Pontifícia Universidade Católica do Rio de Janeiro

Ísis Ferreira Lira

Essays on Labor Economics: Gender Gap in Formal Employment and the Role of Labor Inspections in Brazil

Tese de Doutorado

Thesis presented to the Programa de Pós-graduação em Economia of PUC-Rio in partial fulfillment of the requirements for the degree of Doutor em Economia.

Advisor : Prof. Gustavo Gonzaga
Co-advisor: Prof. Tomás Guanziroli

Rio de Janeiro
May 2025

Pontifícia Universidade Católica
do Rio de Janeiro

Ísis Ferreira Lira

Essays on Labor Economics: Gender Gap in Formal Employment and the Role of Labor Inspections in Brazil

Thesis presented to the Programa de Pós-graduação em Economia of PUC-Rio in partial fulfillment of the requirements for the degree of Doutor em Economia. Approved by the Examination Committee.

Prof. Gustavo Gonzaga
Advisor
Departament of Economics – PUC-Rio

Prof. Tomás Guanziroli
Co-advisor
Universidad Andrés Bello – Universidad Andrés Bello

Prof. Renata Narita
Department of Economics – PUC-Rio

Prof. Juliano Assunção
Department of Economics – PUC-Rio

Prof. Daniel Da Mata
Department of Economics – FGV EESP

Prof. Breno Sampaio
Department of Economics – UFPE

Rio de Janeiro, May 6th, 2025

Ísis Ferreira Lira

MSc. in Economics (UFJF, 2021). BA. in Economics (UFJF, 2019).

Bibliographic data

Lira, Ísis

Essays on Labor Economics: Gender Gap in Formal Employment and the Role of Labor Inspections in Brazil / Ísis Ferreira Lira; advisor: Gustavo Gonzaga; co-advisor: Tomás Guanziroli. – Rio de janeiro: PUC-Rio , Departamento de Economia, 2025.

v., 131 f: il. color. ; 30 cm

Tese (doutorado) - Pontifícia Universidade Católica do Rio de Janeiro, Departamento de Economia.

Inclui bibliografia

1. Economia – Teses. 2. Economia do Trabalho. 3. Economia do Desenvolvimento. 4. Desigualdade de Gênero. 5. Fiscalização do Trabalho. I. Gonzaga, Gustavo. II. Guanziroli, Tomás. III. Pontifícia Universidade Católica do Rio de Janeiro. Departamento de Economia. IV. Título.

CDD: 620.11

Acknowledgments

First and foremost, I thank God for guiding and supporting me throughout my entire life.

I cannot find words to express my gratitude to my parents, Eliane and Daniel, and to my entire family for their unwavering support and unconditional love. Everything I am today, I owe to you.

I am deeply grateful to each one of my friends who, throughout this journey, have provided warmth and encouragement. Thank you for everything, Lorena, Tiana, Lucas Nascimento, Rafael, Lucas Lourenço, Joyce, Vinicius, Damares, Vitória, Humberto, and Pedro. Special thanks to Thaiany for the love and partnership that made this PhD period bearable.

I am thankful to each of the professors I have had the privilege of learning from. In particular, I would like to express my gratitude to those who have accompanied me in recent years – my advisors Gustavo Gonzaga and Tomás Guanziroli – and to Laura Schiavon, Flávia Chein, and Ricardo Freguglia for helping me reach this point.

I am grateful to the examining committee for dedicating their time and effort in evaluating this work.

I would like to thank the entire team at the Department of Economics at PUC-Rio and the Faculty of Economics at UFJF for all support.

I'm grateful to PUC-Rio, CAPES and FAPERJ for the financial support.

This study was financed in part by the Coordenação de Aperfeiçoamento de Pessoal de Nível Superior - Brasil (CAPES) - Finance Code 001.

Abstract

Lira, Ísis; Gonzaga, Gustavo (Advisor); Guanziroli, Tomás (Co-Advisor). **Essays on Labor Economics: Gender Gap in Formal Employment and the Role of Labor Inspections in Brazil**. Rio de Janeiro, 2025. 131p. Tese de doutorado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

This dissertation consists of three essays on Labor Economics. The first chapter investigates the impact of job displacement on gender differences in formal labor market outcomes in Brazil. Using administrative matched employer-employee data from 2003 to 2020 and a difference-in-differences approach, it examines mass layoffs as an exogenous shock. The findings reveal that women experience larger and more persistent declines in formal employment than men after displacement. While both men and women suffer wage losses upon reemployment, the magnitude is similar. The results suggest that post-displacement labor market dynamics play an important role in sustaining gender disparities in formal employment, with gender differences in reemployment explaining a substantial share of the employment gap.

The second chapter estimates the effects of labor inspections on establishment outcomes in Brazil. Leveraging rich administrative data and a staggered difference-in-differences strategy, the analysis shows that inspections significantly reduce employment, primarily by decreasing hiring rather than increasing separations. Inspections also raise the probability of establishment exit, particularly among younger and medium-sized establishments. At the worker level, inspections generate a temporary and small increase in employment, followed by wage stagnation for stayers, while leavers experience stable or slightly improved earnings. The findings indicate that enforcement operates through both deterrence—affecting even non-notified firms—and punishment, with fined firms exhibiting more pronounced adjustments.

The third chapter provides a descriptive analysis of the determinants of labor inspections in Brazil. Inspections focus on large establishments, covering about one-third of formal sector workers annually. Roughly 40% of inspected establishments are audited for the first time each year, while the rest have been previously inspected. Inspections are also more frequent among establishments with higher turnover and those located closer to enforcement offices, suggesting that both establishment characteristics and logistical constraints shape inspection allocation.

Keywords

Labor Economics Development Economics Gender Inequality Labor Inspection

Resumo

Lira, Ísis; Gonzaga, Gustavo; Guanziroli, Tomás. **Ensaios em Economia do Trabalho: Desigualdade de Gênero no Emprego Formal e o Papel da Fiscalização do Trabalho no Brasil**. Rio de Janeiro, 2025. 131p. Tese de Doutorado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Esta tese é composta por três ensaios em Economia do Trabalho. O primeiro capítulo investiga o impacto da perda de emprego nas diferenças de gênero nos resultados do mercado de trabalho formal no Brasil. Utilizando dados administrativos combinados de empregador e empregado de 2003 a 2020 e uma abordagem de diferenças em diferenças, o capítulo analisa os desligamentos em massa como um choque exógeno. Os resultados revelam que as mulheres experimentam quedas mais acentuadas e persistentes no emprego formal em comparação aos homens após a perda do emprego. Embora tanto homens quanto mulheres sofram perdas salariais ao serem reempregados, a magnitude das perdas é similar. Os resultados sugerem que as dinâmicas do mercado de trabalho após a perda de emprego desempenham um papel importante na manutenção das disparidades de gênero no emprego formal, com as diferenças de gênero no reemprego explicando uma parte da lacuna de emprego.
O segundo capítulo estima os efeitos das inspeções trabalhistas sobre os variáveis dos estabelecimentos no Brasil. Usando dados administrativos e uma estratégia de diferenças em diferenças escalonada, a análise mostra que as inspeções reduzem significativamente o emprego, principalmente pela diminuição das contratações, ao invés do aumento das separações. As inspeções também aumentam a probabilidade de saída dos estabelecimentos, especialmente entre os mais jovens e de porte médio. No nível dos trabalhadores, as inspeções geram um aumento temporário e pequeno no emprego, seguido por uma estagnação salarial para os trabalhadores que permanecem, enquanto os que saem experimentam ganhos estáveis e maiores. Os resultados indicam que a fiscalização opera pelos efeitos dissuasão e punição.
O terceiro capítulo fornece uma análise descritiva dos determinantes das inspeções trabalhistas no Brasil. As inspeções concentram-se em grandes estabelecimentos, abrangendo cerca de um terço dos trabalhadores do setor formal anualmente. Aproximadamente 40% dos estabelecimentos inspecionados por ano estão recebendo a primeira fiscalização. As inspeções também são mais frequentes entre os estabelecimentos com maior rotatividade e aqueles localizados mais próximos aos escritórios de fiscalização, sugerindo que tanto as características dos estabelecimentos quanto as restrições logísticas moldam a alocação das inspeções.

Palavras-chave

Economia do Trabalho Economia do Desenvolvimento Desigualdade de Gênero Fiscalização do Trabalho

Table of contents

List of figures

List of tables

1
The Effects of Job Displacement on the Gender Employment Gap

This paper examines the impact of job displacement on formal employment outcomes in Brazil, focusing on gender differences. Using RAIS administrative data from 2003 to 2020 and a matched difference-in-differences approach, we analyze mass layoffs as an exogenous employment shock. We find that women experience greater declines in formal employment than men after displacement. In the first year, women's probability of holding a formal job drops by 32%, compared to 23% for men. Over time, the gender gap narrows but does not fully close in the 8 years following layoff. Heterogeneity analysis shows that education, tenure, maternity leave history, and firm size influence the extent of employment losses. Layoffs also result in wage losses. However, the magnitude of this effect is quite similar for men and women, with both experiencing a reduction of approximately 5% upon reemployment. Our findings highlight the role of post-displacement labor market dynamics in sustaining gender disparities in formal employment. A back-of-the-envelope calculation suggests that gender differences in reemployment rates explain about 28% of the observed gap in formal employment.

KEYWORDS: Job Displacement, Mass Layoff, Employment, Gender Gap Código JEL: D22, K20, K42

1.1 Introduction

In developing countries, formal employment plays a central role as a source of social protection and economic stability, providing workers with access to benefits that are unavailable in informal positions (Fields, 2011). In Brazil, formal employment guarantees numerous rights, including unemployment insurance, retirement pensions, medical leave, and paid maternity leave - the latter being particularly relevant for women. Despite these advantages, women are less represented than men in formal employment, with a persistent gap of 8 percentage points over the last decade.

While gender disparities in formal employment have been extensively documented in high-income countries, the dynamics following job loss may differ substantially in developing economies due to distinct institutional settings and labor market structures. In particular, informality offers an alternative form of employment that is often more accessible, though less stable, for displaced women. Additionally, limited childcare provision and social safety nets constrain women's job search efforts and make transitions back to formal work more difficult. These constraints are not only structural but gendered, amplifying the potential for asymmetric reemployment outcomes.

In this paper, we study the reasons for this gap, with a particular focus on everyday labor market frictions. More specifically, we examine whether unemployment events have differential impacts on the future formal employment outcomes of men and women, and whether these effects are significant enough to account for the persistent gap in male and female formal labor force participation rates. Involuntary job separations are common among workers in developing countries. For example, in Brazil, about 19% of formal workers are fired from their jobs each year[1]. Difficulties in finding new formal employment—particularly for women, who may experience longer delays before reemployment—can lead to extended and repeated unemployment spells, contributing to the observed gender gap. Our results confirm this suspicion and indicate that a significant portion of the participation gap arises from differential responses to separation events.

We use the Brazilian linked employer-employee dataset (RAIS) to conduct the empirical analysis. RAIS contains information on all formal labor contracts in Brazil. During our period of analysis, from 2003 to 2020, the full dataset comprises approximately 1.1 billion observations, covering 114.5 million workers and 9.6 million establishments. One of the key advantages of RAIS is its ability to track workers

[1]Statistics calculated using RAIS data for the period 2008–2012.

across different jobs, as it includes precise hiring and separation dates. Using this information, we observe workers' formal labor market status for up to five years before a separation event and eight years afterward. The data also include the reason for separation—whether it was initiated by the employer or the employee, for example. In our analysis, we restrict the sample to employer-initiated separations. Given the inability to differentiate between unemployment and informal employment during the periods between formal jobs, we focus our analysis on transitions between formal employment and non-formal employment.

Due to the endogenous nature of job separations, our empirical analysis focuses on mass layoffs. Employer-initiated separations can be triggered either by firm-level factors—such as restructuring —or by employee-related factors, such as poor performance or lack of effort. In the latter case, the post-separation employment status may not reflect the opportunities available to the average worker. Mass layoffs, by contrast, provide a plausibly exogenous shock to employment at the individual level. When an establishment dismisses a large share of its workforce, it is less likely that separations are driven by the characteristics of individual workers and more likely that they reflect broader firm strategies beyond any one worker's control. Following Britto *et al.* (2022), we define a mass layoff as an event in which an establishment with at least 15 employees dismisses 33% or more of its workforce within a calendar year. Our findings are robust to more restrictive definitions. We analyze separations from 452,901 mass layoff events that occurred between 2008 and 2012.

To further control for life-cycle employment patterns and business cycle fluctuations, we employ a matched difference-in-differences approach. Specifically, we compare the formal employment trajectories of laid-off and non-laid-off workers during the same period, separately for women and men. For each mass layoff year, we construct a pool of treated workers—those separated due to a mass layoff—and a pool of potential control workers —those not dismissed during that year. Treated workers are matched to control workers of the same gender, age, schooling, and race, within the same earnings category, and employed at firms of similar size, sector, and geographic location (state). By comparing workers of the same age, we ensure that our estimates are not confounded by gender-specific life-cycle events, such as parenting or retirement.

The descriptive analysis reveals a striking gap in labor force participation between men and women following employer-induced job displacement (not limited to mass layoffs). One year after being dismissed, 71% of men have secured another formal job, compared to only 60% of women. Over time, more individuals exit the formal sector, but the male-female gap remains persistent. These exits may result from subsequent separations, voluntary quits, or retirement. However, we cannot directly attribute them to the initial separation event, as it is not uncommon in Brazil for employers and employees to mutually agree on an employer-initiated separation when the employee wishes to resign (Gonzaga *et al.*, 2003).

The results from the matching difference-in-differences analysis reinforce the descriptive findings, showing that women are less likely than men to hold a formal job after a mass layoff. In the first year following displacement, the probability of formal employment declines by approximately 23% for men and 32% for women, relative to their respective control groups. This initial gender gap—around 9 percentage points—narrows over time and nearly disappears by the eighth year after the layoff. Importantly, this convergence is not driven by a relative improvement in women's employment outcomes, but rather by a larger decline in formal employment among women in the control group. We interpret long-term effects with caution, as employment reductions among the control group after the first year may also result from subsequent layoffs. Nevertheless, eight years after a mass layoff, displaced workers—both men and women —are about 10% less likely to hold a formal job compared to similar workers who were not dismissed during the layoff year.

The formal employment gender gap following mass layoffs varies significantly across some worker characteristics. In the first year after mass layoff, women with primary education or less experience a 40% decline in formal employment, compared to a 25% decline among men with similar educational backgrounds. Workers with a high school education are less affected, with declines of 30% for women and 22% for men. Occupational differences are also pronounced. Blue-collar women are the most affected, but a statistically significant gender gap also exists among lower-level white-collar workers. In contrast, we do not find a significant gap among workers in professional or managerial occupations. The gender gap in employment outcomes is also slightly larger among workers dismissed from larger firms. Finally, maternity leave history reveals an important source of heterogeneity. Among women who had taken maternity leave in the years prior to displacement (a proxy for motherhood), 40% are not formally employed one year after job loss, compared to 31% among those who had not taken maternity leave. This corresponds to a gender gap of 17 percentage points for mothers and 8 percentage points for non-mothers, relative to men.

In contrast, some characteristics do not exhibit differential effects. Age, for example, does not drive the results: although older workers of both genders experience larger initial losses, the gender gap remains stable among workers aged 20 to 50. Similarly, the year of dismissal, worker tenure, employment sector, and regional informality rates do not individually account for the persistent gender disparity. Taken together, these findings suggest that no single factor fully explains the enduring gender gap.

So far, we have documented a large and widespread difference in formal employment between men and women one year after displacement. But is this discrepancy large enough to contribute to the overall gender gap in formal employment? To illustrate the role of labor market frictions in shaping this gap, we perform a back-of-the-envelope calculation. Specifically, we use a simple model of transitions between formal and non-formal employment and calibrate key parameters using observed equilibrium outcomes. Our results suggest that these post-displacement differences account for approximately 28% of the overall gender gap in formal employment. This finding highlights that a significant portion of the gap is not due to women who never entered the labor force. Rather, almost half comes from women who were formally employed but, after an involuntary job loss, either stopped searching for formal work or were unable to quickly find a comparable job offering similar wages and amenities.

Several factors may explain the gender gap following job displacement. First, workers involved in mass layoff events could opt not to return to the formal sector at all: 14.6% of displaced women never reenter within eight years, compared to about 10% of displaced men. This discrepancy may reflect differences in family responsibilities, personal priorities, or other factors that lead women to withdraw permanently.

The second channel concerns women who do intend to return but face greater obstacles or exercise more selective preferences, resulting in longer job search durations. While more than 80% of men who return do so in the first year, only about 75% of women do, with additional returns occurring in the subsequent two years. In the third part, we investigated the hypothesis that women might wait to return to the market waiting for better offers. Looking at wage, we found evidence that both women and men return to lower-paid positions. That is, the delay in women returning may be more related to the difficulty in finding positions.

