any above 60th _[-8,0]	0.018	0.13	1253
Any above 65th _[-8,0]	0.014	0.12	1253
Any above 70th _[-8,0]	0.013	0.11	1253
Any above 75th _[-8,0]	0.011	0.11	1253
Any above 80th _[-8,0]	0.01	0.1	1253
Any above 85th _[-8,0]	0.0072	0.084	1253
Any above 90th _[-8,0]	0.004	0.063	1253
Any above 95th _[-8,0]	0.0024	0.049	1253
Trafficker			
Listed		✓	
Unlisted		✓	
Donation			
Unconditional pre-inspection		✓	
Balanced Panel			

C Results

C.1 Graphical Analysis

Figure C.1: Average Donation Relative to Inspection

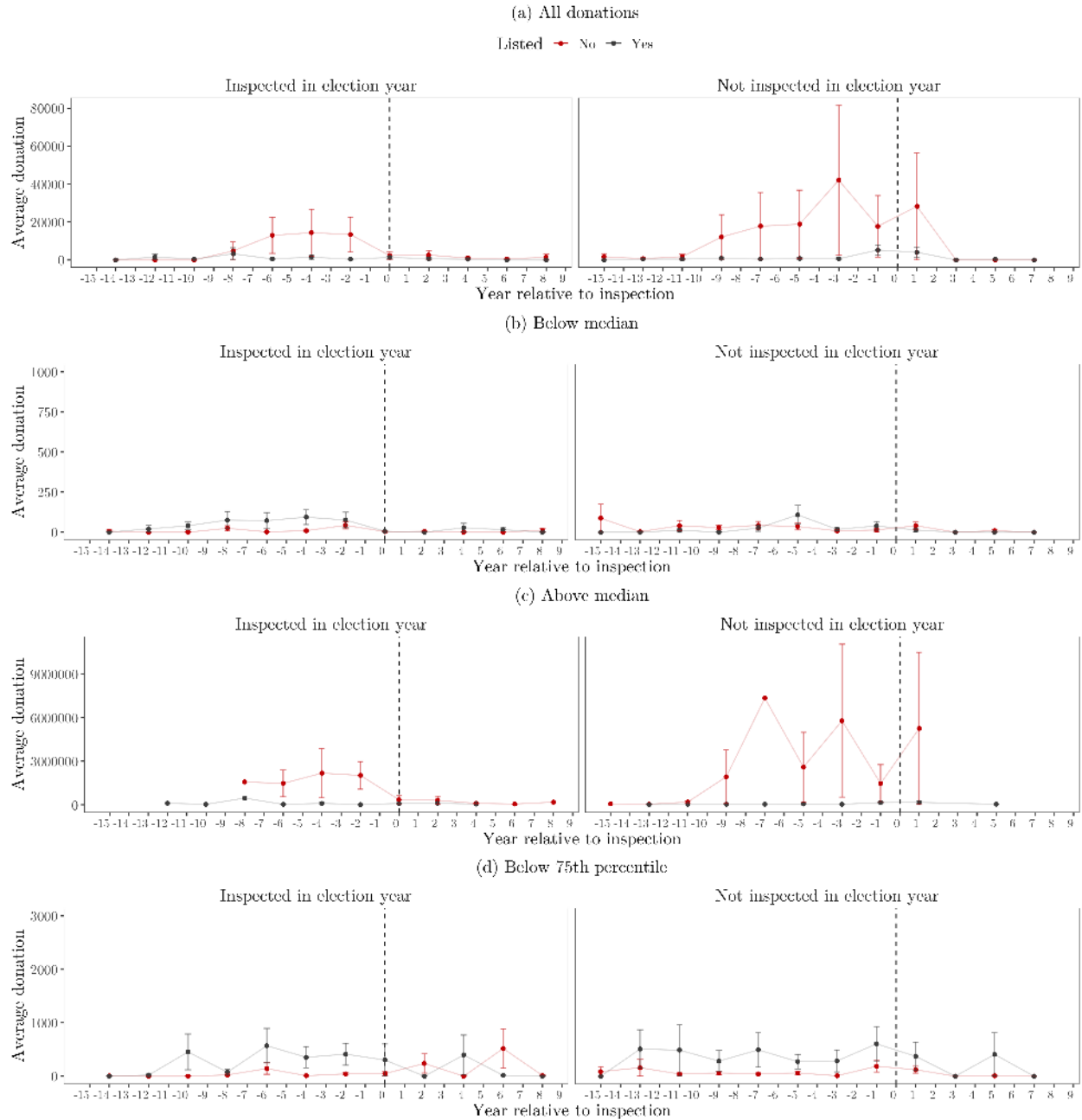


Figure C.1: Average Donation Relative to Inspection

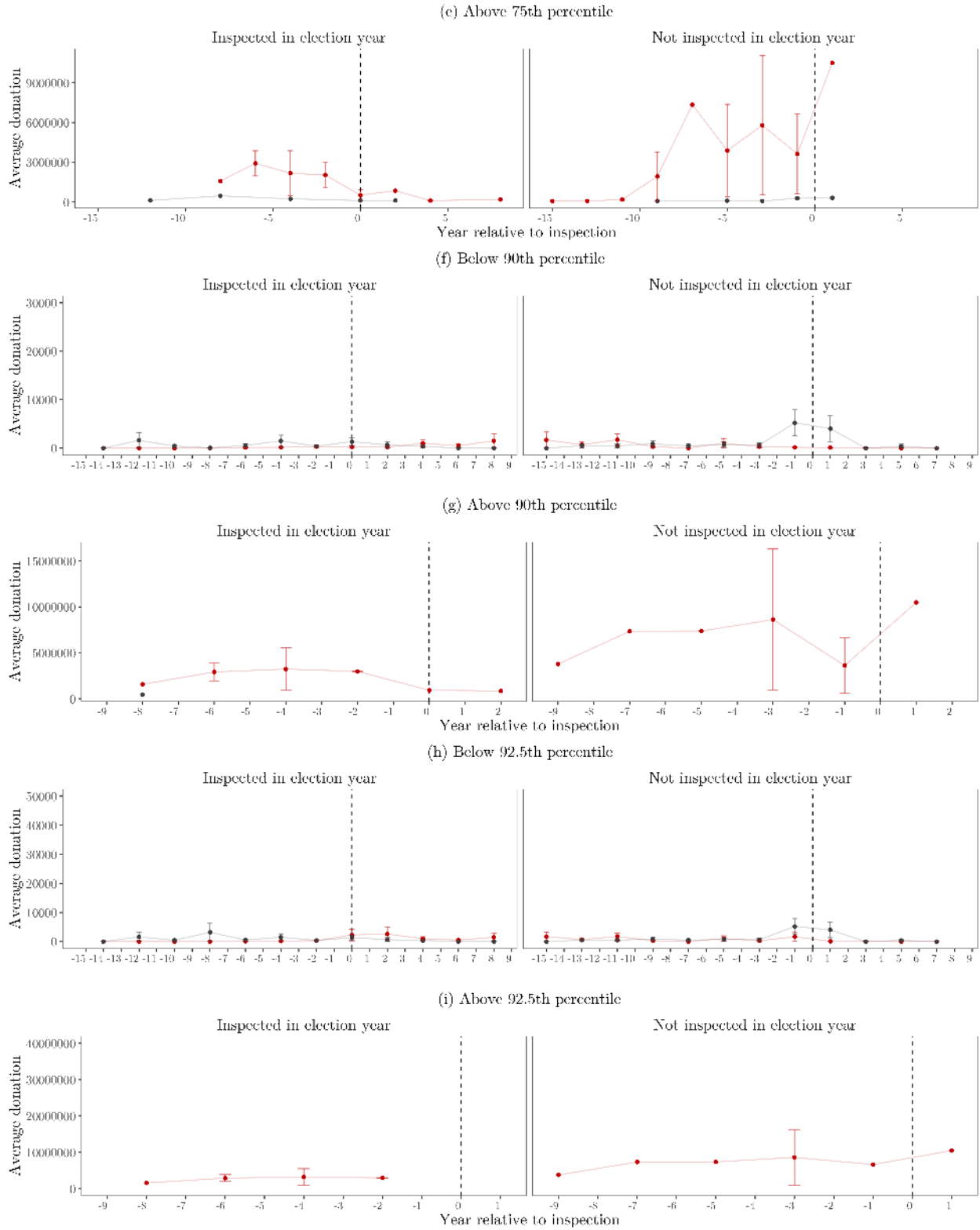
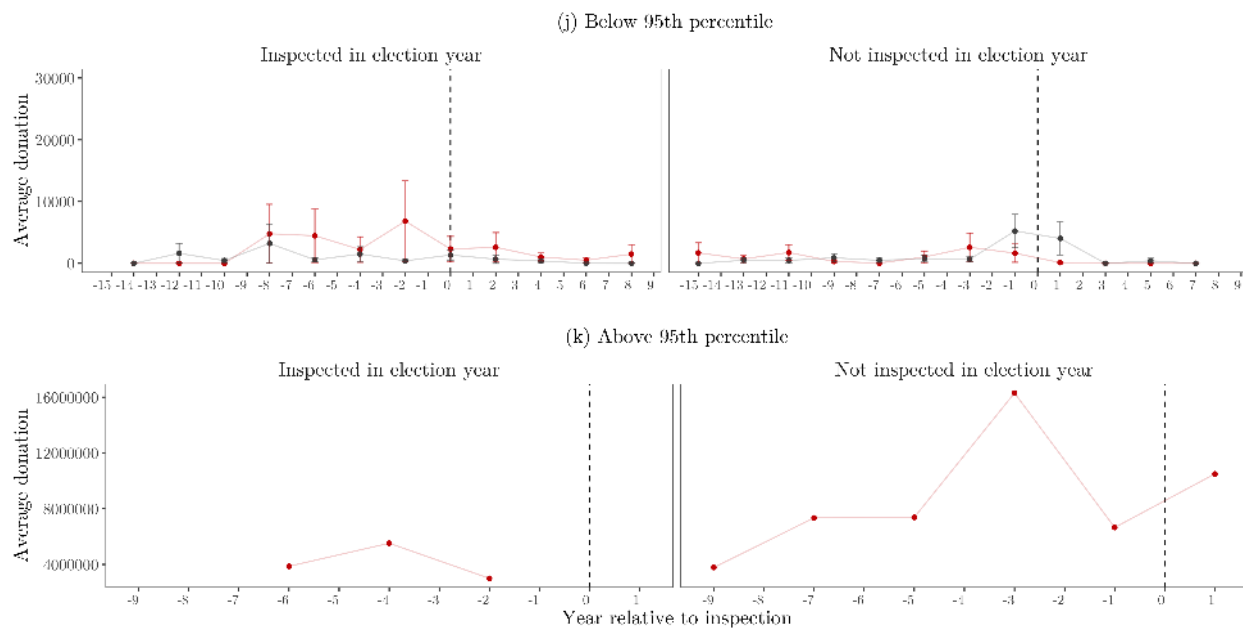