Related literature: This work contributes to two strands of literature. First, there is a large body of research examining the gender gap in employment and earnings (Bertrand *et al.*, 2004; Goldin *et al.*, 2006; Goldin, 2006; Blau & Kahn, 2013; Goldin, 2014; Card *et al.*, 2016; Blau & Kahn, 2017; Kleven *et al.*, 2019; Sharma, 2023). We contribute to this literature by investigating how employment shocks affect the participation gap and contribute to the non-convergence of participation rates between women and men. The second strand investigates the consequences of job displacement on a range of individual outcomes, including labor market, crime, and health (Jacobson *et al.*, 1993; Von Wachter *et al.*, 2009; Dell *et al.*, 2019; Gathmann *et al.*, 2020; Bhalotra *et al.*, 2021; Britto *et al.*, 2022; Corado, 2023; Schmieder *et al.*, 2023; Amorim *et al.*, 2023; Bertheau *et al.*, 2023).

When narrowing the focus to labor market outcomes, the existing literature can be broadly classified into three groups:(i) studies that examine the effects of job displacement without addressing gender differences, in both Brazil and other countries; (ii) studies that consider gender in the Brazilian context but focus on outcomes other than employment, such as wages; and (iii) studies that investigate gender-specific employment effects, but in high-income countries.

Several studies have documented the long-term impacts of job displacement on labor market outcomes, focusing on earnings and employment without considering gender heterogeneity. In the U.S. context, Jacobson *et al.* (1993) find that displaced workers experience average earnings losses of approximately 25% six years after displacement, while Von Wachter *et al.* (2009) report persistent effects of around 20% even 15 to 20 years later. More recently, Bertheau *et al.* (2023), using harmonized data from several European countries—including Denmark, Sweden, Austria, France, Italy, Spain, and Portugal—estimate earnings losses ranging from 11% to 32% and employment effects between 3% and 17%, five years after displacement. For Brazil, Britto *et al.* (2022) find that displaced workers face employment losses of around 20% and earnings declines of nearly 40% four years after separation. Similarly, Corado (2023) estimate reductions of 15% in employment and 50% in earnings over a comparable period.

Including gender perspective in Brazilian context, Almeida & Narita (2024) analyze wage losses after job separation. They find that both men and women experience immediate and persistent earnings declines, with women facing slightly smaller losses (5.71%) compared to men (7.76%). The magnitude of losses increases with longer unemployment spells and earlier career disruptions. Additionally, the reason for job separation matters, as mass layoffs lead to smaller wage losses for women than regular dismissals. Bhalotra *et al.* (2021), focusing on analyzing the effects of mass layoffs on domestic violence, find that women experience a 23% decline in employment and a 40% decline in earnings in the years following displacement, while men experience reductions of 22% and 42%, respectively.

More closely related to our goal in this paper—understanding whether gender differences in employment arise after job displacement and the mechanisms behind them—recent studies have examined similar questions in high-income countries.Exploiting plant closures due to bankruptcy as a source of exogenous variation, Meekes & Hassink (2022) show that in the Netherlands, women experience employment losses 9 percentage points greater than men in the first month following displacement. The gap declines rapidly and stabilizes at around 2 percentage points by the second year.

In Denmark, Ivandić & Lassen (2023) also use plant closures to study post-displacement dynamics. They find that unemployment increases by 14.2 percentage points for women and 9.8 percentage points for men over a two-year horizon. The gender gap persists even after controlling for human capital, with childcare responsibilities playing a key role in constraining women's labor market recovery. Using German administrative data and leveraging mass layoffs as a source of exogenous job displacement, Illing *et al.* (2024) find that the employment effect in the year of job loss is 3 percentage points larger for women than for men. However, this gap closes by the second year post-dismissal when comparing women with similar observable characteristics to those of dismissed men.

Our paper contributes to this literature by providing evidence on the formal employment effects of job displacement through a gender lens in the context of a middle-income country. Brazil offers a particularly salient setting for this analysis, given its persistently low female labor force participation and high levels of informality—features that sharply contrast with those in most high-income economies. By documenting how job loss differentially affects men and women in this environment, our findings offer valuable insights that may apply to other developing nations facing similar labor market challenges.

This paper is organized as follows. Section 1.2 describes the dataset. Section 1.3 discusses the identification strategy. Section 1.4 presents evidence of the effects of job displacement on formal employment by gender, while Section 1.5 investigates the effects on wages. Finally, Section 1.6 offers concluding remarks.

1.2
Brazilian Formal Labor Market Data

We use labor market administrative records from the Brazilian employer-employee matched dataset (RAIS - *Relação Anual de Informações Sociais*). RAIS is collected by the Ministry of Labor annually since 1975 and covers the universe of formal workers-establishments in Brazil[2].

The dataset provides detailed information on individuals (gender, race, age and education), establishments (sector, size, legal regime, and location), and contract characteristics (wage, hours contracted, type of contract, tenure, date of hire, date of separation, reason of separation, among others). For each worker and establishment on RAIS, we have information about the unique tax identifier[3]. Using

[2]The RAIS dataset does not include records of formally registered domestic workers.
[3]CPF is an individual taxpayer identification number in Brazil, used for personal identification.

this information, it is possible to construct a job history by tracking workers over time and across establishments.

The main caveat of using RAIS is related to its scope. RAIS includes only formal workers and establishments, lacking information on employees out of the labor force, informally employed, unemployed, or self-employed. That is, the job history that we can construct is limited to formal registers.

In this paper, we used RAIS data from 2003 to 2020 to identify workers involved in mass layoffs between 2008 and 2012. This approach allows us to track workers' trajectories for a minimum of five years prior to the layoff and ten years thereafter.

1.3
Identification Strategy

1.3.1
Sample and Matching Procedure

We constructed the sample to analyze the effects of job displacement on formal employment by gender in three steps. First, we use Britto *et al.* (2022) definition of mass layoff in an establishment: i) has at least 15 employees; ii) dismisses 33% or more of the workforce in a year[4]. This allow us to identify workers fired for reasons possibly exogenous using data from 2008 and 2012.

Second, the potential control group consists of workers who were never involved in mass layoff events from 2008 to 2012. For both potential units of treated and control workers, we applied 5 restrictions: i) 18-49 age range; ii) full time contracts (30 or more hours contracted); iii) private sector; iv) workers dismissed once or never in mass layoffs during the period from 2008 to 2012; v) workers employed for at least 2 years before mass layoff in the same establishment[5]. This final restriction is intended to restrict the sample to workers with stronger firm attachment, which is especially important in the Brazilian context, given the prevalence of high labor turnover and short employment spells (Illing *et al.*, 2024; Bertheau *et al.*, 2023; Szerman, 2023). By excluding workers with weak or marginal employment histories, the restriction helps isolate the effects of displacement from patterns of transitory labor force participation.

CNPJ is the business equivalent, used to identify establishments/firms and organizations.

[4]We only considered employer-initiated separations without cause. In RAIS, the category of the termination cause variable is "*Rescisão de contrato de trabalho sem justa causa por iniciativa do empregador*".

[5]For the control group, the restriction is applied based on the match year. That is, given all the restrictions, worker j is a potential counterfactual for worker i if, in the year of i's mass layoff, j had been employed in the same establishment for at least two years.

Third, we implemented a matching procedure to create a suitable control group for workers mass laid off. We match each treated worker with a control worker who (i) is not displaced in the same calendar year, and (ii) belongs to the same gender, birth cohort, education (4 categories), race (3 categories), tenure (11 categories) earnings category (percentiles), firm size (percentiles), sector of activity of the establishment (17 categories), and state (27). When treated individuals are matched with multiple controls, one control unit is randomly selected. This prevents weighting issues that may arise.

Table 1.1: Balanced Test

	Women			Men		
	Treated	Control	Difference	Treated	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Age	31.97	32.31	0.3468***	32.87	33.14	0.2765***
	(0.0123)	(0.0126)	(0.0177)	(0.0089)	(0.0091)	(0.0127)
Education						
Less primary	0.0927	0.0962	0.0034***	0.2265	0.2288	0.0023***
	(0.0005)	(0.0005)	(0.0003)	(0.0005)	(0.0005)	(0.0007)
Primary	0.1441	0.1607	0.0165***	0.2141	0.2334	0.0193***
	(0.0006)	(0.0006)	(0.0008)	(0.0005)	(0.0005)	(0007)
High school	0.6460	0.6281	-0.018***	0.5158	0.4916	-0.0242***
	(0.0008)	(0.0008)	(0.0011)	(0.0006)	(0.0006)	(0.0008)
College	0.1171	0.1149	-0.002***	0.0435	0.0461	0.0026***
	(0.0005)	(0.0005)	(0.0007)	(0.0002)	(0.0002)	(0.0003)
Wage	1,608.75	1,653.88	45.13***	2,049.73	2,061.22	11.48***
	(2.47)	(2.80)	(3.74)	(2.50)	(2.54)	(3.57)
Tenure (in years)	2.61	2.99	-0.3842***	2.72	3.32	0.6031***
	(0.0043)	(0.0053)	(0.0068)	(0.0032)	(0.0042)	(0.0053)
Occupational						
Blue Collar	0.5302	0.5081	-0.0221***	0.7858	0.7626	-0.0232***
	(0.0008)	(0.0008)	(0.0011)	(0.0005)	(0.0005)	(0.0007)
White Collar Lower Level	0.3056	0.3173	0.0117***	0.1062	0.1161	0.0099***
	(0.0007)	(0.0007)	(0.0010)	(0.0003)	(0.0003)	(0.0005)
Professional	0.1311	0.1373	0.0062***	0.0807	0.0912	0.0104***
	(0.0005)	(0.0005)	(0.0008)	(0.0003)	(0.0003)	(0.0004)
Manager	0.0330	0.0372	0.0042***	0.0271	0.0300	0.0028***
	(0.0003)	(0.0003)	(0.0004)	(0.0002)	(0.0002)	(0.0003)
Maternity Leave	0.1354	0.0995	-0.0359***	-	-	
	(0.0005)	(0.0005)	(0.0007)	-	-	
Sectors						
Industry[6]	0.2262	0.2198	-0.0064***	0.2673	0.2715	0.0042***
	(0.0007)	(0.0006)	(0.0009)	(0.0005)	(0.0005)	(0.0007)
Construction	0.0178	0.0131	-0.0047***	0.1699	0.1483	-0.0215***
	(0.0002)	(0.0002)	(0.0003)	(0.0004)	(0.0004)	(0.0006)
Commerce	0.3368	0.3467	0.0099***	0.2451	0.2565	0.0115***
	(0.0007)	(0.0007)	(0.0011)	(0.0005)	(0.0005)	(0.0007)
Services	0.4191	0.4203	0.0013***	0.3177	0.3236	0.0059***
	(0.0008)	(0.0008)	(0.0011)	(0.0005)	(0.0005)	0.0007
Informality Rate (%)	22.05	22.10	0.0554***	22.92	22.88	0.0380***
	(0.0080)	(0.0083)	(0.0116)	(0.0062)	(0.0063)	(0.0088)
Number of workers	382,499	382,499		761,234	761,234	

Note: Summary statistics are computed from RAIS data using the year before the mass layoff event. Standard errors are presented in parentheses. Columns (1) and (3) refer to a sample of workers fired in mass layoff events from 2008 to 2012. Columns (2) and (4) report summary statistics for the matched control group after implementing the matching algorithm. Further details on the matching algorithm are found in Section 1.3.1. The variables are: age, indicator variables for whether the worker has less than primary, primary, high school and college education, average earnings (in Brazilian reais), tenure, indicator variables for whether the worker holds a managerial, professional, white collar lower level and blue collar position, indicator variable for whether the women had maternity leave, indicator variables for economic sector the establishment belongs to (industry, construction, commerce and services) and informality rate in the city.

In our final sample, we successfully match 382,499 women and 761,234 men to a control unit. This corresponds to approximately 40 million observations, considering the period from 2003 to 2020.

Table 1.1 presents the balanced test by gender and treatment status using data for the year before the dismissal. Overall, the treated and control groups are well balanced across most observable characteristics. While many of the differences are statistically significant —likely due to the large sample size —their magnitudes are small, indicating that the matching procedure was effective in generating comparable groups.

However, the group of treated women differs from the group of treated men in observed characteristics. Dismissed male workers are, on average, less educated than dismissed female workers. About 24% of women have primary education or less, compared to 44% of men. Fired women earned an average of R$1,600, whereas men earned around R$2,050. Regarding tenure, both groups had, on average, approximately 2-3 years. This tenure is higher than the national average, reflecting the sample restriction to workers with at least two years of tenure prior to dismissal. This criterion was introduced to exclude high-turnover workers and focus the analysis on individuals with more stable labor market attachment.

Female and male workers also differ in occupational categories. 78% of men were employed in a blue-collar occupation, compared to 53% of women. The two sectors with the highest female participation are services and commerce (40% and 34%). Among men, the sectors with the highest participation are industry and commerce (29% and 26%). Regarding labor market characteristics, there are few differences between groups in informality rate.

In Section 1.4.1, we conducted a heterogeneity analysis to examine whether the differences in observed characteristics between treated women and men partially account for the differential effect of dismissal on formal employment.

1.3.2 Mass Layoff Events

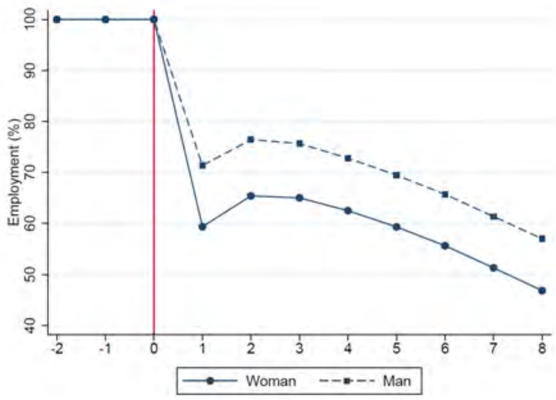
We use mass layoff events as an exogenous shock at the individual level. The main argument is that these events are predominantly driven by firm-level negative external shocks rather than by the characteristics or performance of the dismissed workers (Gathmann *et al.*, 2020).

To highlight the importance of using an exogenous event for identification, Figure 1.1(a) displays the employment trajectory of a random sample of workers laid off between 2008 and 2012, while Figure 1.1(b) shows the employment trends for workers laid off en masse and those in the control group, as outlined in Section 1.3.1.

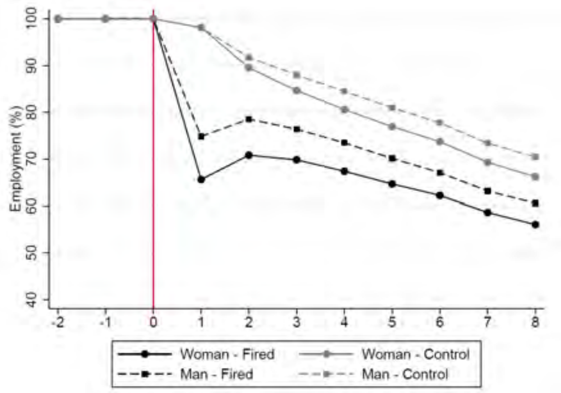
We observe that workers laid off in ordinary layoffs experience a proportionally larger decline in employment compared to those affected by mass layoff events. Endogenous factors such as poor worker-establishment matches, low productivity, or other individual characteristics may influence ordinary layoffs. As a result, considering dismissals more broadly introduces selection bias, potentially contaminating the analysis of unexpected dismissals.

Figure 1.1: Employment Evolution

(a) All fires - employment



(b) Mass layoff - employment



Note: The figure presents the percentual of employed workers per year, where year 0 corresponds to the year of dismissal. The sample includes individuals who were continuously employed for at least two years before dismissal. In the [-2;0] interval, all workers were employed (100%). Panel A includes all types of dismissals, while Panel B is restricted to mass layoffs.

Using mass layoff shocks we identify 452,901 events from 2008 to 2012 involving 1,143,733 workers who had been employed at the establishment for at least 2 years[7].

Figure 1.2 presents the characteristics of mass layoff events by establishment size, share of layoff, economic sector, and location. Panel (a) shows the percentage distribution of firms by firm size. Most of these events occurred in small establishmnets, particularly within the categories of 15-30 employees, which collectively represent more than 50% of the total. In particular, 90% of the events were concentrated in firms with fewer than 100 employees, reflecting the distribution of firm sizes in Brazil. For example, in 2010, approximately 90% of the formal establishments had fewer than 100 employees. (MTE, 2010).

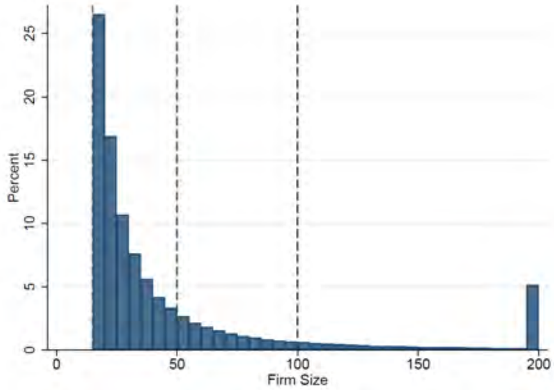
The distribution of layoff percentages during mass layoff events is shown in Panel (b) of Figure 1.2. Most events concentrated in the 30-40% layoff range, where nearly 30% of the observations occurred, indicating that less severe layoffs were

[7]Defining mass layoff as the dismissal of 33% or more of the workforce within a year in companies with 15 or more workers.

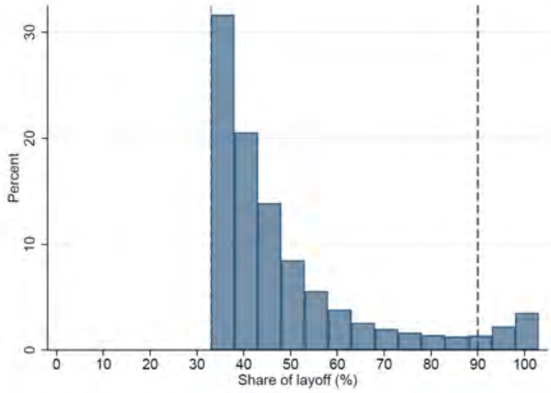
more frequent. The distribution is right-skewed, with a gradual decline in frequency as the layoff percentage increases. Events involving layoffs of more than 50% of the workforce were progressively less frequent, while extreme cases—where 100% of employees were laid off—are also present, highlighting possible business closures. The vertical red line at 33% likely represents a threshold for defining mass layoffs, with most events occurring close to this minimum threshold.

Figure 1.2: Mass Layoff Characteristics

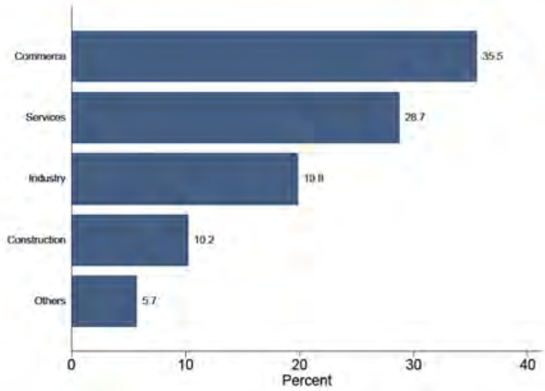
(a) Firm size



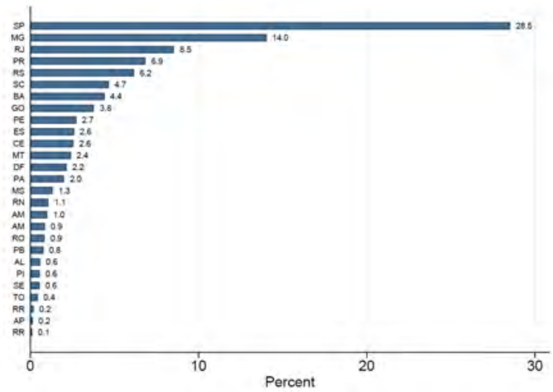
(b) Share of workers laid off



(c) Economic sectors



(d) States



Note: Panel A depicts the distribution of firm sizes among companies involved in mass layoff events during the analyzed period. The distribution starts at 15, reflecting the applied mass layoff definition: firms with at least 15 employees that dismissed at least 33% of their workforce in a given year. The thresholds of 50 and 100 are highlighted as they are used in alternative definitions of mass layoffs. Panel B illustrates the distribution of the share of workers dismissed in mass layoff events. The distribution begins at 33%, aligning with the mass layoff definition. The threshold of 90% is highlighted as it is considered in alternative definitions.C presents the distribution of firms involved in mass layoff events by industry, classified according to the CNAE sector.Panel D shows the distribution of firms involved in mass layoff events by state.

Mass layoffs were relatively well distributed across sectors (Figure 1.2(c)). 36.5% occurred in the commerce sector, followed by 28.7% in services, 19.8% in industry, and 10.2% in the construction sector. This distribution reflects the broad impact of economic shocks on different segments of the economy, with a particular concentration in labor-intensive sectors like trade and services. Figure 1.16(b) in

Appendix shows a similar distribution for all establishments in RAIS, except for the change in the order between the commerce and services sectors.

The distribution of mass layoff events across Brazilian states, as shown in Figure 1.2(d), is heavily concentrated in the Southeast and South regions of the country. São Paulo accounts for the largest share, with 28.5% of the events, followed by Minas Gerais (14.0%) and Rio de Janeiro (8.5%). Paraná (6.9%) and Rio Grande do Sul (6.2%) also contribute significantly. This concentration is consistent with the distribution of establishments in RAIS between 2008 and 2012 (Figure 1.16(c) in Appendix) .

1.3.3 Matching DiD using Mass Layoff Events

Our empirical strategy aims to identify the effects of job displacement on formal employment by gender. We use the difference-in-difference approach exploiting mass layoffs events for identification of causality.

Using matched treated and control workers, we estimate the following specification:

$$Y_{itg} = \beta_1 Treat_{ig} + \sum_{k=-2,k\neq -1}^{k=+8} \gamma_k \mathbf{1}(t = t^* + k) \times Treat_{ig} + \sum_{k=-2,k\neq -1}^{k=+8} \theta_k \mathbf{1}(t = t^* + k) + \beta X_{itg} + \delta_t + e_{itg}, \tag{1-1}$$

in which subscripts i, t and g stand for worker, year and gender. Formal employment is a dummy equal 1 if the worker was employed in the formal sector in t, 0 otherwise[8]; $Treat_{ig}$ is a variable that indicates whether worker i was treated, or involved in a mass layoff; $\mathbf{1}(t = t^* + k)$ are dummies indicating the distance k for the mass layoff year t^*; X_{itg} is a set of control variables, as age dummies and education; and δ_t are year fixed effects. Year fixed effects control for common shocks affecting workers each year. Standard errors are clustered at *city* $\times$ *year* level. As a robustness check, I also estimate models clustering at the individual (worker) and state $\times$ year levels (see Figure 1.19 in Appendix). In addition, to account for time-varying shocks specific to sectors and occupations, we include $Industry_i \times \delta_t$ and $Occupation_i \times \delta_t$ (Figure 1.18 in Appendix)[9].

To summarize the average effect over all periods, we also estimate the equation:

$$Y_{itg} = \beta_1 Treat_{ig} + \gamma Post_{ig} \times Treat_{ig} + \theta Post_{ig} + \beta X_{itg} + \delta_t + e_{itg} \tag{1-2}$$

[8]If we do not observe worker i employed in any year t, we impute the value zero in the employment variable. In these cases, all fixed variables of the individual are imputed, but variables that depend on the employment relationship or that change over time are considered missing.

[9]We use the occupations and industries that worker i was engaged in at the time of dismissal or in the year of the match (for the control group).

where the dummy $Post_{ig}$ identifies the entire period after layoff, and all other variables are defined as in Equation 1-1.

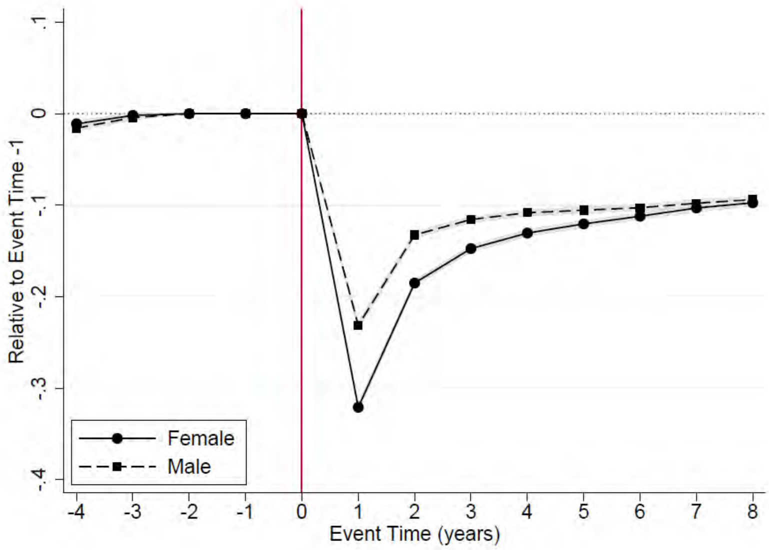
Our identification hypothesis relies on two assumptions. First, exogeneity of the job displacement at individual level. We argue in Section 1.3.2 that mass layoff are less likely to be correlated with workers' observable or unobservable characteristics. Second, we also aim to ensure that, in the absence of the mass layoff, the outcomes for the treated and control groups would have followed parallel trends for $k > 0$. To test this assumption, we show that the pre-event employment rate and wage in are very similar (Figure 1.14 in Appendix 1.6).

1.4 Effect of Job Displacement on Formal Employment by Gender

Figure 1.3 presents the effect of job loss due to a mass layoff on formal employment by gender. Each point estimate (γ_k) represents the comparison between treated groups with their respective controls[10].

The probability of formal employment declines by 32% for displaced women and 23% for displaced men in the first year after mass layoff. For both men and women, there is a strong recovery in employment in the following 3 years. However, the effect appears to be persistent, around 10%, from the fifth year onwards for men, with a tendency for a slight recovery for women.

Figure 1.3: Effects of Job Displacement on Formal Employment by Gender



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

[10]See Figure 1.1(b) to observe the employment path for both groups and gender.

The formal participation gap between women and men is approximately 9 percentage points in the year following mass layoffs. This gap narrows over time, reaching almost zero in the eighth year.

Table 1.2 summarizes the average effect in the first 1 and 8 years after the mass layoff. As shown in the Figure 1.3, the impact on formal employment is greatest in the short run. The effect for displaced women is, respectively, 32%, and 14.6%. For displaced men, the impact is 23%, and 12%. Column (3) of the Table 1.2 shows the difference in effect between women and men.

Table 1.2: Difference-in-differences estimates: Effects of Job Displacement on Formal Employment by Gender

	Female	Male	Difference
	(1)	(2)	(3)
1 year: Treat × Post	-0.3208***	-0.2313***	-0.0890***
	(0.0023)	(0.0023)	(0.0020)
8 years: Treat × Post	-0.1460***	-0.1197***	-0.0275***
	(0.0025)	(0.0015)	(0.0026)
Number of workers	764,998	1,522,468	

Note: Each cell represents the estimate of the difference-in-differences parameter (γ) from Equation 1-2. Column (1) presents an estimation of the female sample and Column (2) for the male sample. Column (3) shows the statistical difference between columns (1) and (2). "1 year: Treat × Post" means the average effect one year after dismissal; "8 year: Treat × Post" means the average effect of the eight years after dismissal. Regressions include covariates and year-fixed effects. Standard errors are clustered at the city-year level and presented in parentheses.

We implement additional checks to confirm the robustness of the results. Our results are robust to the inclusion of individual fixed effects, sector and occupation-specific fixed effects, and variation in the level of clustering of the error term (Figures 1.17, 1.18 and 1.19 in Appendix).

We also test alternative mass layoff definitions. In our main estimation, we followed Britto *et al.* (2022): establishments with at least 15 workers that laid off 33% or more of their workforce within a year. In Figures 1.20, 1.21 and 1.22 on Appendix we vary the establishment size (15, 50 and 100 employees) and the lower threshold for the percentage of laid-offs employees (33% and 90%). The results show that the estimated coefficients remain quite similar to those in the baseline estimation.

However, when we consider all layoffs, the observed effect becomes significantly larger (more negative)[11]. This may be directly related to selection bias in general

[11] All layoffs means that no definition of mass layoff is applied to identify laid-off workers

displacement. For example, workers being laid off due to low productivity, who would have more difficulty finding a job. We argue that mass layoffs are more likely to be exogenous to individual characteristics.

The evidence presented here is consistent with the existing literature. Job displacement has a negative and permanent effect on the probability of being employed (Britto *et al.*, 2022; Bertheau *et al.*, 2023; Corado, 2023; Schmieder *et al.*, 2023; Ivandić & Lassen, 2023) and the effects are bigger for women (Meekes & Hassink, 2022; Ivandić & Lassen, 2023; Illing *et al.*, 2024)[12].

Our findings suggest that the effect is significantly larger for women in the short term (9 percentage point), and it takes approximately 8 years for the gender employment gap to close after a job displacement. Illing *et al.* (2024) for German context find that, in the year of job loss, the employment effect is 3 percentage points larger for women than for men. However, this gap closes by the second year post-dismissal for women with characteristics similar to those of dismissed men. In the Netherlands, Meekes & Hassink (2022) report a 9 percentage point larger employment effect for women in the first month after dismissal. The gap narrows quickly, stabilizing at around 2 percentage points from the second year onward. Ivandić & Lassen (2023) analyze the long-term effects of job displacement on labor market outcomes in Denmark. They find that women face a higher risk of unemployment and experience larger earnings losses in the two years following displacement. Even after controlling for differences in human capital, a significant gender gap in unemployment remains. Using a standard decomposition framework, the authors identify child care responsibilities as a key barrier to women's reemployment.

Comparing our results with those of other countries requires highlighting key aspects of the Brazilian context. Brazil exhibits lower female participation in both the labor force and formal employment, which can influence how job displacement affects women. Between 2008 and 2019, the female participation rate was about 55-58%, while in Germany and the Netherlands it was 70% (ILO, 2020). The Brazilian labor market is also characterized by high rates of informality, meaning that many workers are not officially registered or protected by labor laws. In recent years, around 40% of the country's workforce was employed in informal jobs (IBGE, 2020). Additionally, Brazil has significant migration between different positions within the labor market, as workers often move between formal, informal, unemployment and

[12] Auxiliary estimates in Bhalotra *et al.* (2021) reveal minimal differences in the unemployment response between women and men using Brazilian data. Discrepancies between these findings and those of the present study might be attributed to variations in sample restrictions and the analysis period. Since the authors did not primarily investigate this result, direct comparisons are challenging. Furthermore, our findings are in line with the findings of the international literature.

inactivity (Cuco & Souza, 2019).

These factors make our findings an interest contribution to the literature. Much of the existing evidence on job displacement is based on data from developed or high-income countries, where labor markets tend to be more stable and formal. In these contexts, women may experience different labor market dynamics than in Brazil and Latin America, where informality and job insecurity are present. By focusing on Brazil, our study provides insights that could be relevant to other developing countries facing similar labor market challenges.

Several factors may explain the gender gap in formal employment following job displacement. First, women may reconsider their decision to supply labor. On the one hand, they face greater barriers to (re)employment, including gender discrimination, limited availability of flexible work, and closer to home (Bertrand *et al.*, 2004; Booth & Van Ours, 2013; Le Barbanchon *et al.*, 2021; Ho *et al.*, 2024). On the other hand, family responsibilities and shifting personal priorities can drive women to permanently exit the labor market, such as maternity and family arrangements (Kleven *et al.*, 2019). In this analysis, 14.6% of female workers fired in a mass layoff do not return to the formal sector in the eight years following their dismissal. For male workers, the proportion is about 10%. More in-depth analyses are challenging since we do not have available data that capture information on the reemployment decision process in the Brazilian formal labor market.

Second, women tend to experience longer job search durations after displacement (Kunze & Troske, 2012; Ivandić & Lassen, 2023). This may be due to challenges in finding a job or preferences that limit the pool of available positions — stemming from the same barriers that may lead women to exit the labor market. In this analysis, we found that 82.6% of male workers who return to formal employment do so in the year following their dismissal, while among women the proportion is 75.3%. In contrast, in the second and third year the return percentage for women is 12.5% and 5.4%, while for men it is 9.8% and 3.4% (Figure 1.15 on Appendix 1.6).

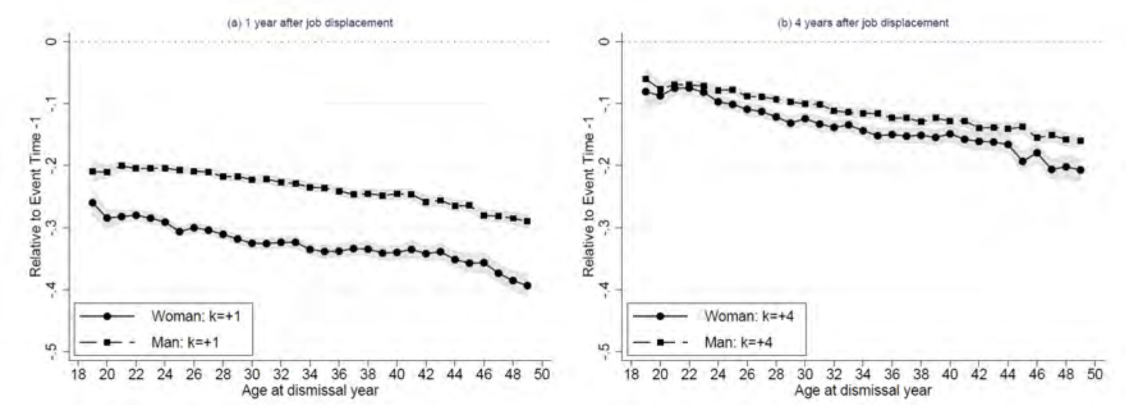
In Section 1.5, we test the hypothesis that women take longer to return to employment as they wait for jobs of comparable wage to those they held prior to their dismissal.