Figure C.1: Average Donation Relative to Inspection



C.2 Tables

Table C.1: Effect of Political Connection on Likelihood of Listing, Balanced Unconditional Donation

	Listed	
	(1)	(2)
Specification I		
Any donation	0.2709*** (0.0624)	0.3276*** (0.0528)
Any ruralist		-0.2549 (0.2017)
R ²	0.08268	0.08450
RMSE	0.43996	0.43939
F-test	0.02318	0.10548
Specification II		
N candidates	-0.0015*** (0.0003)	-0.0025*** (0.0009)
Any ruralist		0.2265 (0.2135)
R ²	0.06974	0.07102
RMSE	0.44305	0.44261
F-test	0.01928	0.08737
Specification III		
N candidates = 1	0.3082*** (0.0916)	0.3204*** (0.0490)
N candidates between 2 and 5	0.3882*** (0.0921)	0.4061*** (0.0921)
N candidates > 5	-0.2194* (0.1096)	-0.1707 (0.1375)
Any ruralist		-0.0665 (0.1800)
R ²	0.09010	0.08995
RMSE	0.43817	0.43808
F-test	0.02408	0.10314
Specification IV		
Cum. donation ₅ , arcsinh	0.1036*** (0.0148)	0.1046*** (0.0124)
Cum. donation ₅ , arcsinh square	-0.0070*** (0.0013)	-0.0069*** (0.0010)
Any ruralist		-0.0482 (0.1599)
R ²	0.08814	0.08819
RMSE	0.43865	0.43850
F-test	0.02416	0.10551
Specification V		
Long-term connection	0.0341 (0.1183)	-0.0029 (0.1577)

Any ruralist		0.0617 (0.1976)
<hr/>		
R ²	0.06827	0.06814
RMSE	0.44340	0.44330
F-test	0.01884	0.08356
<hr/>		
<i>Fixed-effects</i>		
Year of inspection	✓	✓
State of inspection	✓	✓
<hr/>		
Controls	✓	✓
Observations	1,223	1,221
Dependent variable mean	0.30253	0.30221
<hr/> <hr/>		

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Clustered (Year of inspection) standard-errors in parentheses.

Campaign finance in the 5 years prior to inspection, including the year of inspection.

Table C.2: Likelihood of Listing with Percentile Dummy, Unbalanced Unconditional Donation

	Listed								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any above 5th perc. _[-8,0]	0.2818*** (0.0591)								
Any above 10th perc. _[-8,0]		0.2863*** (0.0609)							
Any above 15th perc. _[-8,0]			0.2896*** (0.0630)						
Any above 20th perc. _[-8,0]				0.2688*** (0.0655)					
Any above 25th perc. _[-8,0]					0.2945*** (0.0666)				
Any above 30th perc. _[-8,0]						0.3192*** (0.0680)			
Any above 35th perc. _[-8,0]							0.2715*** (0.0721)		
Any above 40th perc. _[-8,0]								0.2473*** (0.0756)	
Any above 45th perc. _[-8,0]									0.2407*** (0.0799)
R ²	0.08534	0.08488	0.08414	0.08096	0.08298	0.08483	0.07896	0.07632	0.07508
RMSE	0.44123	0.44135	0.44152	0.44229	0.44180	0.44136	0.44277	0.44341	0.44370
F-test	2.5617	2.5465	2.5223	2.4185	2.4843	2.5449	2.3535	2.2683	2.2287
<i>Fixed-effects</i>									
Year of Inspection	✓	✓	✓	✓	✓	✓	✓	✓	✓
State of Inspection	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,253	1,253	1,253	1,253	1,253	1,253	1,253	1,253	1,253
Dependent variable mean	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Unbalanced campaign finance in the 8 years prior to inspection, including the year of inspection.

Table C.2 Cont.: Likelihood of Listing with Percentile Dummy, Unbalanced Unconditional Donation

	Listed									
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)
Any above 50th perc. _[-8,0]	0.2099** (0.0881)									
Any above 55th perc. _[-8,0]		0.2114** (0.0915)								
Any above 60th perc. _[-8,0]			0.1525 (0.0971)							
Any above 65th perc. _[-8,0]				0.1766 (0.1098)						
Any above 70th perc. _[-8,0]					0.1879 (0.1163)					
Any above 75th perc. _[-8,0]						0.1235 (0.1248)				
Any above 80th perc. _[-8,0]							0.0804 (0.1298)			
Any above 85th perc. _[-8,0]								0.0623 (0.1565)		
Any above 90th perc. _[-8,0]									-0.1928 (0.2075)	
Any above 95th perc. _[-8,0]										-0.4205 (0.2676)
R ²	0.07249	0.07224	0.07004	0.07013	0.07014	0.06889	0.06843	0.06826	0.06880	0.07004
RMSE	0.44432	0.44438	0.44491	0.44489	0.44488	0.44518	0.44529	0.44534	0.44521	0.44491
F-test	2.1457	2.1378	2.0676	2.0705	2.0711	2.0313	2.0168	2.0113	2.0285	2.0676
<i>Fixed-effects</i>										
Year of Inspection	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State of Inspection	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
<i>Fit statistics</i>										
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,253	1,253	1,253	1,253	1,253	1,253	1,253	1,253	1,253	1,253
Dependent variable mean	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Unbalanced campaign finance in the 8 years prior to inspection, including the year of inspection.

Table C.3: Likelihood of Listing with Quadratic Model and Percentile Dummy, Unbalanced Unconditional Donation

	Listed					
	(1)	(2)	(3)	(4)	(5)	(6)
Cum. donation ₈ , arcsinh	0.0884*** (0.0213)	0.0973*** (0.0215)	0.0993*** (0.0222)	0.1087*** (0.0275)	0.0847*** (0.0291)	0.0765*** (0.0286)
Cum. donation ₈ , arcsinh square	-0.0054*** (0.0018)	-0.0063*** (0.0018)	-0.0068*** (0.0022)	-0.0077*** (0.0027)	-0.0050* (0.0027)	-0.0042 (0.0026)
Any above 50th _[-8,0]			0.0610 (0.1645)			
Any above 75th _[-8,0]				0.1581 (0.2379)		
Any above 90th _[-8,0]					-0.2173 (0.3394)	
Any above 95th _[-8,0]						-0.4472 (0.4088)
R ²	0.02507	0.09224	0.09235	0.09258	0.09255	0.09314
RMSE	0.45554	0.43957	0.43954	0.43949	0.43949	0.43935
F-test	16.069	2.7256	2.6674	2.6747	2.6739	2.6928
<i>Fixed-effects</i>						
Year of inspection		✓	✓	✓	✓	✓
State of inspection		✓	✓	✓	✓	✓
<i>Fit statistics</i>						
Controls		✓	✓	✓	✓	✓
Observations	1,253	1,253	1,253	1,253	1,253	1,253
Dependent variable mean	0.30726	0.30726	0.30726	0.30726	0.30726	0.30726

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Unbalanced campaign finance in the 8 years prior to inspection, including the year of inspection.