1.4.1 Heterogeneity

To better understand the results, it could be useful to investigate whether differences in observable characteristics play a role. Especially considering that the group of treated women differs, on average, from the group of treated men. We conducted this analysis considering individual characteristics, employment contracts, firms, and the local labor market.

Age: A possible consensus on the findings is that labor market shocks may impact women and men differently throughout the life cycle. To explore this hypothesis, we estimated the effects for women and men at different ages at dismissal year. Figure 1.4 shows that the older the worker, the greater the effect of dismissal in the first (a) and fourth (b) years post-displacement. The gap is larger in the first year but constant throughout the life cycle. With this evidence, age differences at the time of the shock do not appear to explain the result found.

Figure 1.4: Heterogeneous Effects by Age

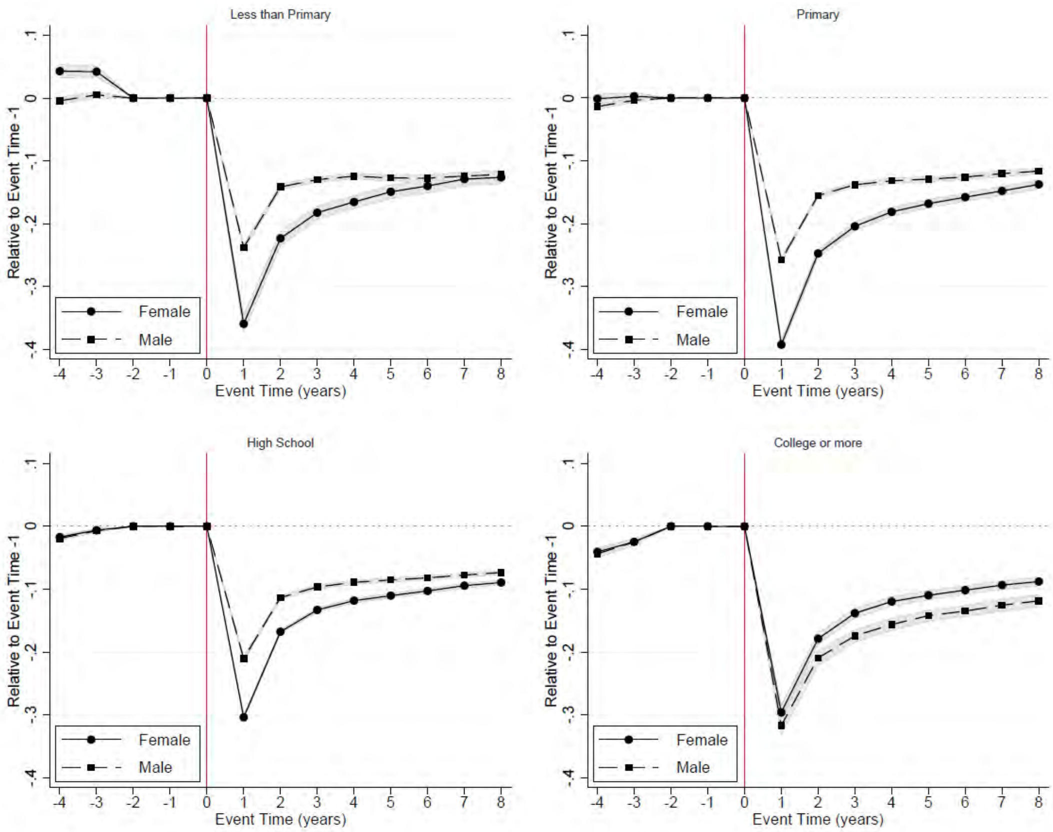


Note: The figure presents estimates of the parameters γ_{+1} and γ_{+4} in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Education: Figure 1.5 shows the effect of mass layoff for each education category. For less-educated workers (Panels (a) and (b)), the effect for women in the year following job displacement is greater than that for men by 12.2 and 13.4 percentage points. The gap closes for workers with an education level lower than primary starting from the sixth year onwards. However, for workers with primary education, the gap remains relatively constant from the sixth year onwards.

For more educated workers (Panels (c) and (d)), there is evidence that women with high school education are more affected initially (a difference of 9 percentage points compared to men), but the gap stabilizes at around 2 percentage points starting from the sixth year after the mass layoff. For workers with a college education or higher, the effects are similar for men and women in the year following the layoff. However, the gap tends to widen from the second year onward, with men being more adversely affected.

Figure 1.5: Heterogeneous Effects by Education

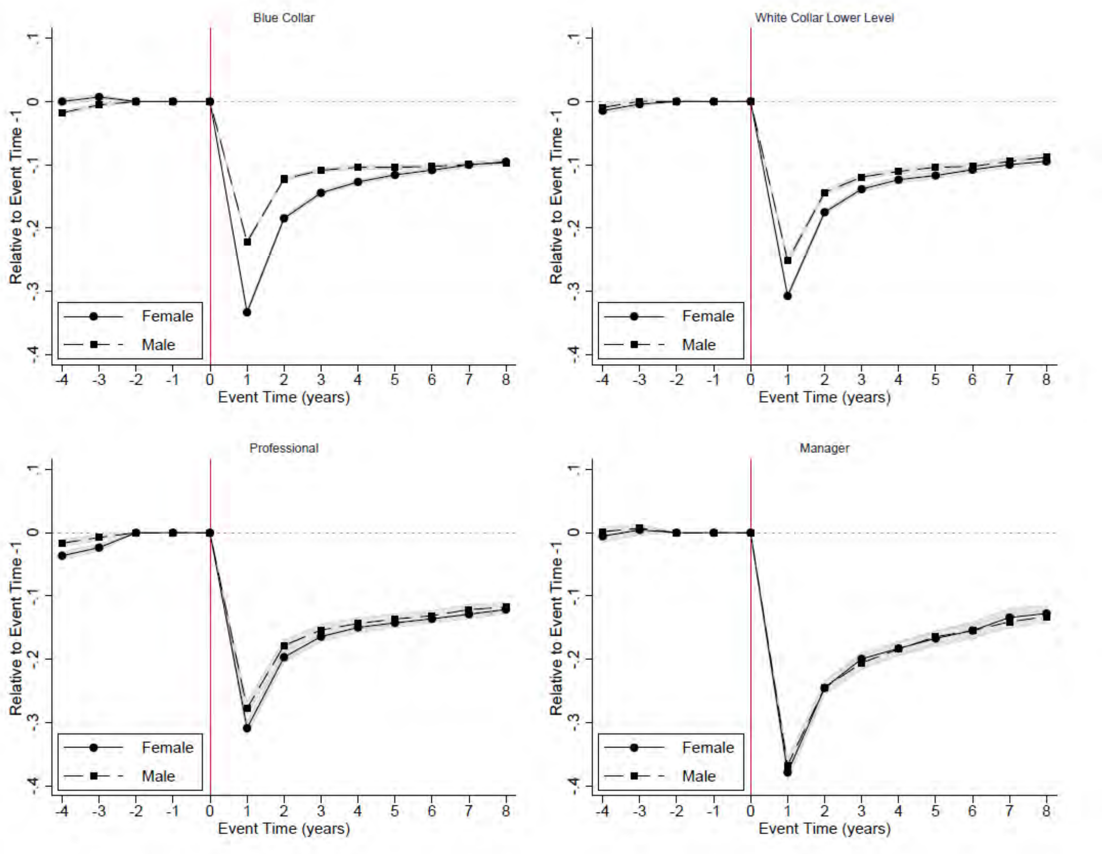


Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. "Less than Primary" contains 70,268 women and 327,944 men. "Primary" contains 113,381 women and 323,072 men. "High School" contains 473,125 women and 728,858 men. "College or more" contains 85,701 women and 64,974 men. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

The different effects across education levels may be directly related to the type of job held at the time of displacement and the outside options available in the local labor market. Less-educated workers may be more likely to migrate to informal positions or self-employment. On the other hand, more-educated workers may transition into entrepreneurship (Dal-Ri, 2024).

Occupation: Figure 1.6 shows the analysis of the impacts, separated into four occupation categories: blue collar, white collar lower level, professional, and manager. The effect of dismissal appears to be more concentrated among blue-collar workers, who represent 53% of women and 78% of men in the sample. For workers in professional and managerial occupations, there is no evidence of a gender differential effect. Furthermore, the impact is greater for managers, with a 40% decrease in the probability of being employed in the year following the dismissal. Dal-Ri (2024) shows that after a mass layoff managers are 5 percentage points more likely to start longer-lasting businesses.

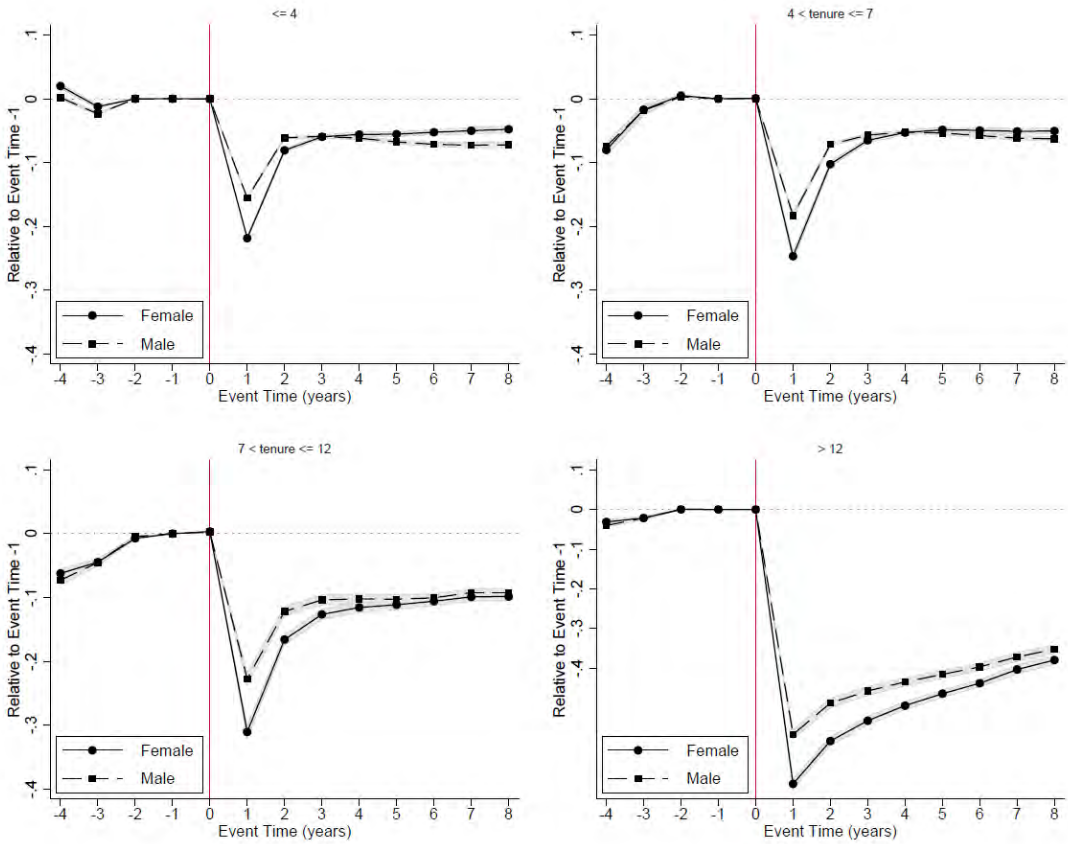
Figure 1.6: Heterogeneous Effects by Occupations



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. “Blue Collar” contains 403,136 women and 1,185,004 men. “White Collar Lower Level” contains 241,465 women and 171,484 men. “Professional” contains 103,866 women and 131,968 men. “Manager” contains 27,193 women and 43,890 men. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Tenure: Figure 1.7 shows the effect for four tenure categories: up to 4 years, between 4 and 7 years, between 7 and 12 years, and over 12 years. It is interesting to note that, in all categories, the effect for women is greater than the effect for men. Moreover, the longer the tenure, the greater the decrease in the probability of being employed. The variation in the probability of employment for women at k=+1 is approximately 70%, while for men it is 57%. The evidence is consistent with what the literature says about the accumulation of firm-specific capital, which is not easily transferable to other companies and/or sectors (Becker, 1975).

Figure 1.7: Heterogeneous Effects by Tenure

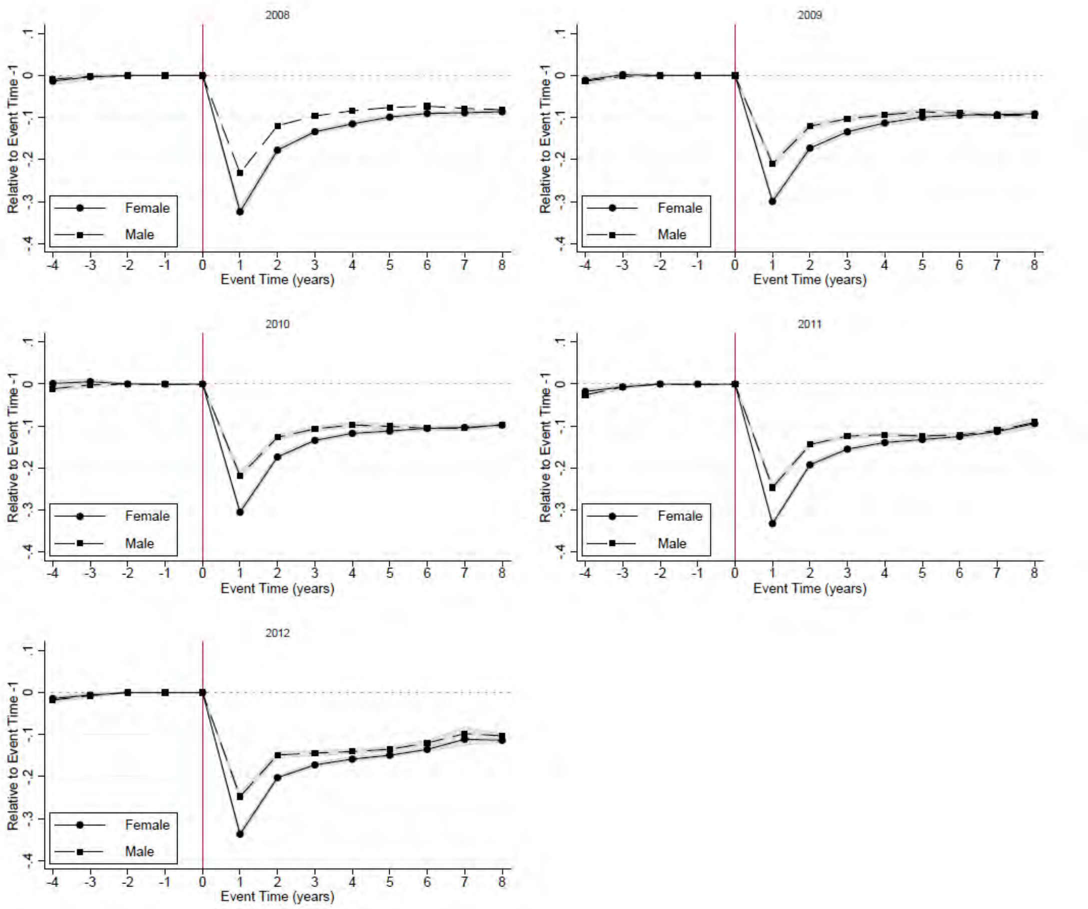


Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. "<=4" contains 237,355 women and 503,901 men. "4<tenure<=7" contains 172,233 women and 353,271 men. "7<tenure<=12" contains 189,843 women and 353,307 men. ">12" contains 176,345 women and 323,195 men. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Year of Mass Layoff: Figure 1.8 presents the effect for the year of mass layoff to test whether the economic cycle can affect the response to a negative employment shock. The mass layoff analysis period, 2008 to 2012, encompasses years of growth and recession in the Brazilian economy[13]. However, there is no evidence of a heterogeneous effect according to the year of dismissal.

[13]In 2008, 2010, 2011 and 2012, Brazil's GDP grew by 5.2%, 7.5%, 2.7% and 0.9% respectively. Only in 2009 was the variation negative at -0.2%, as a result of the 2008 subprime crisis.