Table C.4: Effect of Political Connection on Dirty List Outcomes, Balanced Unconditional Donation

	Off DL before 2 years				Total Days on DL			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification I								
Any donation	0.1780*** (0.0368)	0.1428** (0.0497)	0.0385* (0.0191)	0.0399* (0.0195)	-165.4*** (37.80)	-135.0*** (37.42)	-17.84* (9.922)	-15.90 (11.17)
Any ruralist		0.1338 (0.1039)		-0.0095* (0.0051)		-126.9 (114.2)		-16.84 (19.15)
Public list x Any donation			0.1625*** (0.0523)	0.0911 (0.0591)			-173.2*** (47.63)	-117.4** (41.84)
Public list x Any ruralist				0.2897*** (0.0726)				-227.0** (79.88)
R ²	0.17719	0.17962	0.33983	0.34666	0.33304	0.34037	0.48796	0.50085
RMSE	0.45347	0.45281	0.40618	0.40409	354.45	350.97	310.57	305.30
F-test	1.1844	0.80279	1.1325	0.83379	2.7464	1.8920	2.0966	1.5768
Specification II								
N candidates	0.0059*** (0.0018)	0.0026* (0.0013)	0.0020 (0.0016)	0.0019 (0.0022)	-9.112*** (1.748)	-6.319*** (1.461)	-1.424 (1.040)	1.069 (2.387)
Any ruralist		0.2286** (0.1007)		0.0036 (0.0304)		-165.4 (113.9)		-45.16 (35.43)
Public list x N candidates			0.0045 (0.0031)	-0.0003 (0.0024)			-8.229*** (2.475)	-6.561** (2.713)
Public list x Any ruralist				0.3743*** (0.0858)				-246.7** (97.94)
R ²	0.16768	0.17385	0.33006	0.34254	0.32790	0.33654	0.48222	0.49773
RMSE	0.45608	0.45440	0.40918	0.40536	355.81	351.99	312.30	306.26
F-test	1.1081	0.77158	1.0839	0.81874	2.6834	1.8599	2.0489	1.5572
Specification III								
N candidates = 1	0.1652*** (0.0516)	0.1540** (0.0599)	0.0445 (0.0259)	0.0449 (0.0261)	-112.2** (41.41)	-114.0** (41.50)	-26.72* (12.37)	-26.98* (12.54)
N candidates between 2-5	0.1545* (0.0748)	0.1131 (0.0911)	0.0329*** (0.0099)	0.0269** (0.0104)	-180.5** (72.70)	-153.7** (69.66)	2.159 (14.56)	12.51 (23.98)
N candidates > 5	0.2763** (0.1075)	0.1579 (0.1645)	0.0109 (0.0115)	-0.0296* (0.0157)	-359.7** (127.9)	-325.0** (124.4)	-19.34 (14.48)	51.24* (25.73)
Any ruralist		0.1321 (0.1369)		0.0405*** (0.0088)		-25.49 (124.0)		-70.16** (28.32)
Public list x N candidates = 1			0.1192* (0.0650)	0.0928 (0.0711)			-80.40 (47.18)	-72.46 (40.48)
Public list x N candidates between 2-5			0.1515 (0.0931)	0.0810 (0.1123)			-227.5** (87.01)	-183.0* (92.58)

Public list x N candidates > 5			0.3606** (0.1167)	0.1921 (0.1512)			-431.5*** (128.6)	-394.6*** (112.7)
Public list x Any ruralist				0.2285** (0.0773)				-63.39 (80.04)
R ²	0.17791	0.17977	0.34181	0.34677	0.33719	0.34229	0.49520	0.50329
RMSE	0.45327	0.45277	0.40557	0.40406	353.35	350.46	308.37	304.56
F-test	0.59512	0.48216	0.63473	0.53087	1.3990	1.1449	1.1990	1.0132
Specification IV								
Cum. donation ₅ , arcsinh	0.0183*** (0.0036)	0.0155** (0.0051)	0.0034* (0.0018)	0.0037* (0.0020)	-17.85*** (4.261)	-15.68*** (4.299)	-1.582 (0.9328)	-1.371 (1.122)
Any ruralist		0.0791 (0.1198)		-0.0188* (0.0092)		-62.60 (118.0)		-14.37 (21.85)
Public list x Cum. donation ₅ , arcsinh			0.0185*** (0.0050)	0.0117* (0.0060)			-19.95*** (5.077)	-15.29** (4.994)
Public list x Any ruralist				0.2360** (0.0883)				-153.1* (83.32)
R ²	0.17931	0.18052	0.34393	0.34808	0.33718	0.34274	0.49451	0.50391
RMSE	0.45288	0.45256	0.40492	0.40365	353.35	350.34	308.58	304.37
F-test	1.2016	0.80774	1.1533	0.83902	2.7978	1.9120	2.1522	1.5962
Specification V								
Long-term connection	0.5237*** (0.0448)	0.3697*** (0.1008)	-0.0140 (0.0082)	-0.0599*** (0.0076)	-647.9*** (115.3)	-512.4*** (122.2)	-7.871 (18.53)	33.49* (17.21)
Any ruralist		0.1990* (0.1040)		0.0472*** (0.0024)		-160.4 (104.2)		-42.48*** (2.413)
Public list x Long-term connection			0.5535*** (0.0514)	0.3349*** (0.1012)			-689.8*** (128.0)	-500.0*** (124.4)
Public list x Any ruralist				0.3092*** (0.0843)				-250.2*** (76.93)
R ²	0.16979	0.17554	0.33110	0.34339	0.32896	0.33846	0.48206	0.49892
RMSE	0.45550	0.45394	0.40886	0.40510	355.53	351.48	312.35	305.89
F-test	1.1248	0.78070	1.0890	0.82182	2.6962	1.8759	2.0476	1.5647
<i>Fixed-effects</i>								
DL year	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,286	1,277	1,286	1,277	1,286	1,277	1,286	1,277
Dependent variable mean	0.50933	0.50822	0.50933	0.50822	763.34	761.77	763.34	761.77

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Clustered (DL year) standard-errors in parentheses.

Campaign finance in the 5 years prior to DL publication, including the year of listing.

Table C.5: Effect of Political Connection on Dirty List Outcomes, Donation Conditional on Inspection

	Off DL before 2 years				Total Days on DL			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification I								
Any donation	0.4770** (0.1776)	0.5636*** (0.1290)	-0.0007 (0.0004)	0.0190 (0.0109)	-274.0*** (77.35)	-308.1*** (76.94)	1.947*** (0.2229)	-9.450 (5.935)
Any ruralist		-0.3491 (0.2220)		-0.0592 (0.0339)		137.5* (68.03)		34.23* (18.46)
Public list x Any donation			0.4942* (0.2376)	0.5736*** (0.1609)			-235.5** (93.21)	-267.0** (94.43)
Public list x Any ruralist				-0.3947 (0.3305)				161.8** (67.66)
R ²	0.30194	0.30501	0.42928	0.43289	0.43631	0.43734	0.66969	0.67116
RMSE	0.41685	0.41594	0.37692	0.37572	253.92	253.69	194.37	193.94
F-test	2.8116	1.9017	1.9557	1.4176	5.0312	3.3682	5.2714	3.7905
Specification II								
N candidates	0.0555 (0.0339)	0.1311** (0.0478)	-0.0031 (0.0018)	0.0190 (0.0109)	-37.87*** (8.283)	-78.93*** (19.79)	1.952* (0.9913)	-9.450 (5.935)
Any ruralist		-0.7226** (0.3108)		-0.2684 (0.1537)		392.0*** (126.0)		138.2 (83.75)
Public list x N candidates			0.1154 (0.1086)	0.3544* (0.1776)			-52.66* (28.08)	-147.7* (70.27)
Public list x Any ruralist				-1.275* (0.6001)				491.3 (305.4)
R ²	0.27975	0.28441	0.41346	0.42669	0.42236	0.42535	0.66167	0.66646
RMSE	0.42343	0.42205	0.38211	0.37777	257.04	256.38	196.72	195.32
F-test	2.5246	1.7222	1.8328	1.3822	4.7527	3.2075	5.0847	3.7108
Specification III								
N candidates = 1	0.6697*** (0.0817)	0.6698*** (0.0818)	0.0190 (0.0109)	0.0190 (0.0109)	-378.0*** (89.50)	-378.0*** (89.61)	-9.450 (5.935)	-9.450 (5.943)
N candidates between 2-5	0.1549 (0.4103)	0.1821 (0.3822)	0.1566 (0.4151)	0.1860 (0.3814)	-66.19 (77.58)	-55.88 (74.19)	-65.35 (79.10)	-56.66 (73.19)
N candidates > 5	0.3657*** (0.0344)	0.4198*** (0.0336)	-0.0401 (0.0230)	0.0187 (0.0746)	-342.7*** (23.44)	-322.2*** (8.735)	24.78* (12.53)	42.18 (24.22)
Any ruralist		-0.0544 (0.0607)		-0.0589 (0.0737)		-20.65 (15.05)		-17.40 (22.44)
Public list x N candidates = 1			0.7341*** (0.0557)	0.7342*** (0.0559)			-353.8** (124.4)	-353.7** (124.5)
R ²	0.30945	0.30949	0.43798	0.43802	0.44230	0.44231	0.67442	0.67442

RMSE	0.41460	0.41459	0.37403	0.37402	252.57	252.57	192.98	192.98
F-test	1.4564	1.1653	1.4473	1.2666	2.5775	2.0621	3.8469	3.3662
Specification IV								
Cum. donation ₅ , arcsinh	0.0424** (0.0179)	0.0602*** (0.0118)	-0.0004 (0.0003)	0.0019 (0.0011)	-24.11*** (7.265)	-31.78*** (7.261)	0.3958** (0.1371)	-0.8946 (0.5810)
Any ruralist		-0.5852** (0.2333)		-0.0644 (0.0369)		251.7** (86.30)		36.45* (20.11)
Public list x Cum. donation ₅ , arcsinh			0.0449* (0.0252)	0.0637*** (0.0136)			-20.41** (8.907)	-27.83** (9.392)
Public list x Any ruralist				-0.6751* (0.3173)				269.0** (110.0)
R ²	0.29804	0.30489	0.42580	0.43341	0.43313	0.43589	0.66708	0.66991
RMSE	0.41802	0.41597	0.37806	0.37555	254.64	254.01	195.14	194.31
F-test	2.7598	1.9007	1.9281	1.4206	4.9665	3.3484	5.2097	3.7691
<i>Fixed-effects</i>								
DL year	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	351	351	351	351	351	351	351	351
Dependent variable mean	0.53276	0.53276	0.53276	0.53276	656.22	656.22	656.22	656.22

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Clustered (DL year) standard-errors in parentheses.

Campaign finance prior to DL publication and conditional on inspection, including years of inspection and listing.

D Facts about Trafficking and Campaign Finance in Brazil

In this subsection, I summarize facts about trafficker profile, economic sector, spatial prevalence of traffickers, and campaign finance that are relevant to the analysis I conduct. Albeit non-exhaustive, they expand the current understanding of trafficking in Brazil.

Fact 1: The majority of inspected traffickers, whether listed or not, are bosses as opposed to firms. Moreover, partners, associate companies, managers, recruiters, and other entities are involved with trafficking to some capacity, and some of them wind up in the DL. Figures [D.3](#) and [D.4](#) confirm that the listing criteria does not precisely specify who in the supply chain must be included in the DL.

Fact 2: In my sample, the number of inspections and of inspected employers over time has remained relatively stable, experiencing a noticeable dip in 2016, one of the years when the List was not published. Of the inspected employers in any given year, almost half participated in labor exploitation. These are the entities I study in this paper.

Fact 3: The number of entities per DL soared from 2011 to 2015 and plummeted afterwards, the period when the DL stopped being published. It went up in 2017, when the DL was made public again, but it remained at pre-2011 levels afterwards. Notice that although the DL is updated semesterly, a number of corrections are made immediately after its publication, a phenomenon that has been attributed to adjudication.

Fact 4: Most traffickers stay on the list less than 1,000 days (approx. 2.7 years). Within this group, the largest mass of traffickers remains listed between 500 (16.7 months) and 1,000 days (33.3 months), roughly.

Fact 5: Multiple delays exist in the DL process, and campaign finance behavior studied in this thesis takes into account the relative timing of these events. Figures [D.7](#) and [D.8](#) show that it takes

approximately one to two years for a trafficker to be listed after an inspection, and contribution frequency diminish after inspection. On the other hand, being included in the DL *per se* does not seem to affect election financing as fast. Figure D.9 contains the same information of Figure D.7(b), restricted to listed entities and subdivided by the gap between DL publication and inspection. Surprisingly, traffickers who are listed quickly (time gap = 0 years) do not make a contribution in the period leading up to an inspection, but those who are punished late do (time gap = 1, 2, 3, 4 years). Figures 5 and D.1 confirm such pattern and motivate my analysis. Lastly, Panels (a) and (b) of Figure D.2 suggest that the number of donors decreases when an inspection or DL happens, meaning that the fewer listed traffickers make larger donations in the year they are included in the List.