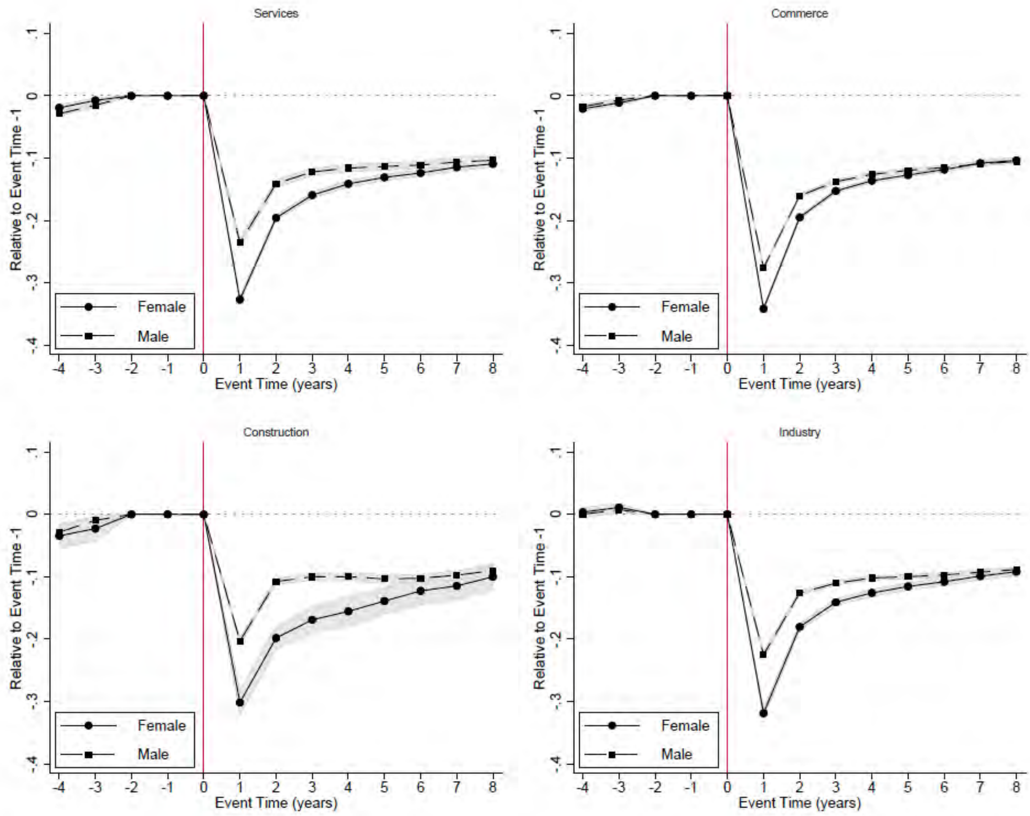
Figure 1.8: Heterogeneous Effects by Year of Mass Layoff



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. "2008" contains 130,750 women and 279,164 men. "2009" contains 139,208 women and 295,466 men. "2010" contains 154,802 women and 314,120 men. "2011" contains 175,646 women and 317,700 men. "2012" contains 175,552 women and 327,224 men. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Sector: Figure 1.9 presents the effect of mass layoff for four sectors: services, commerce, construction and industry. There is little difference in the employment trajectory of women and men after dismissal across sectors. For workers in the service and industry sectors, the probability of employment and the gender gap exhibit very similar patterns. In the service sector, although the effect on women is stronger at k=+1, the difference for men is smaller and decreases more quickly. In the construction sector, where male participation is dominant, dismissed male workers are able to relocate more quickly. Women, on the other hand, show the same behavior as women in other sectors.

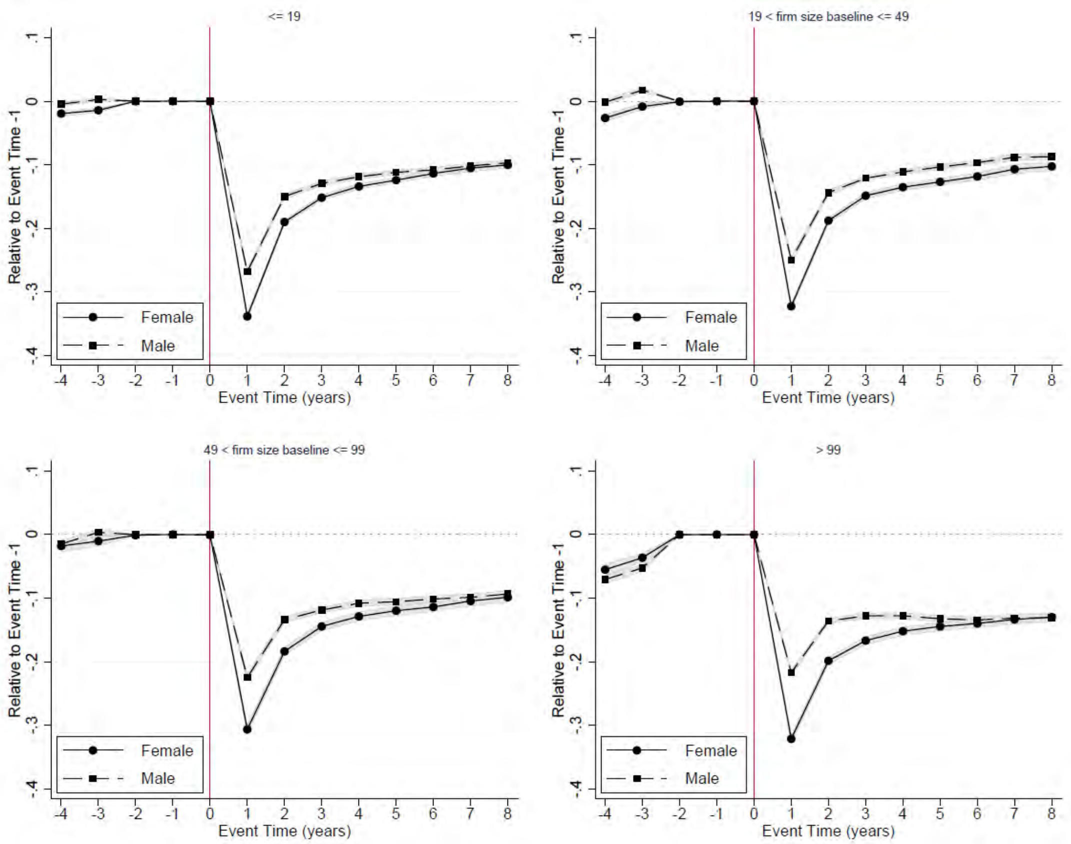
Figure 1.9: Heterogeneous Effects by Economic Sector



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. "Industry" contains 170,391 women and 413,344 men. "Construction" contains 11,758 women and 242,036 men. "Commerce" contains 260,285 women and 380,699 men. "Services" contains 298,582 women and 371,149 men. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Firm size: Figure 1.10 shows the results subdivided by the size of the establishment in the year of the mass layoff. It is interesting to note that the larger the establishment, the larger the initial gap in participation. This is because men return to the market faster than women.

Figure 1.10: Heterogeneous Effects by Establishment Size



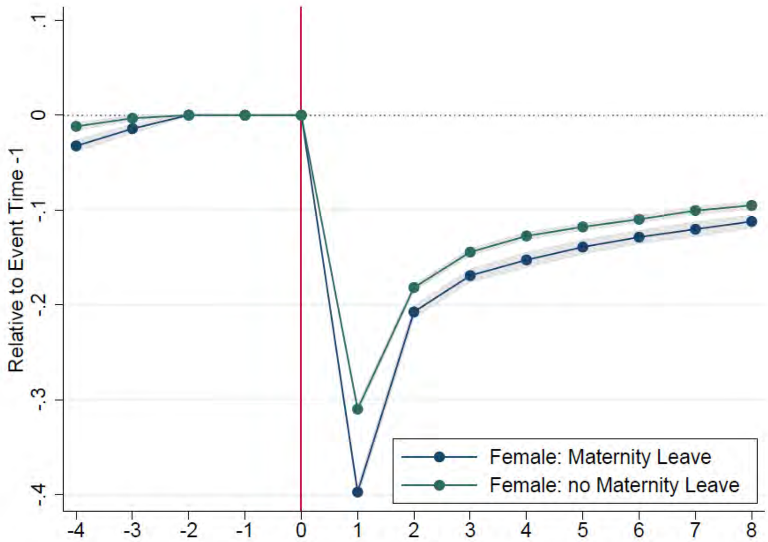
Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. "<=19" contains 337,300 women and 513,329 men. "19<tenure<=49" contains 145,224 women and 272,575 men. "49<tenure<=99" contains 70,860 women and 161,626 men. ">99" contains 222,392 women and 586,144 men. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Maternity Leave: The literature has strong evidence that the child penalty partially explains differences between women and men in the labor market. Kleven *et al.* (2019) show that, after the birth of a child, women experience a 13% decline in labor market participation and a 20% reduction in earnings. In contrast, the effects for men are not statistically significant.

To test whether our results are driven by motherhood, we use the maternity leave information available in the RAIS from 2007 onwards. This data has two limitations. First, maternity leave can only be observed if the woman was employed in the formal sector (and therefore included in the RAIS dataset). The sample is restricted to workers employed for at least two years before the mass layoff. This ensures that we observe motherhood for at least two years prior to the dismissal. However, we are unable to identify if it is the first child. Second, we do not have information on paternity.

Figure 1.11 shows the effect of mass layoff divided between women who took maternity leave and women who did not take maternity leave in the period of analysis. Women who took maternity leave before dismissal experience a larger effect on the probability of being employed after dismissal (about 40% at k=+1). However, the effect for women who did not take maternity leave is similar to the effect for women in general (Figure 1.3). This result suggests that motherhood affects women even more, but does not explain the gap with men.

Figure 1.11: Heterogeneous Effects by Maternity Leave



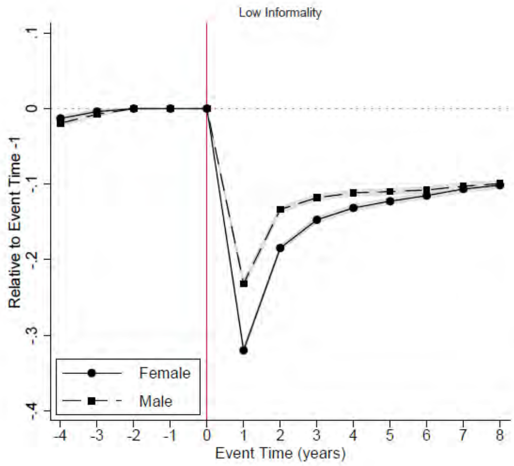
Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. "Maternity Leave" contains 90,499 women. "No Maternity Leave" contains 685,277 women. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Informality: Transitions to other types of employment may help explain the failure to return to the formal labor market after dismissal. To test this hypothesis, we use data on the informality rate in the microregion where the individual was employed at the time of dismissal[14]. Regions with higher informality rates are expected to have more informal employment opportunities, which may influence the supply of formal jobs.

Figure 11 shows the results dividing workers into locations with lower and higher informality rates, using the national informality rate in 2010 as the threshold[15]. We observe that the gap in formal employment between women and men at k=+1 is 1.5 percentage points larger in regions with more informality (Panel b). That is, the post-dismissal employment gap is persistent even in locations with low levels of informality.

[14]Data from the 2010 Census (IBGE).
[15]In 2010, the informality rate in Brazil was 27%.

Figure 1.12: Informality



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. "Low Informality" contains 654,100 women and 1,222,793 men. "High Informality" contains 121,666 women and 310,881 men. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

1.4.2 Back-of-the-envelope Calculation

In this section, we present a simple back-of-the-envelope calculation to estimate the extent to which gender differences in reemployment rates contribute to the gap in formal labor force participation.

We define $N_{k,t}$ as the total number of working-age individuals of gender $k \in w, m$ at time t. Individuals can either be formally employed ($F_{k,t}$) or not in formal employment ($UI_{k,t}$), where the latter includes unemployed individuals, informal workers, and those out of the labor force. Thus, the identity $N_{k,t} = F_{k,t} + UI_{k,t}$ holds. The number of formally employed men and women at time $t+1$ consists of those who remain in formal employment, those who exit, and those who enter, as summarized in the following equation:

$$F_{k,t+1} = \Big(1 - \delta_k - (1 - \alpha_k)\beta_k\Big)F_{k,t} + \gamma_k UI_{k,t} \tag{1-3}$$

where δ denotes the share of formal workers who voluntarily leave the formal labor force, β is the share who are dismissed, α represents the share of dismissed workers who find a new formal job within the same year, and γ denotes the share of individuals outside the formal sector who enter it.

In steady-state, $F_{k,t+1} = F_{k,t} = F_k$, implying that

$$F_k = \Big(1 - \delta_k - (1-\alpha_k)\beta_k\Big) F_k + \gamma_k UI_k \tag{1-4}$$

$$N_k = F_k + UI_k \tag{1-5}$$

From the equations above, it follows that:

$$\frac{F_k}{N_k} = \frac{\gamma_k}{\delta_k + (1-\alpha_k)\beta_k + \gamma_k} \tag{1-6}$$

This expression defines the steady-state share of individuals in formal employment as a function of separations, reemployment, and entry into the formal sector.

We calibrate δ_k and β_k using RAIS data, based on average observed shares of voluntary quits and dismissals between 2008 and 2012. The parameter α_k capturing reemployment rates, is taken directly from our main empirical results (Table 2.9, first row) , and corresponds to the estimated share of displaced formal workers who regain formal employment within one year. Finally, the parameter γ_k is selected to match the observed steady-state share of formal employment $\frac{F_k}{N_k}$ for each gender, ensuring internal consistency.

Table 1.3: Back-of-the-Envelope Calculation

			Men	Women	Counterfactual Women
			(1)	(2)	(3)
		Calibration:			
Parameter	Symbol	Target/Source			
Share quit (form formal job)	δ	Average share of workers who resigned	5.1%	4.3%	4.3%
Share fired (from formal job)	β	Average share of dismissed workers	15.8%	22.0%	22.0%
Share hired (from non-formal)	γ	Match F_k/N_k	3.8%	2.6%	2.6%
Share of fired workers (from formal job) who return in the same year	α_k	Table 1.2, first row	**76.9%**	**66.7%**	**76.9%**
		Model moments and counterfactual:			
Share in the formal sector	F_k/N_k		30.3%	18.3%	21.7%
Formal employment gender gap			12%		8.6%
		Model fit (data):			
Share in the formal sector	F_k/N_k		30.2%	18.4%	

Note: The data cover the period from 2008 to 2012. In this period, Brazil had, on average, approximately 150 million people aged 15–64 and around 36 million formal sector workers, of which 22.1 million were men and 14.7 million were women. The moments for δ and β are sourced from RAIS (2008–2012).

Using this framework, we simulate a counterfactual scenario in which women experience the same reemployment rate as men (i.e., $\alpha_w = \alpha_m$). As shown inTable 1.3, the formal employment gender gap was 12 percentage points in the data: 30.3% of men and 18.3% of women held a formal job. Under the counterfactual where women's reemployment rate equals that of men (keeping all other parameters fixed), the predicted formal employment share for women would rise from 18.3% to 21.7%, reducing the gender gap to 8.6 percentage points. This suggests that differences in reemployment rates alone account for approximately 28% of the observed formal employment gender gap.

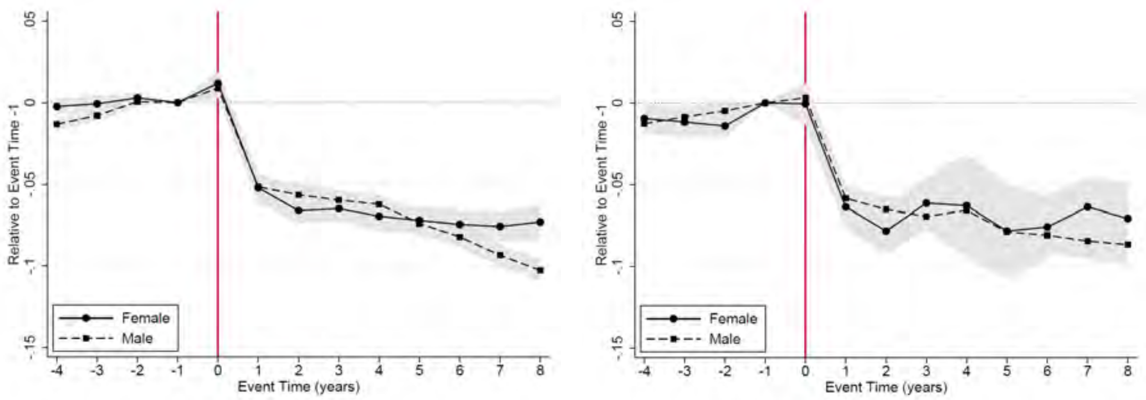
1.5 Do Female Workers Wait for Better Jobs?

In this paper, we show that women have their employment trajectories more affected by a layoff than men. One possible explanation is that women take longer to achieve a reallocation due to different parameters in the job search (Kunze & Troske, 2012; Ivandić & Lassen, 2023). In this sense, we analyzed the wage variable to test whether women are waiting to return to the formal job market in positions as good as those they had before.