Figure D.1: Average Donation Relative to Listing

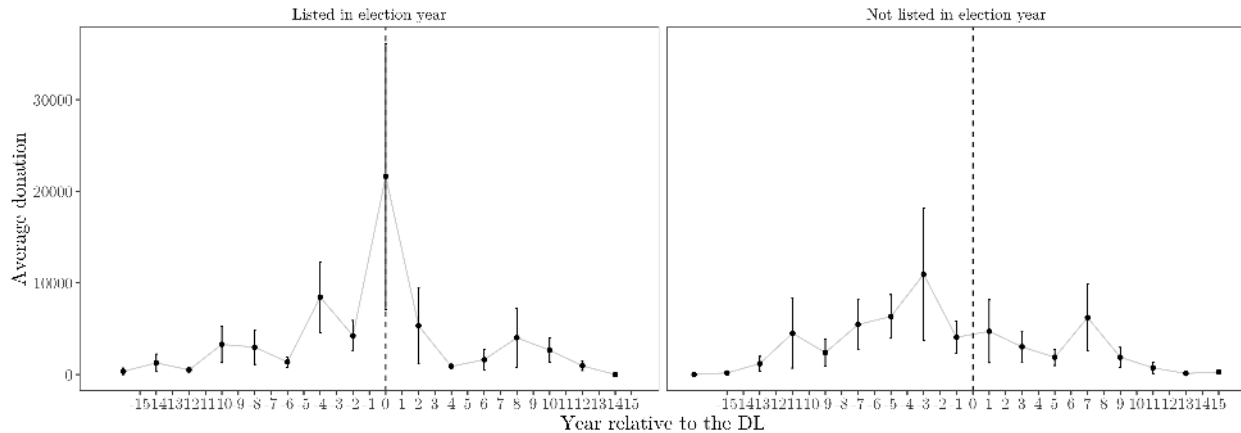


Figure D.2: Freq. of Donation over Time

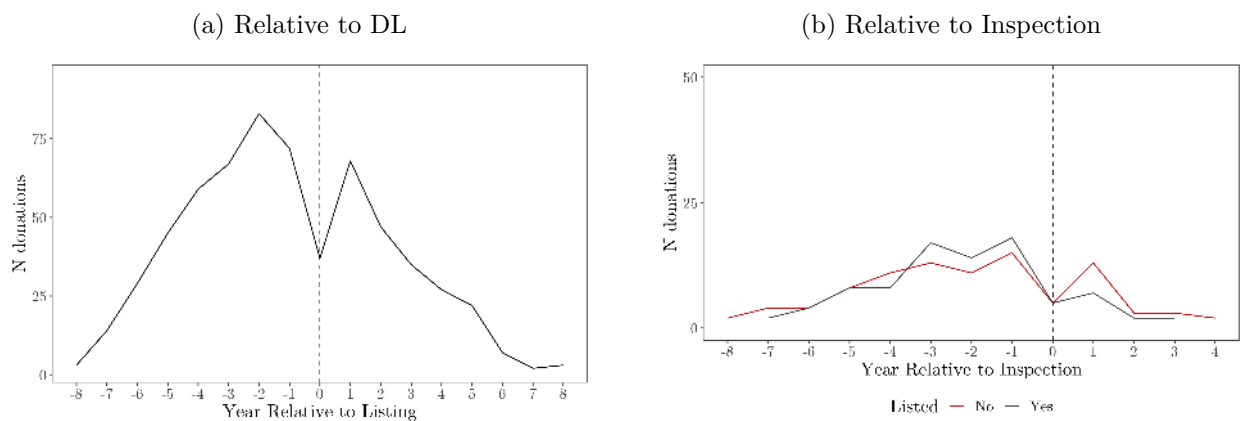


Figure D.3: Type of Trafficker

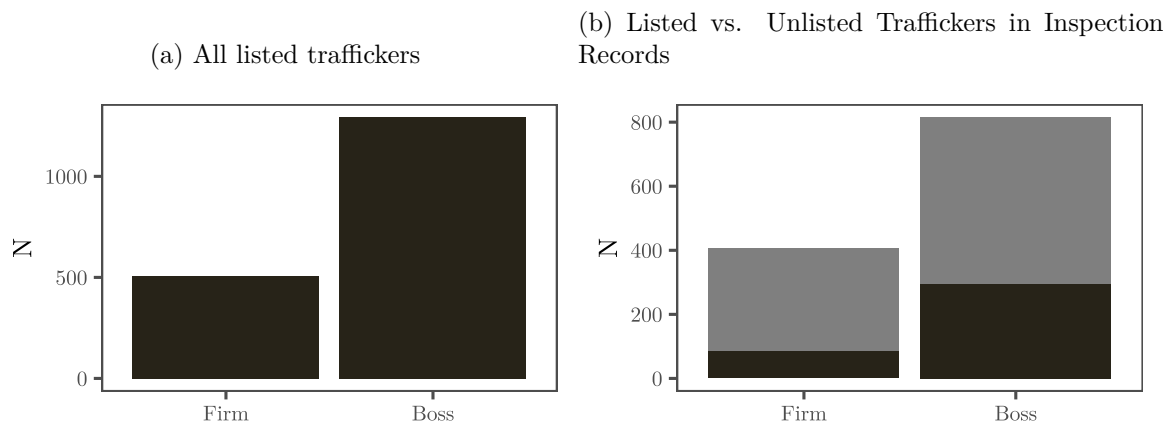


Figure D.4: Trafficker Role in Labor Scheme

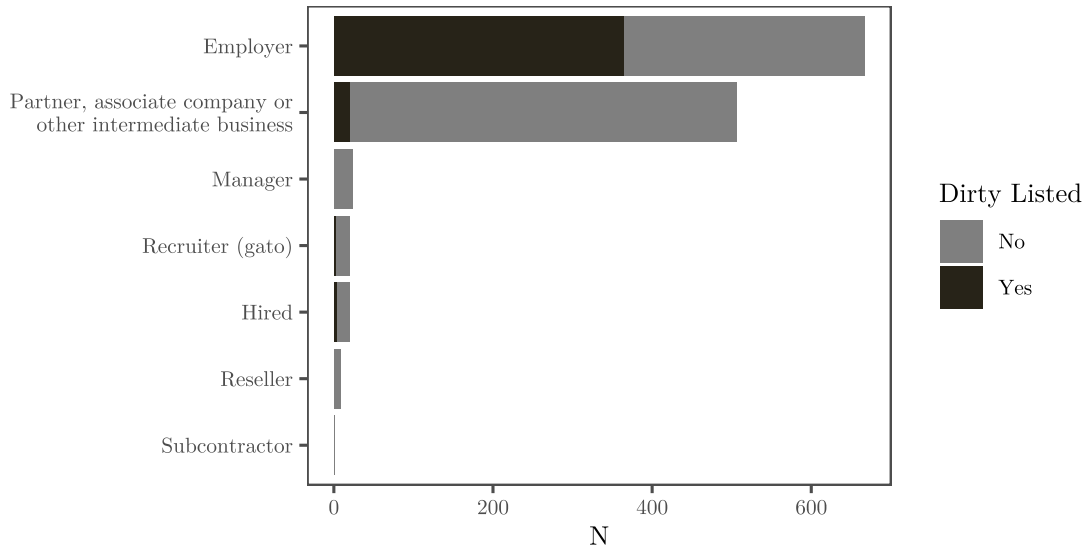


Figure D.5: Historical Trends

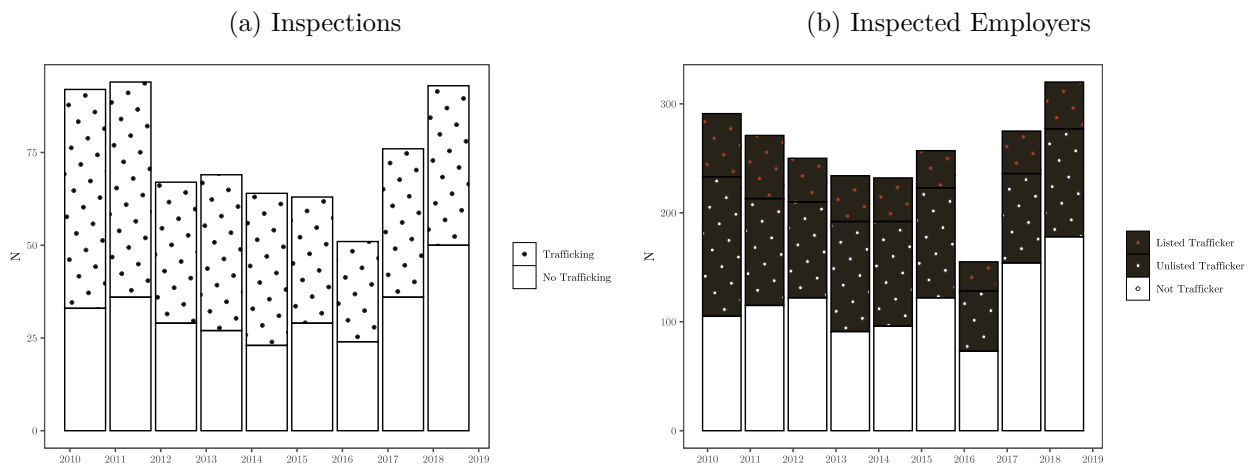


Figure D.6: Distribution of Days on DL

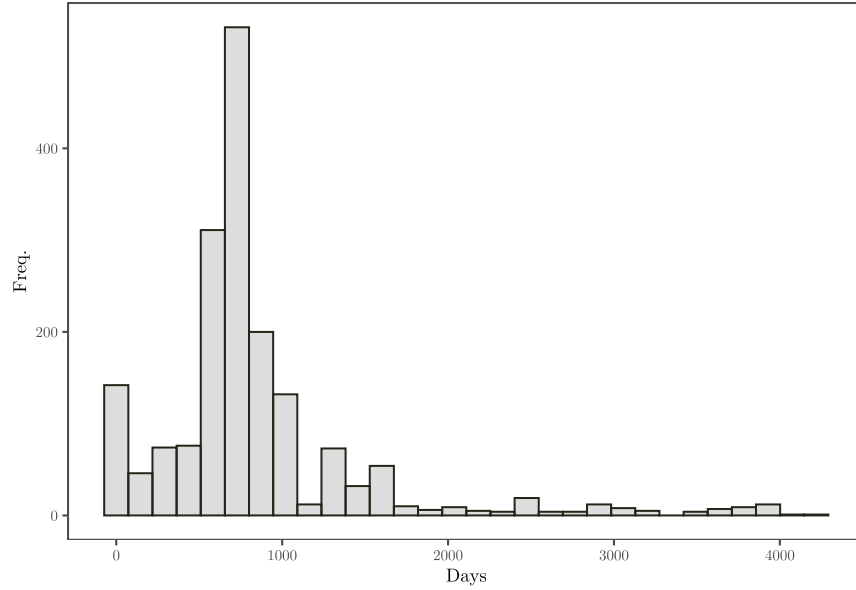
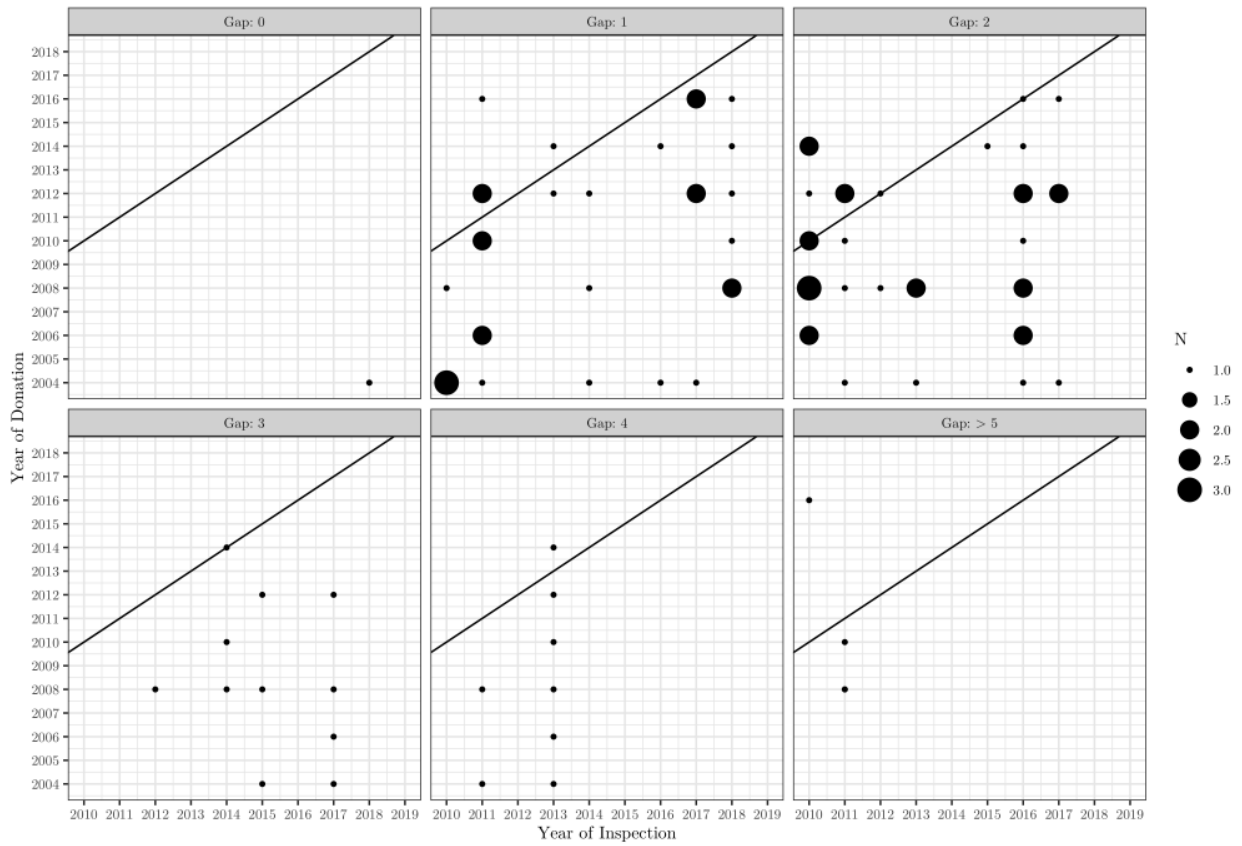