Conditional on getting a formal job after dismissal, Figure 1.13(a) plots the change in monthly wage based on compensation in the year prior to the dismissal event. Women and men experience a negative wage variation in the order of 5% in the first year, reaching 10% for men and 8% for women in $k = +8$. Figure 1.13(b) shows a negative variation also in the hourly wage. The decrease in wages was not accompanied by a decrease in working hours.

Figure 1.13: Effects of Job Displacement on Wages by Gender

(a) Δ monthly wage (b) Δ hourly wage



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $\Delta Wage_{it}$ variable is the change in wage based on the wage of $k = -1$. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

The initial hypothesis is not supported by empirical evidence. In contrast, female and male workers who return to formal positions receive lower wages than they received before the layoff. This result suggests that women who take a long time to find reemployment are possibly due to difficulties in finding positions, and not because they are choosing better offers.

Our results align with the job displacement literature, which consistently documents a decline in earnings following layoffs (Jacobson *et al.*, 1993; Britto *et al.*, 2022; Bertheau *et al.*, 2023; Corado, 2023). From a gender perspective, the evidence is mixed: Ivandić & Lassen (2023) and Illing *et al.* (2024) find a small difference in earnings losses between men and women, whereas Meekes & Hassink (2022) provide evidence that women experience a slight earnings increase after layoffs compared to men.

The key distinction in our findings lies in the earnings trajectory. While the literature generally suggests a recovery over time, similar to employment trends, our results document a persistent shift and, in some cases, a further decline as the time since layoffs increases. This downward trend may be explained by the cumulative impact of multiple layoffs.

1.6 Final Considerations

This paper sheds light on the differential effects of job displacement on formal employment outcomes for men and women in Brazil, offering valuable insights into the labor market dynamics of a middle-income country. By leveraging a large-scale administrative dataset (RAIS), we identify and analyze the long-term consequences of mass layoffs—an exogenous shock to employment—on the formal employment trajectories of men and women. Our findings confirm that women face a significantly greater decline in formal employment following displacement, with a gap that, although narrowing over time, remains substantial even after eight years.

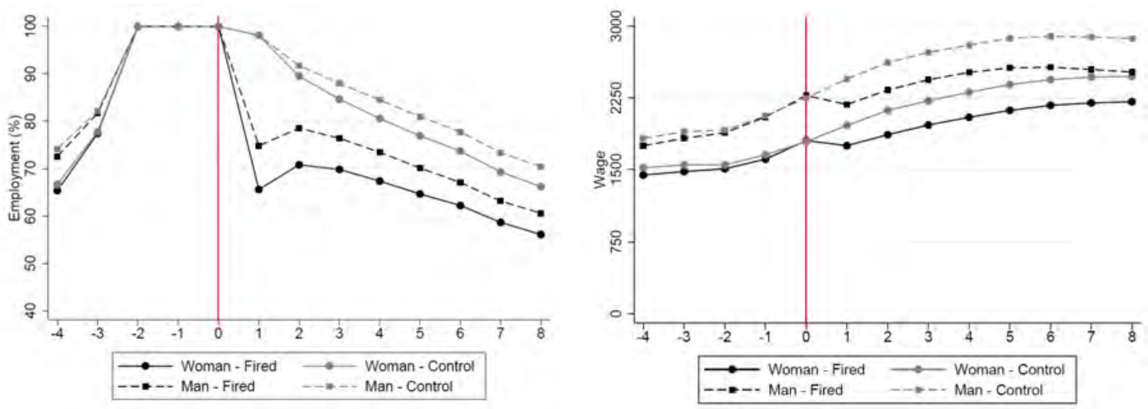
The results suggest that labor market frictions, particularly the delayed reentry into formal employment for women, contribute substantially to the persistent gender gap in formal labor force participation. We show that gender differences in reemployment rates account for approximately 28% of the overall gender gap in formal employment. Moreover, we identify key factors that exacerbate this gap, including educational background and occupational type. These findings are consistent with broader gender inequalities in labor market access and recovery.

Our study contributes to the literature by documenting the significant role of job displacement in exacerbating the gender gap in formal employment, particularly in a developing country context. While much of the existing research on this topic has focused on high-income countries, our work underscores the unique challenges faced by women in middle-income settings like Brazil. These challenges are not only a result of gender-specific labor market dynamics but are also compounded by broader structural factors, such as the high levels of informality and the limited social protection for displaced workers.

Appendix

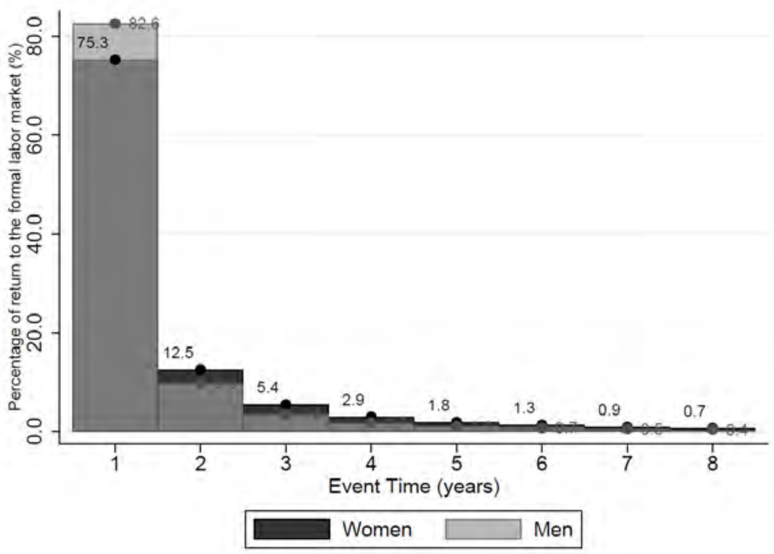
Figure 1.14: Employment and wages evolution - treated and control

(a) Employment (b) Wages



Note: The figure presents the percentage of employed workers per year. The sample was restricted to workers employed for at least 2 years before dismissal. Between [-2;0] all workers were employed (100%). Prior to $k = -2$, no employment restrictions were applied.

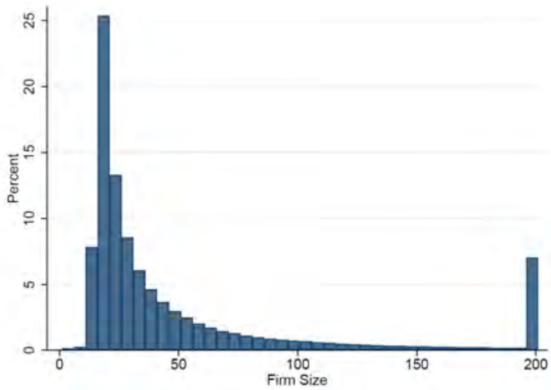
Figure 1.15: Return to the formal labor market



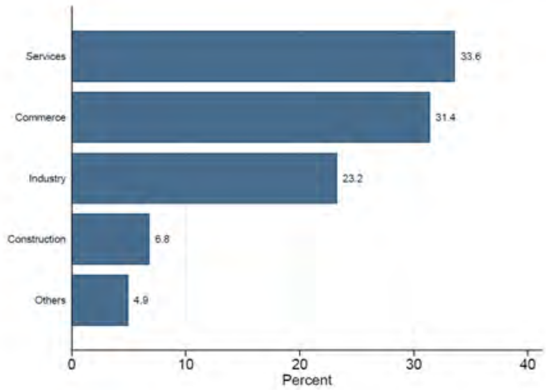
Note: The figure shows the percentage of workers laid off in mass layoff events who return to the formal labor market each year after being laid off.

Figure 1.16: RAIS Establishments' Characteristics

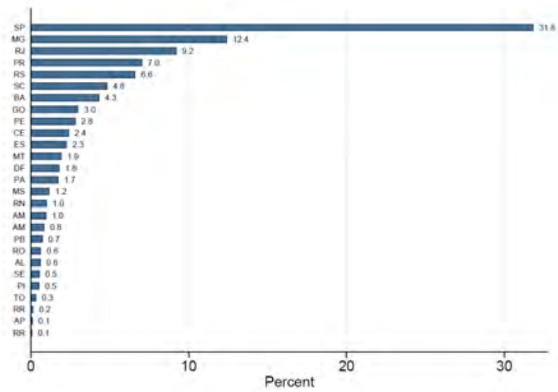
(a) Firm size



(b) Economic sectors

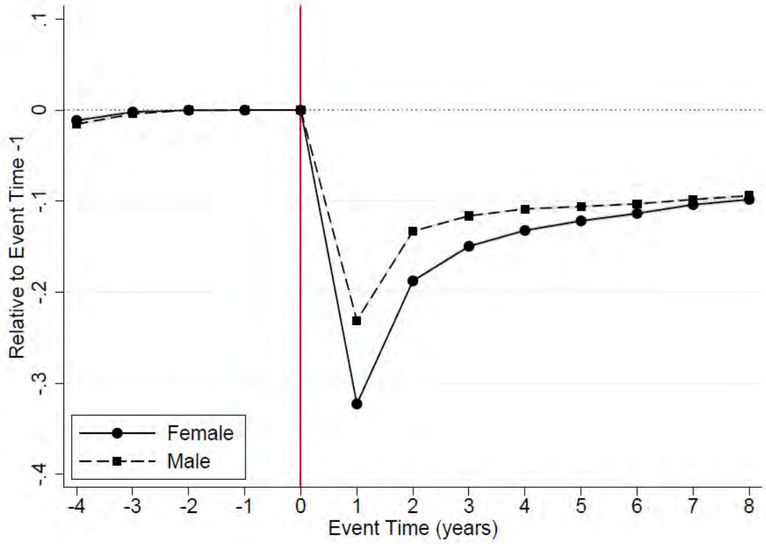


(c) States



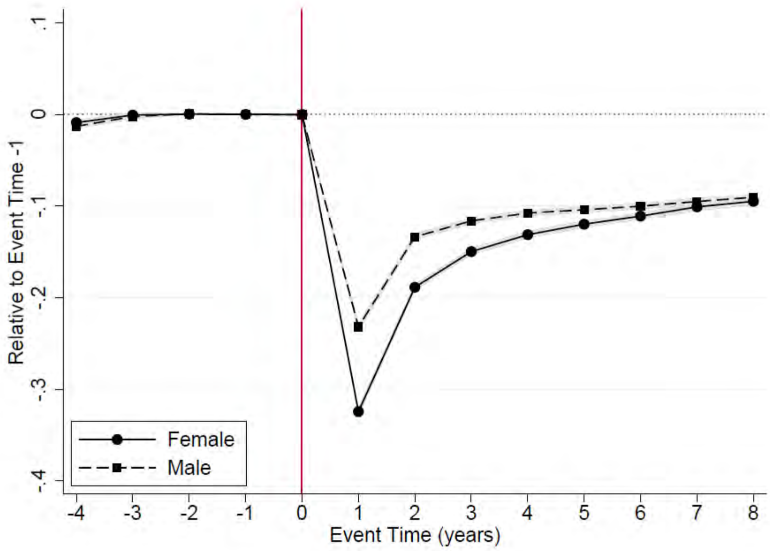
Note: Panel A depicts the distribution of firm sizes among the universe of establishments in RAIS between 2003 and 2018. Panel B presents the distribution of establishments by industry, classified according to the CNAE sector. Panel C shows the distribution of firms by state.

Figure 1.17: Robustness - Worker fixed effects



Note: The figure presents estimates of the parameters γ_k in Equation 1, including worker fixed effects. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

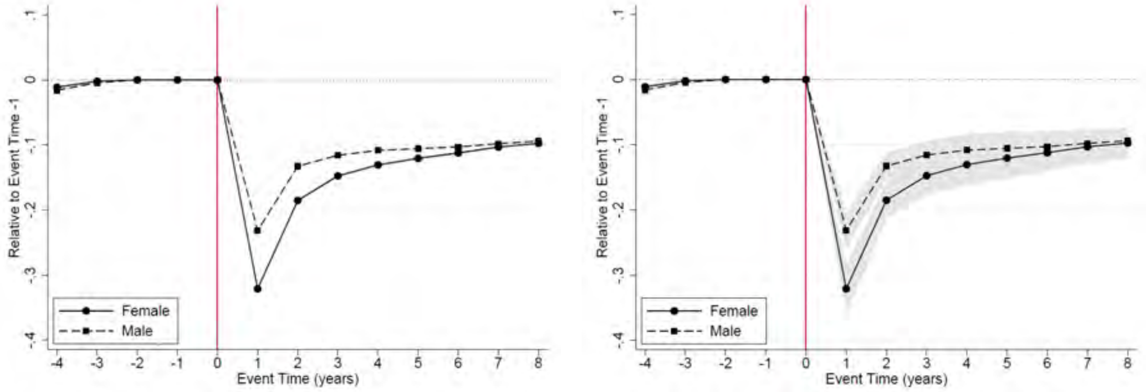
Figure 1.18: Robustness - Sector and occupation-specific fixed effects



Note: The figure presents estimates of the parameters γ_k in Equation 1, including sector and occupation-specific fixed effects. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level.

Figure 1.19: Robustness - Test of the cluster level of the error term

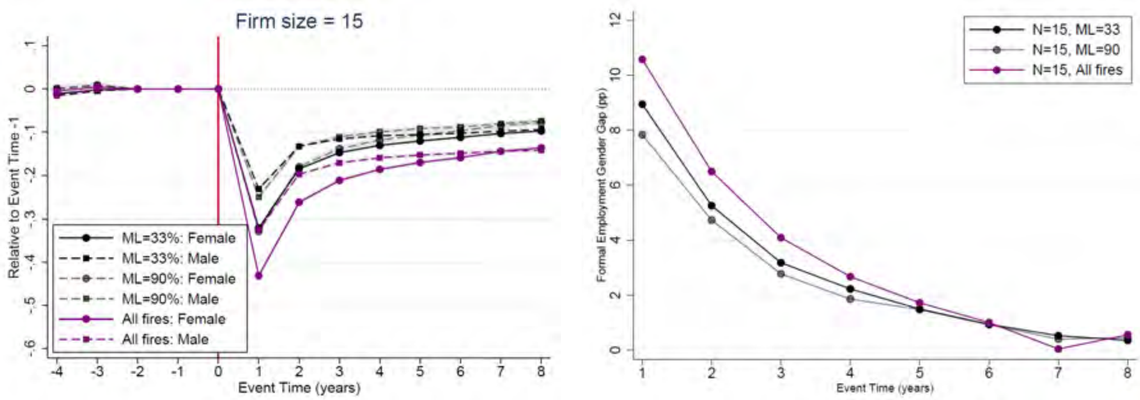
(a) Cluster Worker Level (b) Cluster State Level



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the worker and state levels (Panel A and B, respectively).

Figure 1.20: Robustness - Alternative definitions of mass layoff (firm size = 15)

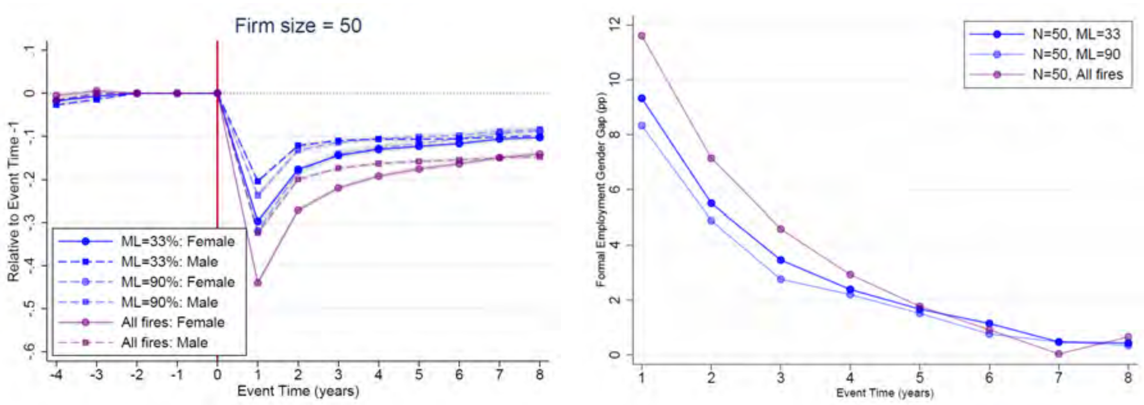
(a) (b)



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level. $ML = 33\%$ represents the definition of mass layoffs that considers layoffs of 33% or more of the workforce in the year. $ML = 50\%$ represents the definition of mass layoffs that considers layoffs of 50% or more of the workforce in the year. $ML = 90\%$ represents the definition of mass layoffs that considers layoffs of 90% or more of the workforce in the year. "All fires" represents the estimate when no mass layoff definition is used.