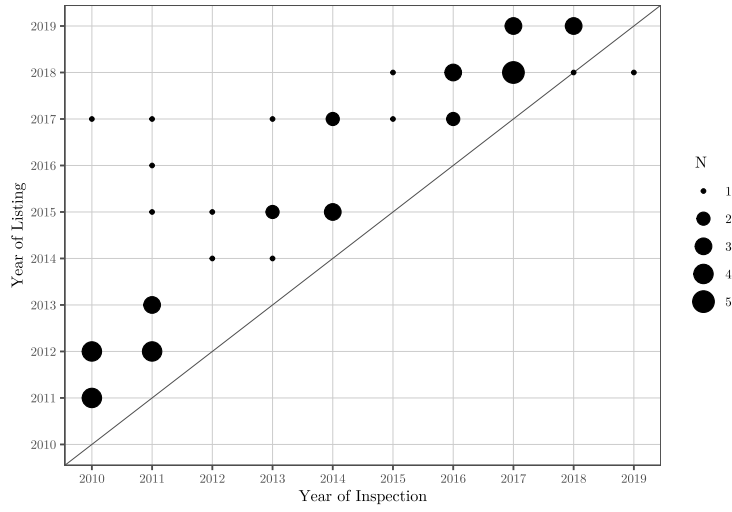
Figure D.9: Timing of Donations Relative to Inspection Year, by Gap between Inspection and DL Publication



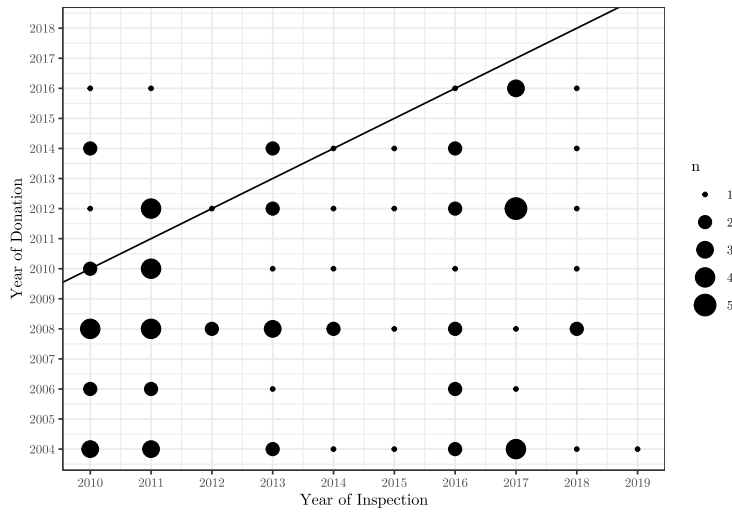
Note: Gap = DL - Inspection (in years).

Figure D.7: Delays Over Time

(a) Inspection and DL



(b) Donation and Inspection



(c) DL and Donation

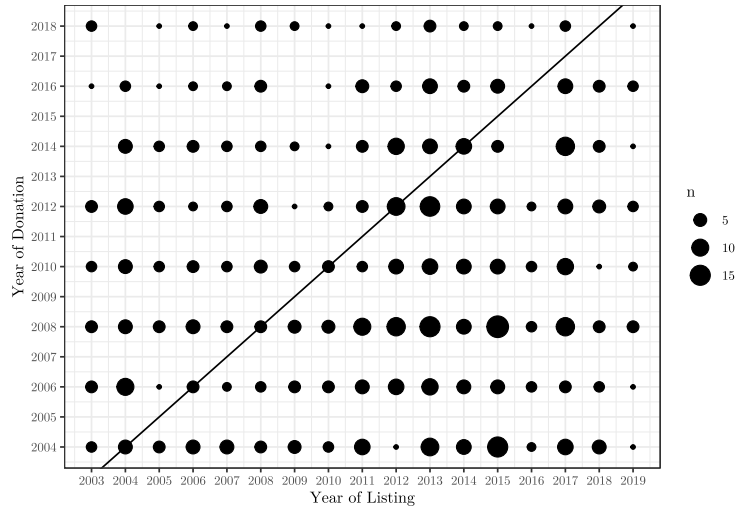
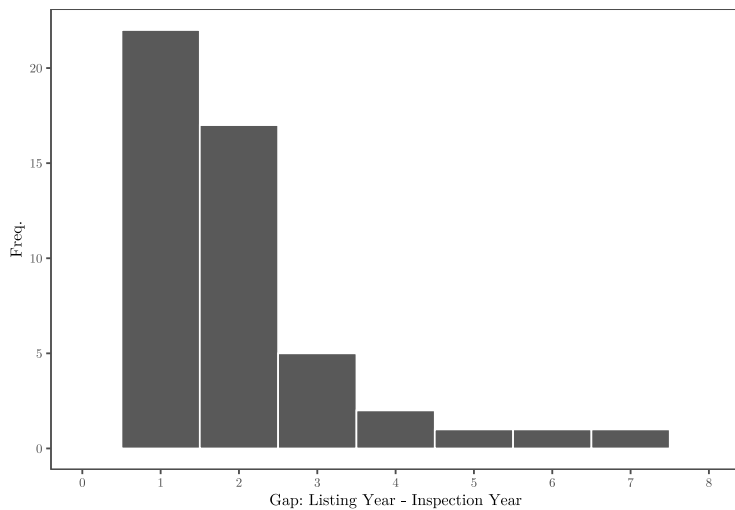
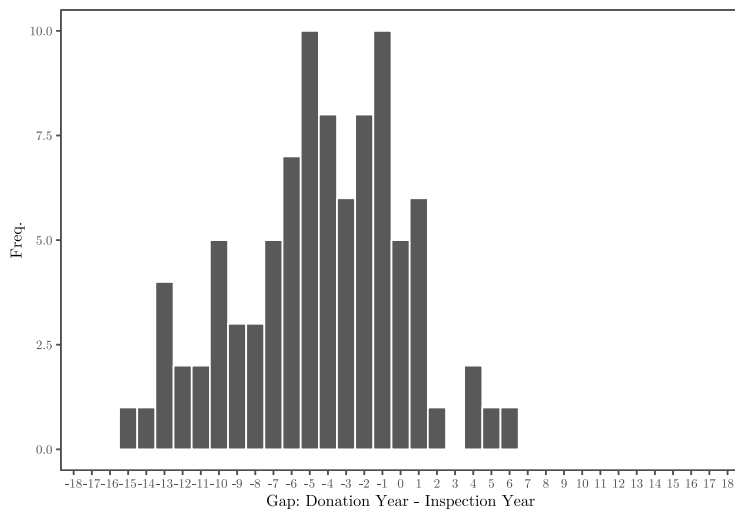


Figure D.8: Delay Distribution (in years)