Figure 1.21: Robustness - Alternative definitions of mass layoff (firm size = 50)

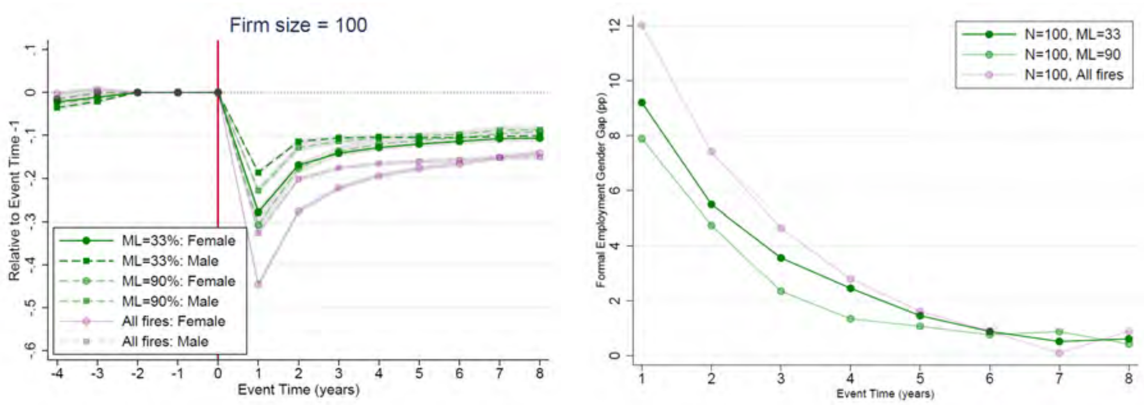
(a) (b)



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level. $ML = 33\%$ represents the definition of mass layoffs that considers layoffs of 33% or more of the workforce in the year. $ML = 50\%$ represents the definition of mass layoffs that considers layoffs of 50% or more of the workforce in the year. $ML = 90\%$ represents the definition of mass layoffs that considers layoffs of 90% or more of the workforce in the year. "All fires" represents the estimate when no mass layoff definition is used.

Figure 1.22: Robustness - Alternative definitions of mass layoff (firm size = 100)

(a) (b)



Note: The figure presents estimates of the parameters γ_k in Equation 1. The omitted category is the difference between treated and control groups in the year before the mass layoff. $Employment_{it}$ variable is equal 1 if the worker i was employed in the year t, 0 otherwise. The sample is composed of workers involved in mass layoff events during 2008-2012 and their counterfactuals (see Section 1.3.1 for more details). Standard errors are clustered at the city-year level. $ML = 33\%$ represents the definition of mass layoffs that considers layoffs of 33% or more of the workforce in the year. $ML = 50\%$ represents the definition of mass layoffs that considers layoffs of 50% or more of the workforce in the year. $ML = 90\%$ represents the definition of mass layoffs that considers layoffs of 90% or more of the workforce in the year. "All fires" represents the estimate when no mass layoff definition is used.

2
When the Inspector Knocks at the Door: Effects of Labor Inspections in Brazil

This paper estimates the effects of labor inspections on establishments outcomes in Brazil. Using detailed administrative data on labor inspections and linked employer-employee records, we apply a staggered difference-in-differences strategy. We find that inspections lead to a significant decline in employment, driven primarily by a reduction in hiring rather than an increase in separations. The probability of exit also rises, particularly among young and medium-sized establishments. At the worker level, inspections generate a temporary increase in employment probability, followed by wage stagnation for those who remain in inspected establishments, while workers who separate exhibit stable or slightly improved earnings. Two key enforcement mechanisms shape these effects: (i) deterrence, where even non-notified establishments adjust labor practices in anticipation of future inspections, and (ii) punishment, where notified/fined establishments experience larger employment and wage adjustments. These findings highlight how enforcement can influence firm behavior through both direct compliance costs and deterrence effects, with implications for labor market regulation in developing economies.
KEYWORDS: Labor Inspections; Establishment Outcomes; Worker Outcomes; Deterrence Effect; Punishment Effect
Código JEL: D22, K20, K42

2.1 Introduction

Brazil has one of the strictest labor laws in the world, designed primarily to protect employees' rights and ensure minimum working conditions (Cardoso & Lage, 2007). This legislation imposes significant costs on establishments, which may strategically decide whether to comply with the regulations[1]. To enforce labor regulations and promote compliance, the Brazilian government makes extensive use of on-site labor inspections. For example, in 2016, 168,974 establishments were inspected and 65,636 were notified of a labor irregularity.

The economic effects of labor inspections, however, are theoretically ambiguous. While enforcement can improve worker well-being by increasing compliance with legal protections and reducing irregular practices (Besley & Burgess, 2004; Ronconi, 2010), it may also impose rigidities that distort establishments' employment decisions (Almeida & Carneiro, 2009; Heckman & Pagés, 2004). In contexts of strict labor regulation and high payroll taxes—such as in Brazil—firms may react to enforcement either by formalizing contracts and improving compliance, or by reducing hiring, shifting to informal arrangements, or downsizing (Cardoso & Lage, 2007). At the worker level, inspections may result in short-term displacements but also foster long-term improvements through better job matches and access to social protection. The net effect of labor inspections remains an empirical question, particularly in contexts of high informality and limited enforcement capacity.

This study aims to fill these gaps by analyzing the effects of labor inspections on establishments' exit, employment, hiring, separations, and wage-setting decisions, as well as on worker-level outcomes in Brazil. Labor inspections are carried out by local agencies of the Ministry of Labor, which have discretion over whether and when to inspect a given establishment. Inspections may be random or triggered by complaints, and their outcomes range from notifications and minor fines to indefinite stop-work orders affecting entire establishments. Using an event-study approach, we find that establishments' responses extend beyond the direct effects of penalties, with significant employment reductions even in establishments that were not formally notified. The relative equilibrium effects on workers are relatively small.

We combine two datasets for our empirical analysis: the Brazilian linked employer-employee dataset (RAIS) and a novel dataset on labor inspections. The labor inspection data were obtained from the Ministry of Labor through Freedom of Information Act requests, covering approximately 1.3 million inspections during the period from 2007 to 2017. These data provide monthly information on inspections

[1] Cardoso & Lage (2007) estimate that for a worker to receive a net wage of R$100, employers must disburse approximately R$165.

at the establishment level, including notifications and the nature of the infractions detected. By linking this information to RAIS, we are able to track establishments and workers over time and observe their labor market outcomes before and after an inspection takes place.

Using this data, we implement three complementary strategies to study the effects of labor inspections at both the establishment and worker levels. First, to examine whether inspections affect firm survival —i.e., whether inspected establishments are more likely to exit the market —we use an unbalanced panel of establishments that includes all units inspected once during the analysis period, as well as establishments never inspected. In this setup, never-inspected firms serve as the control group, and we apply both traditional and staggered difference-in-differences (DiD) estimators.

Second, to assess how establishments adjust their labor outcomes —such as employment levels, hiring, separations, and wages —in response to inspections, we restrict the sample to a balanced panel of establishments that were inspected exactly once during the period and remained active in RAIS throughout the entire 11-year window. This restriction ensures comparability over time and allows us to isolate inspection effects from establishments exit dynamics. In this setting, control units consist of establishments not yet treated at a given point in time, and we again employ both traditional and staggered DiD frameworks[2].

Third, we turn to the individual level to study how inspections affect workers employed in treated establishments at the time of inspection. For this, we adopt a matching DiD strategy, focusing on the 2011 inspection cohort. Treated individuals are those employed at inspected establishments in the year of inspection, while matched controls are similar workers employed at non-inspected establishments. Matching is based on observable characteristics, allowing us to isolate the impact of being employed at an establishment undergoing inspection.

Importantly, the timing of inspections —relative to the occurrence of infractions —is not determined by the establishment. This institutional feature helps mitigate concerns that firms could influence when they are inspected in response to their compliance status. We assume that the timing of the inspection is unexpected by the establishment.

The results from the establishment-level event study show that a significant share of inspected establishments close following a labor inspection. We find that 10% of establishments exit the formal sector after an inspection, relative to the control group. This effect is particularly pronounced among medium-large estab-

[2]The staggered DiD helps control for business cycle effects while maintaining a comparable pool of establishments.

lishments (50–500 employees), younger establishments (up to three years old), and establishments in the construction sector. We do not take a position on whether these exits are positive or negative, as some may result from enforcement actions aimed at protecting workers' wellbeing, while others may reflect voluntary decisions by establishments. That said, 8% of inspected establishments that neither committed any infraction nor received a notification also exit, suggesting that stop-work orders account for only a small share of establishment closures.

Labor inspections also lead to significant changes in key establishment-level outcomes, particularly turnover and employment. On average, employment declines by 7.8% within six semesters after an inspection, driven largely by a 9.5% drop in hirings and a 5.3% reduction in separations. These effects vary substantially by establishment characteristics. Medium-sized establishments (50-500 employees) show larger reductions in employment and hiring compared to smaller establishments (10-49 employees). Younger establishments (up to three years old) are the most affected, with employment and hiring falling by 15.5% and 20.9%, respectively. These reductions may reflect a shift in establishments' perception that turnover has become more costly. They may also indicate a tendency for establishments to "freeze" human resource decisions following an inspection. We also find a modest decline in establishment-level wages of -2.2%.

We further explore the outcomes of inspections—whether establishments received a notification or a fine—to help disentangle the two primary mechanisms through which inspections operate: punishment and deterrence. The punishment mechanism comes into effect when inspections result in financial sanctions, formal notifications of non-compliance, or direct intervention. Establishments subject to these penalties face an immediate increase in compliance costs, which can lead to adjustments such as workforce reductions, wage changes, or investments in compliance measures. In contrast, the deterrence mechanism operates even in establishments that are inspected but not penalized. For these establishments, the inspection itself raises the perceived likelihood of future enforcement, prompting preemptive changes in labor practices to avoid potential sanctions.

The results reveal significant heterogeneity in how establishments respond to inspections, depending on whether they were fined, notified, or simply inspected without penalties. Establishments that were inspected but not notified show notable adjustments: an increased probability of exit (8%), a reduction in employment (-7.2%), but no change in average wages. These effects highlight the importance of deterrence, as even establishments that are not penalized adjust their behavior to mitigate future risks. Establishments that received a fine experience slightly stronger effects, including a higher probability of exit (9.2%), larger employment reductions

(-8.3%), and a decline in average wages (-2.9%). Comparing the magnitude of these effects suggests that deterrence is as influential as punishment, indicating that the hidden costs of inspections can be just as consequential as the direct financial penalties.

Establishments respond differently depending on the number, and type of infractions. Those with a single violation show stronger changes in labor dynamics, while those with multiple infractions are slightly more likely to exit. Greater penalty intensity —measured by the fine-to-infraction ratio —amplifies reductions in employment and hiring, whereas lower ratios lead to stronger effects on separations and wages. Even mild sanctions appear sufficient to trigger behavioral adjustments, underscoring the role of deterrence. Responses also vary by violation type: exit effects are strongest for compensation-related infractions, followed by health and safety, social contributions, working hours, and informal employment. Yet only health and safety violations lead to meaningful changes in labor dynamics. This likely reflects differences in compliance costs. For example, while fines for informal work are fixed and predictable, health and safety violations can result from factors beyond establishment control, making enforcement more uncertain and costly[3].

So far, we have shown that inspections significantly affect establishments' hiring decisions. But do these changes have any consequences for workers previously employed at inspected establishments? Our worker-level analysis indicates that inspections do not lead to long-term negative employment outcomes and have only limited effects on wages. In the short term, we find a modest but statistically significant increase in the probability of employment for workers in inspected establishments, with an estimated effect of 1% one semester after the inspection. This effect gradually declines and converges to zero by the sixth semester, suggesting that the initial improvement in employment prospects is not persistent.

In terms of wages, the effects depend on whether workers remain with the same employer or transition to a new job. For those who stay with the same employer, inspections are associated with slower wage growth compared to their counterparts in non-inspected establishments, resulting in an estimated relative wage reduction of approximately 5%. In contrast, workers who leave their employer experience stable or slightly improved wages after transitioning, relative to the control group.

In sum, while our results show strong negative effects at the establishment level, these do not appear to carry over to workers who switch jobs. However, it is important to note that these are relative equilibrium effects. It remains possible that inspections have broader aggregate consequences—for example, by affecting

[3]The amount of the fine is fixed per unregistered worker. In the case of a repeat offense, the amount doubles. The establishment may have to retroactively pay labor charges.

entrepreneurship or reducing overall employment and wage levels for both treated and control groups. Given the number of inspections conducted each year, it is not unreasonable to expect potential general equilibrium effects. We encourage future research to explore these broader implications.

This paper contributes to the growing body of literature on the effects of increased enforcement on establishments' compliance with labor regulations and its broader implications for labor market outcomes. The existing literature can be divided into two main strands. The first strand uses aggregate data, typically at the regional or municipal level, to evaluate how enforcement affects outcomes such as formalization rates, employment dynamics, and overall compliance (Abras *et al.*, 2018; Almeida & Carneiro, 2012; Ronconi, 2012, 2010). The second strand exploits labor inspections as a source of exogenous variation to assess the effects of external shocks, such as trade liberalization, or the introduction of specific labor regulations (Berlinski & Gagete-Miranda, 2024; Szerman, 2024; Ponczek & Ulyssea, 2022).

A more recent strand of the literature focuses on analyzing the effects of labor inspections on establishment dynamics, using disaggregated establishment-level data. Samaniego de la Parra & Bujanda (2024) exploit exogenous variation from over 480,000 random work-site inspections in Mexico and find that increasing the cost of informal jobs leads to persistent declines in formal employment at the establishment level, with inspections temporarily increasing the probability of formalization for informal workers but also raising the likelihood of job dissolution in informal arrangements. However, they find no significant impact on the probability of establishment exit, suggesting that establishments are generally able to absorb compliance costs without being forced out of the market.

In the Brazilian context, recent studies have focused primarily on labor inspections aimed at addressing informal employment. Prado *et al.* (2023) document how inspections lead to a significant surge in the formalization of previously informal workers, driven by establishments' need to comply with labor regulations. However, this formalization comes with long-term costs, as establishments face a higher probability of exit and tighter credit conditions, suggesting that the cost of formal labor is difficult to absorb in the long run. Similarly, Foguel & Corseuil (2024) evaluate a large-scale intervention that combined communication and direct inspections, finding that both components increased formalization but had no significant impact on regular, formal labor demand. These effects were short-lived, as establishments reverted to their previous compliance behavior after the intervention. Together, these studies highlight the challenges establishments face in maintaining compliance when enforcement targets informal labor arrangements.

This paper makes three key contributions to the literature on labor enforcement and establishment dynamics. First, unlike most existing studies that focus exclusively on inspections targeting informal labor, we analyze a broad set of labor inspections encompassing all types of regulatory enforcement. This comprehensive approach allows us to evaluate the overall effects of inspections more thoroughly and differentiate outcomes based on the specific types of violations detected. Second, leveraging the richness of the administrative data, we are able to examine key inspection characteristics, such as whether the establishment was fined, the type of violation committed, and the amount of the fine imposed. This enables us to capture nuanced aspects of the inspection process and estimate how these characteristics influence establishment and worker outcomes. Third, we extend the analysis beyond the establishment level by examining the impacts of inspections on individual workers, providing novel evidence on worker trajectories in response to labor enforcement within the Brazilian labor market. Together, these contributions advance the understanding of how labor inspections influence compliance and labor market dynamics, offering insights into the design of more effective enforcement strategies.

This paper is organized as follows. Section 2.2 provides the institutional framework of labor inspections in Brazil. Section 2.3 discusses the data sources used in the analysis, the sample constructed, and presents descriptive statistics. Section 2.4 examines the effects of inspections at the establishment level, while Section 2.5 explores the heterogeneous effects of inspections based on inspection characteristics. Section 2.6 extends the analysis to worker-level outcomes. Finally, Section 2.7 offers concluding remarks.

2.2 Labor Inspections in Brazil

In this section, we describe the institutional structure of labor inspections, define what constitutes a labor inspection, and explain the process for selecting establishments to be inspected.

Labor inspections in Brazil are the responsibility of the Ministry of Labor,[4] with a specific department, the *Secretaria de Inspeção do Trabalho* (SIT), dedicated to handling inspection-related issues. Strategies and action plans are formulated at the national level based on identified needs and goals. While SIT operates nationwide, its presence is established through decentralized units, such as *Superintendências Regionais do Trabalho e Emprego* (SRTE), *subdelegacias*, and *gerências*. On-site inspections are carried out by public employees known as labor inspectors (*auditores-fiscais do trabalho*), who are selected through a public competitive exami-

[4] Also referred to as the Ministry of Labor and Employment or the Ministry of Economy, depending on the administration in place.

nation for government positions. It is a well-compensated profession—placing in the top 5% of the income distribution—and also offers job stability.

Labor inspections have operated on a considerable scale. Between 2007 and 2017, labor inspectors carried out over 2.6 million inspections across the country, averaging 236,000 establishments inspected per year. In about 40% of the cases per year, the visit represented the establishment's first recorded interaction with the labor enforcement (Lira, 2025). These efforts led to the identification and notification of more than 2.3 million labor code violations.