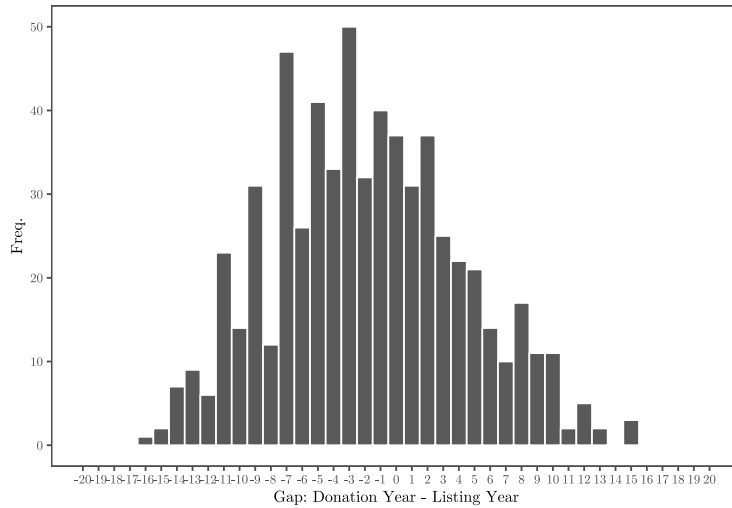
(a) Inspection and DL Gap



(b) Donation and Inspection Gap



(c) DL and Donation Gap



E Data Treatment

E.1 Trafficker Data

Data Source 1: Inspection Reports

Inspection reports produced by the Brazilian Federal Labor Prosecution Office (FLPO) provided data on all employers subject to a labor inspection under suspicion of trafficking. Each report summarizes an investigation and contains details about employers, workers, business location, economic activity, inspectors and police support, dates of inspection, labor code violations, fees and compensation owed to workers, and a determination of the leading inspector about whether labor conditions typified trafficking. Reports from 1996 to 2021 are available, but they are originally stored in non-searchable pdf documents, often with handwritten passages. Recent efforts by HTDL in partnership with the FLPO made available a dataset with a subset of these reports for selected variables of interest.

Data Source 2: Dirty List of Slave Labor

The Dirty List of Slave Labor (DL or simply “List”) is produced semesterly by the Ministry of Labor³ since 2003 and is available online at <https://www.gov.br/trabalho-e-previdencia/pt-br/pt-br/composicao/orgaos-especificos/secretaria-de-trabalho/inspecao/areas-de-atuacao/combate-ao-trabalho-escravo-e-analogo-ao-de-escravo>⁴. Due to legal battles, a Supreme Court decision suspended its publication from 2015 to 2017, but the Ministry still generated a list for governmental use. Access to these unpublished files was made available with the support of FLPO and I end up with records at the level of each List from 2003 to 2021.

This data provides trafficker identification, date of listing, source of List (publication or internal

³From 2019 to 2021, the List was published by the Ministry of Economy after the temporary dissolution of the Ministry of Labor during Jair Bolsonaro’s administration.

⁴Date of latest access: January 17, 2022.

database), and type of id. I reshape the dataset to the trafficker level and create variables for the first and last day they are included on the List as well as the day they are removed from it, defined as the first List for which their ids do not show up. I also calculate the number of days traffickers stay on the List, that is, the interval between first appearance and removal dates.

Devising a Trafficking Flag

During the production of this thesis, a trafficking flag for inspection was not available in the Lab's inspection records. Several data collection difficulties explain this shortcoming, but, most importantly, an objective definition of trafficking is still lacking. In addition to inspection findings and recommendations, there are other ways to identify the practice in the country, for example with records of civil or criminal prosecution and administrative actions. Here, I chose to use the Dirty List as a proxy of trafficking, i.e. if an inspection involves a listed employer, then all other employers in such inspection are considered to be traffickers despite not being included in the List. This definition has its own limitations, one of which being that it is possible that inspections where trafficking was confirmed are left out if none of its employers were included in the DL and another being that traffickers might not be inspected, but be included in the DL by adjudication. In either case, it is highly likely that this definition leads to undersampling of unlisted traffickers.

E.2 Campaign Finance Data

E.2.1 Donations

Data Source

Historical donation data came from the Brazilian Superior Electoral Court (TSE) data repository which is available online at <https://www.tse.jus.br/eleicoes/estatisticas/repositorio-de-dados-eleitorais-1>. Tables are originally organized by year and states, and elections are biennial, alter-

nating between national and local elections. Originally, datasets are organized at the level of the donation and include variables such as donor identification; candidate's political party and political office; size, type, and date of donation; and state, city, level (national or local), type (ordinary or supplementary), round, and date of election. Data was downloaded for years 2004 through 2018 on March 29, 2021.

Treatment

I performed extensive cleaning and wrangling to achieve the following: (i) standardize variable names across years; (ii) systematize the type of donation, (iii) fix formatting of strings, identifiers, and dates; (iv) remove unidentified donors; (v) remove institutional donors; and (vi) reorganize the data to the level of donor-candidate pair at each election year.

The first step in my treatment was to complete (i) so I could merge all raw tables into a single dataset with all donations in the selected period. The full raw dataset contained 10,186,076 observations. I then translated and systematized the type of donation of all records to avoid encoding errors and, most importantly, corrected category discrepancies across the different years. Table [E.1](#) displays the new values.

Table E.1: Type of Donation

Original values	Classified as
Recursos de Financiamento Coletivo	Crowdfunding
Recursos de Pessoas Jurídicas	Donation from Firm
Recursos de Pessoas Físicas	Donation from Individual
Recursos de outros candidatos/comitês	Donation from other candidate or political committee
Recursos de outros candidatos	Donation from other candidate or political committee
Recursos do fundo partidário	Donation from party fund
Recursos de partido político	Donation from political party
Rendimentos de aplicações financeiras	Investment income
Recursos de origens não identificada	Non-identified source
Erro CD_TITULO Estimável Null	Null
Doações pela internet	Online donation
Recursos de doações pela internet	Online donation
Receitas diversas a especificar	Other
Descrições das doações relativas a comercialização ou evento	Sale of goods or fundraising event
Descrição das doações relativas à comercialização	Sale of goods or fundraising event
Comercialização de bens e/ou realização de eventos	Sale of goods or fundraising event
Comercialização de bens ou realização de eventos	Sale of goods or fundraising event
Recursos Próprios	Self-finance

In part (iii), I removed all Portuguese accents and symbols from candidate and donor name, and converted dates to the international format “YYYY-MM-DD”. In addition, I added leading zeros to donor identification that were lost when I imported the data into R. To do so, I relied on the type of donation and classified IDs as CNPJ if the data indicated “Donation from Legal Entity” or CPF if it said “Donation from Individual.” I then manipulated ID lengths to obtain 14-digit long strings for CNPJs and 11-digit long strings for CPFs. Finally, for CNPJs, I generated a new variable that would record the first 8 digits of donors’ IDs (CNPJ root), which identifies the headquarters of a firm.

Steps (iv) and (v) were relatively more complex. In (iv), I identified donor ids that were incorrectly coded and dropped them. These included id values such as “-1” and “Regular”, and a complete list can be found in Table E.2. I also removed observations with missing ids. The motivation for this was that to compare donations from different donors, I needed uniquely identified entities, but incorrect or missing ids would not allow for that.

In step (v), it is important to note that campaigns may be financed not only by individuals and firms (until 2015), but also by political parties, campaign committees, party funds, crowdfunding, other politicians, and transfers from the candidate herself⁵. I refer to the last six as institutional donors and, based on the type of donation variable, I dropped them. Next, by looking directly at the donor name, I recognized that a number of these transfers were coded as another category, i.e. “Other”, or contained a missing value. To circumvent this, I employed what I will refer to as “string method”: I identified strings associated with these institutional donors (see Table E.2) and removed observations that contained one or more of these strings. This procedure is not bulletproof. In particular, not all party donations could be removed and some individuals might have been mistakenly removed. Finally, I dropped donor names that were exactly the same as the name of the party.

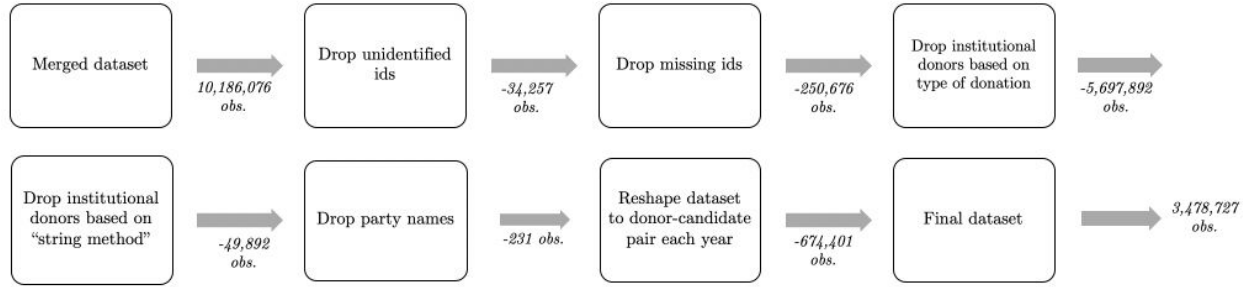
Table E.2: Unidentified IDs and strings associated with institutional donors

Unidentified IDs	“”, “-1”, “#NULO”, “#NULO#”, “—”, “0”, “00”, “00000000”, “000000000”, “0000000000”, “00000000000”, “000000000000”, “0000000000000”, “00000000000000”, “00000000000001”, “00000000001”, “00000000002”, “00000000003”, “00000000004”, “00000000005”, “00000000006”, “00000000007”, “00000000008”, “00000000009”, “00000000010”, “999999999999999”, “99999999999”, “9999999999999”, “Regular”
Institutional Donors	“PARTIDO”, “Partido”, “PART.”, “COMITE”, “Comite”, “COMIT”, “Comit”, “ELEICOES”, “ELEICAO”, “2004”, “2006”, “2008”, “2010”, “2012”, “2014”, “2016”, “2018”, “Direcao”, “DIRECAO”, “Diretorio”, “DIRETORIO”, “DIR.”, “Dir.”, “DIRET”, “COMISSAO”, “COLIGACAO”, “Coligacao”

Lastly, I restructured records to indicate total amount donated for each donor-candidate pair in a given election year. Figure E.1 tracks how many observations were eliminated in each of the procedures described.

⁵For reference, see Table E.1.

Figure E.1: Campaign Donation Cleaning



The final dataset contained 3,478,727 donations from firms and individuals, organized at the level of donor-candidate pairs each election year.

E.2.2 Candidate Metadata

Data Source

Candidate metadata also came from the TSE data repository available online at <https://www.tse.jus.br/eleicoes/est-de-dados-eleitorais-1> and tables were organized by state and election year. Observations are at the candidate-election level and variables include identification number, nickname, gender, race, occupation, level of education, birth-date, birth place, marital status, reelection dummy, dummy for whether they won the election among others. Data was downloaded on May 3, 2021.

Treatment

I first merged metadata from all elections and obtain a raw dataset with 1,866,880 observations. Next, I repeated steps (i) and (iii) like before, reshaped the data to the candidate level, and selected variables for gender, race, education level, and occupation. Note that candidates often reported different race, education level, and occupation across different elections. To deal with these duplicates, I adopted the following strategy:

- If a candidate reported more than one race, I redefined them as "multiracial".

- If a candidate submitted different education levels over time, I kept the highest level of education reported, even if incomplete. For example, if in election 1 candidate A said she had completed middle school and in election 2 she reported an incomplete high school degree, I kept the incomplete high school degree.
- I preserved all occupations, except when the candidate reported to be an elected politician or appointed minister as a *second* profession. In these cases, I eliminated "politician". The reason to do so is that candidates report "politician" as profession when running for re-election, which is not informative of additional employment connections these individuals may have. When the data showed "politician" as the only profession, it means that earlier data with the primary occupation was not available and I kept these as the unique profession.

I constructed dummies for whether at least one of the reported occupations falls in the groups described in Table E.3.

Table E.3: Occupation groups

Category	Occupation
Agricultural work	Farmer, Agronomist, Charcoal burner, Miner, Operator of farm and forestry machinery, Rancher, Agricultural producer, Agricultural technician, Forestry worker, Rural worker, Zoology technician
Business and high-level management	Administrator, Financial asset professional, Retailer, Company director, Businessman, Banker
Lawyer, judge, and other legal occupations	Lawyer, Judges of lower and higher courts, Member of Prosecution Office, Attorney, Justice Office, Servant in electoral court system, Notary
Public service	Commissioned officials, State public servant, Federal public servant, Municipal public servant

Using the ruralist data (described below), I added a flag for pro-trafficker candidates. The final dataset provided information for 1,270,391 candidates from 2004 to 2018.

E.3 Ruralist Data

E.3.1 Members of the Agricultural Parliamentary Front

Data Source

Lists with names of members of the Agricultural Parliamentary Front (FPA) are published both on the Official Journal of the Chamber of Deputies (DCD)⁶ and the website of the Chamber of Deputies at <https://www.camara.leg.br/internet/deputado/frentes.asp>, with the exception of the 53th Congress which is only accessible in the DCD. The publication of the list of members is mandated by the Act of the Board 69 of November 11, 2005, which required all parliamentary fronts to submit a registration request with a minimum of one-third of signatures of Congress members. The request is published on the DCD when the front is officially created (2008 in the case of FPA) and must be renewed in the beginning of each Congress. Legislators' signatures are analyzed and may be rejected if they do not match the signature on file. A mirror of the List is also posted on the Chamber of Deputies website, but there are some unexplained discrepancies and the two lists are not exactly the same for a given year. Because both are public data, I chose to keep the two of them. Table E.4 details these sources. All links were accessed on October 4, 2021.

A caveat is in order: the composition of FPA is dynamic and changes during the term, but these are not currently traceable. Hence, based on the aforementioned publications, 705 politicians were identified to be part of FPA at some point. Data in both of above sources were stored in non-searchable PDF documents and required manual extraction of all pieces of information.

Collection

I manually extracted name, political party, political office, state, and Congress of each ruralist for four different Congresses and store them in a csv file. The identification for this file is the pair

⁶Available online at <https://imagem.camara.leg.br/diarios.asp?selCodColecaoCsv=D>.

Table E.4: Sources of Members of FPA

Congress	Publication on DCD	Link of post on Chamber of Deputies
53 th	April 16, 2008	Unavailable
54 th	April 18, 2011	https://www.camara.leg.br/internet/deputado/frenteDetalhe.asp?id=356
55 th	March 25, 2015	https://www.camara.leg.br/internet/deputado/frenteDetalhe.asp?id=53476
56 th	February 26, 2019	https://www.camara.leg.br/internet/deputado/frenteDetalhe.asp?id=53910

(name of politician, state).

Treatment

I removed all punctuation and accents from the dataset and fixed states that were incorrectly recorded on the lists. I ended up with 687 uniquely identified members.

E.3.2 Members of CAPADR

Data Source

Information about the members of the Committee on Agriculture, Livestock, Food Supply and Rural Development (CAPADR) came from the 2015 Annual Report stored in the [Digital Library of the Chamber of Deputies](#) and available at <https://www2.camara.leg.br/atividade-legislativa/comissoes/comissoes-permanentes/capadr/relatorios-de-atividades/relatorio-de-atividades-2015>. The report contains the name, political party, and state of its member and indicates whether they are president of the Committee, permanent members, or substitutes. It also details activities of the Committee, including roll call voting records on the bills considered. The report was accessed on July 23, 2021.

Collection

I manually extracted name, political party, and state and indicated whether the politician is the president of the Committee, a permanent member or substitute. Most importantly, I registered

whether the politician voted ‘Yes’, ‘No’, or if they abstained from voting on PL 3.842/2012, as explained previously. I stored these values in a csv file. The identification of this dataset is the pair (name of politician, state).

Treatment

I removed all punctuation and accents, fixed states that were incorrectly assigned to politicians, and kept only observations of non-substitute members who voted ‘Yes’. I started with 95 politicians, 46 of which were substitutes and 3 abstained from voting. Thus, the treated dataset has 45 uniquely identified voting members.

E.3.3 Final Ruralist Dataset

I performed a full merge between FPA and CAPADR datasets and the final dataset indicates whether a politician has ever been a ruralist based on affiliation with FPA and support to pro-trafficker bill via participation in the 2015 CAPADR. It has 695 uniquely identified ruralists, 37 of which have been involved with both FPA and CAPADR.

Notice that the CPF of ruralists was not available in the previous sources. However, matching between different datasets using CPF is more reliable than using the key (name of politician, state). Particularly, because politicians change political parties and there are many homonyms, using such key could more easily lead to data mismatch. Recall that the donation dataset has over 3 million observations. For such reasons, I decided to match each of the 695 politicians in the ruralist dataset to their CPF. To perform such task, I used the candidate metadata.

I started by dropping all observations of candidates who were not running for the Chamber of Deputies or the Senate in the candidate metadata dataset, and I created a similar key of (name, state) for each candidate. Here, however, I constructed two keys – (legal name, state) and (nick-

name, state) –, and implemented three methods to identify ruralists' CPF.

Method 1 used politicians' legal name and successfully identified 56 unique candidates. Method 2 used politicians' nickname and found the CPF of 548 candidates. Of these, there were 10 instances where the same key returned two different CPFs. I performed searches by hand on the website of the Chamber of Deputies to tell the candidates apart and checked their affiliation with FPA or CAPADR.

Finally, Method 3 addressed all those pairs that had not been identified in Methods 1 and 2. These cases included instances where the politician had a middle name that prevented the correct identification or some name was misspelled in one of the datasets. I looked up name by name on the candidate metadata. In the end, I was able to uniquely identify 670 candidates by their CPF.