During an inspection visit, labor inspectors are responsible for verifying compliance with all legal provisions related to employment relations, providing guidance to both workers and employers, and identifying potential risk situations (OIT, 2010). If any irregularity is found - such as informal employment, non-payment of FGTS, or violations related to health and safety standards - the establishment is issued a notification for labor code violations (BRASIL, 2002).[5] More severe infractions—such as child labor, forced labor, or situations that pose an immediate risk to workers' lives—may result in immediate stop-work orders.

Upon notification, the establishment has ten days to contest the charges, counted from the date of receipt. The case is then reviewed by a different authority. If the violation is confirmed, a fine is imposed, with a ten-day window for payment. The amount of the fine depends on the severity of the offense and the number of workers affected. The establishment may file an additional appeal, which is reviewed by the designated auditor and subsequently forwarded to the superior department for a final decision.[6] For establishments with up to 10 employees, as well as newly opened businesses, inspections follow the double-visit criterion, which is intended to allow employers to correct irregularities between the first and second visit without facing immediate penalties.[7]

The selection of establishments for inspection is carried out at the local level (*subdelegacias/gerências*) but follows national planning guidelines. Inspections may be triggered either through random selection or in response to complaints—from current or former employees, any citizen, or even anonymous sources in cases involving forced labor (Cardoso & Lage, 2005; Almeida & Carneiro, 2012). Given that the number of labor inspectors has been insufficient to meet the demand for inspections, we are led to believe that local agencies use a range of establishment-level and local labor market data to optimize the selection process.[8] For example,

[5] See Table 2.6 in Appendix 2.7 for more details on the irregularities verified during the inspections.

[6] *Coordenação Geral de Recursos da SIT*.

[7] Except in cases where an infraction involves lack of registration.

[8] In 2016, approximately 2,400 inspectors were in operation, representing a reduction of more

Lira (2025) shows that distance to the nearest enforcement office, establishment size, and high levels of turnover are good predictors of labor inspections.

In the empirical analysis of this paper, we focus on establishments that were inspected only once during the period for which we have data. In such cases, there are strong reasons to believe that the timing of the inspection is not anticipated by the establishment. First, although many establishments are inspected each year, the annual probability of inspection for a large share of establishments is below 5%. For entrepreneurs who have never been inspected before, the perceived or estimated probability is likely even lower. Additionally, establishments are not notified in advance of an upcoming inspection. Second, an irregularity may be present for an extended period before an inspection occurs. Several steps leading up to an inspection are beyond the establishment's control. For instance, establishments do not influence whether a worker chooses to file a complaint, the timing of that complaint, whether the local agency decides to act on it, or how long it takes for the agency to conduct an on-site inspection. These delays often depend on the availability of labor inspectors and the queue of pending inspections. This unpredictability in timing provides a valuable source of variation, which we exploit to analyze the behavior of inspected establishments compared to those not yet inspected.

We describe the inspection data used in this study in the following section.

2.3 Data and Descriptive Statistics

2.3.1 The Brazilian Linked Employer-Employee dataset and Labor Inspections dataset

We match two administrative data sources to study the effects of labor inspections on establishments' and workers' decisions: the Brazilian linked employer-employee dataset (*Relação Anual de Informações Sociais - RAIS*) and administrative records from the Labor Inspections Department (*Secretaria de Inspeção do Trabalho - SIT*).

RAIS is a dataset maintained by the Brazilian Ministry of Labor that covers the universe of formal employment contracts in the country. The data provide detailed information on workers (age, gender, race, and educational attainment),

than 20% from 2011 (SIT, 2011, 2017). This means that the number of inspectors per 10,000 formal workers decreased from 0.66 in 2011 to 0.52 in 2016. As a comparison, in developing countries such as South Africa and Mexico, the corresponding figures in 2016 were 0.8 and 0.2, respectively (ILO, 2020).

establishments (size, municipality, and sector of activity), and the job match (wage, occupation, tenure, hours worked, contractual terms, and dates of hiring and separation). A key advantage of the dataset is that it uniquely identifies both workers and firms, enabling the construction of a high-frequency panel to track employment dynamics over time. We aggregate these records at the semester level, allowing us to analyze how employment, hiring, separations, and wages evolve in the periods before and after inspections. Our analysis uses data spanning the period from 2007 to 2017.

Data on inspection events were obtained through a Freedom of Information Act request (*Lei de Acesso à Informação — LAI*) submitted to the Ministry of Labor. The dataset covers the universe of labor inspections conducted between 2007 and 2017, recording the date of each inspection (month and year), the unique identifier of the establishment (CNPJ), and information on violations identified during inspections: the number of notifications issued to each establishment, the types of infractions detected, and the total monetary penalties imposed on each visit.[9] We classify violations into five categories: health and safety, informality, remuneration, working time, and social contributions.

During the period of analysis, RAIS data include information on 7.3 million establishments. Of these, 6.0 million were never inspected, 793,052 were inspected once, and 538,735 were inspected more than once.

We apply two main restrictions to our sample. First, the empirical analysis in this paper is limited to establishments that were inspected only once during the period. Multiple inspections can lead to overlapping pre- and post-treatment windows, making it difficult to define a clear treatment event. Second, we restrict the sample to establishments with between 10 and 499 employees in their baseline year—that is, the first year they appear in the dataset. This restriction addresses two concerns: small firms (with fewer than 10 employees) are subject to a distinct "double-visit" inspection rule (see Section 2.2), which may be more lenient than inspections applied to larger firms; and large firms (with more than 500 employees) may differ systematically in both observable and unobservable characteristics, reducing the plausibility of comparisons across size categories.

After applying these restrictions, we are left with an *unbalanced panel* of 288,896 establishments, of which 100,260 were inspected and 188,636 were never inspected. This sample is used in our analysis of establishment exit[10]. For the

[9] Although establishments may be cited for multiple violations, the data do not link fines to specific infractions; only the aggregate amount of the penalty is reported.

[10] Exit is an indicator variable equal to one if year t marks the establishment's final observation in the sample.

analysis of employment, hiring, separations, and wages, we rely on a *balanced panel*[11] of 50,639 establishments that were inspected only once between 2007 and 2017 and had employees in every year of that period. The balanced structure ensures consistency across periods and mitigates potential bias arising from changes in sample composition over time.

We discuss the sample used in the worker-level analysis in Section 2.6.

2.3.2 Descriptive Statistics

Table 2.1 presents the characteristics of inspected and never-inspected establishments in the unbalanced panel, as well as inspected establishments in the balanced panel. Columns 1 and 3 present summary statistics for the period before the inspection. Column 2 presents summary statistics for the first period in which we find the never-inspected establishment in the data.

In the unbalanced panel, inspected and never-inspected establishments are similar across most characteristics. Inspected establishments are slightly larger on average, employing 33.4 workers compared to 29.3 in never-inspected firms. The average yearly change in employment is close to zero for both groups. Establishments also resemble each other in terms of log wages (7.63 vs. 7.65), age distribution, and sectoral composition: most are over five years old, and the service and commerce sectors are the most represented. A key distinction between the groups lies in turnover: inspected firms exhibit higher workforce turnover, with both hiring (9.2 vs. 6.0) and separation rates (9.1 vs. 5.1) notably higher than those of never-inspected firms.

Inspection characteristics show that most inspected firms (71%) did not receive any notifications of violations, while 29% were notified and 19.8% were fined. The average number of notifications per inspected firm is 1.05, and the average fine amount is R$1,794 (less than USD 500), indicating that financial penalties are generally modest.

Column 3 of Table 2.1 presents descriptive statistics for inspected establishments in the balanced panel. This sample excludes establishments that exited up to three years after the inspection. Notably, over 90% of these establishments were more than five years old in the period prior to inspection. They are comparable to establishments in the unbalanced panel across most characteristics. The exception is the workforce turnover, which is lower in the balanced sample.

[11] That is, establishments active in RAIS in all 11 years of analysis (2007 to 2017).

Table 2.1: Descriptive Statistics

	Unbalanced Panel		Balanced Panel
	Inspected	Never-Inspected	Inspected
	(1)	(2)	(3)
Establishment Characteristics:			
Number of employees	33.44	29.32	31.83
	(54.83)	(66.22)	(50.96)
Δ(number of employees)	0.0869	0.0138	0.124
	(0.915)	(0.705)	(0.605)
Number of hires	9.17	5.91	7.17
	(26.29)	(17.76)	(18.95)
Number of separations	8.25	5.64	6.40
	(22.92)	(16.38)	(17.18)
Ln(average wage)	7.63	7.65	6.98
	(0.62)	(0.92)	(0.59)
Age:			
Up to 3 years	0.0659	0.0598	0.0358
	(0.248)	(0.237)	(0.186)
3-5 years	0.0765	0.0621	0.0528
	(0.266)	(0.241)	(0.224)
More than 5 years	0.858	0.878	0.911
	(0.349)	(0.327)	(0.284)
Sector:			
Industry	0.246	0.234	0.237
	(0.43)	(0.423)	(0.426)
Commerce	0.313	0.262	0.313
	(0.464)	(0.44)	(0.464)
Services	0.409	0.479	0.429
	(0.492)	(0.500)	(0.495)
Construction	0.0329	0.0256	0.0213
	(0.178)	(0.158)	(0.144)
Inspection Characteristics:			
Inspected without notification	0.71	-	0.747
	(0.453)		(0.435)
Inspected with notification	0.289	-	0.253
	(0.453)		(0.435)
# Notifications	1.054	-	0.973
	(3.254)		(3.25)
Inspected with fines	0.198	-	0.183
	(0.398)		(0.387)
Fine Amount (in *R*$)	1,794	-	1,677
	(13,757)		(14,968)
Fine Amount per notification (in *R*$)	1,831	-	1,949
	(6,346)		(7,230)
Number of Establishments	100,260	188,636	50,639

Note: The table reports descriptive statistics for establishments using information from RAIS and inspection data. Columns 1 and 2 refer to the unbalanced panel: Column 1 presents statistics for establishments inspected only once between 2007 and 2017, while Column 2 reports statistics for establishments not inspected during this period. Column 3 refers to establishments in the balanced panel that were inspected only once during the analysis period. Summary statistics are computed using values from $t = -1$ for inspected establishments, and from the entire period for non-inspected establishments (2007-2017).

2.4
Effects of Labor Inspections on Establishments Outcome

In this section, we present the empirical strategy and the main results from the establishment-level analysis. We begin by describing the staggered DiD approach. Next, we examine the impact of labor inspections on the probability that an establishment exits the formal sector. We then turn to the effects of inspections on establishments that remain active, focusing on employment, hiring, separations, and wages. Finally, we validate our estimates by presenting robustness checks, including a series of additional tests and alternative specifications.

2.4.1
Staggered Difference-in-Differences Approach

In our setting, establishments are inspected at different points in time. Following Callaway & Sant'Anna (2021), we restrict the analysis to a fixed time window before and after each inspection. We estimate the average treatment effect on the treated, $ATT(g,t)$, for each treatment cohort g and period t by comparing outcome trajectories between inspected establishments and the corresponding control group. We restrict the treatment group to establishments inspected only once.

The sample and control group definitions vary depending on the outcome of interest. For the analysis of establishment exit, we use the unbalanced sample and restrict the control group to never-treated establishments. For employment, hiring, separations, and wages, we use the balanced sample and restrict the control group to establishments not yet inspected[12].

The average treatment effect on the treated for unit in the group g at period $t \geq g$ is given by:

$$\widehat{ATT}(g,t) = \frac{\sum_i \Delta Y_{i,g-1,t} 1\{G_i = g\}}{\sum_i 1\{G_i = g\}} - \frac{\sum_i \Delta Y_{i,g-1,t} C_i}{\sum_i C_i} \tag{2-1}$$

where $i \in 1, 2, ..., N$ indexes establishments, $t \in 2007h1, 2007h2, 2008h1, ..., 2017h2$ indexes semesters, and $g \in 2008h1, 2008h2, ..., 2017h2$ denotes treatment cohorts. For the exit outcome, we use the annual panel. In this case, $t \in 2007, 2008, ..., 2017$ indexes years, and $g \in 2008, 2009, ..., 2017$ denotes treatment cohorts. The variable C_i is a binary indicator equal to one for establishments in the control group. The term $\Delta Y_{i,g-1,t} \equiv Y_{i,t} - Y_{i,g-1}$ represents the evolution of outcome Y at time t relative to the period immediately before treatment $(g-1)$.

The staggered DiD approach helps account for business cycle effects while

[12]The balanced sample means that it includes the establishments active in the RAIS data over the 11 years of the analysis (2007 to 2017).

preserving a comparable pool of establishments. The key identifying assumption is parallel trends—that is, in the absence of an inspection, the evolution of outcomes would have been the same for treated and control groups. We justify this assumption in Section 2.2, noting that many establishments are eligible for inspection, and the timing of inspections is shaped by decisions from workers and enforcement agencies that lie outside the establishments' control. As a result, inspections come as a surprise to establishments. In the following sections, we show that pre-treatment trends are parallel, supporting the comparability between treated and control units.

After estimating the average treatment effect on the treated for each treatment cohort g and period t, $\widehat{ATT}(g,t)$, we present the main results using an event-study aggregation, in which we combine the estimates of different cohorts g according to the time relative to the treatment period ($e = t - g$). We then consolidate the post-treatment estimates into a single measure to assess magnitudes.

2.4.2 Probability of Establishment Exit from the Formal Sector

We first present the effects of inspections on establishment exit. The estimates in this section are derived from an unbalanced panel of establishments with 10 to 500 employees that were either inspected once (treatment group) or never inspected (control group) during the period from 2007 to 2017. The outcome of interest is establishment exit—an indicator variable equal to one if year t marks the establishment's final observation in the sample.[13][14] This variable is constructed exclusively using RAIS data. An establishment is classified as inactive if it ceases to appear in the annual RAIS declaration.

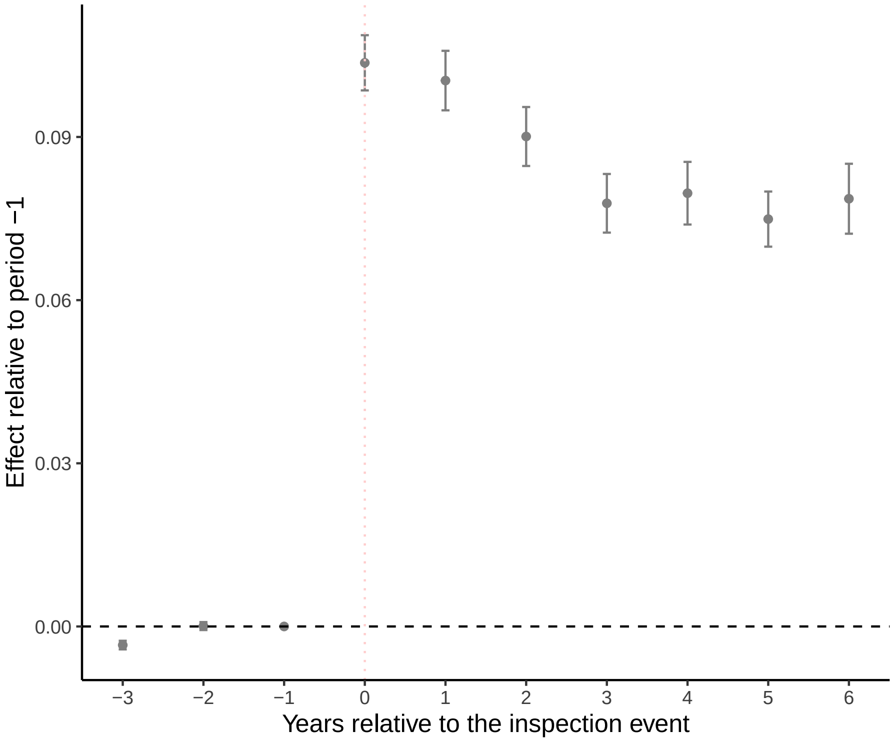
Figure 2.1 presents the main results of this section. After an inspection, treated establishments are more likely to close compared to those in the control group. In the year immediately following an inspection, the relative increase in exit probability is approximately 10%. As more establishments in the control group exit over time, this effect diminishes, reaching 8% four years after the inspection. The average effect over the six-year period following inspection is roughly 8.6% (Column 1 of Table 2.9 in Appendix). The increase in standard errors in the post-treatment period is mainly due to a decline in the number of observations. By construction, all treated units are observed up to $k = 0$. However, from $k = +1$ onward, some firms exit the formal sector or stop appearing in the data, which reduces the sample size in those periods.

[13] Except for 2017, which is the last year of available data.

[14] If the establishment last appeared in 2010, then this year is considered the year of exit. If the establishment does not appear in a year, but then reappears in the following year, then it is not considered to have left the market.

We present these effects by establishment size, age, and sector in Figure 2.2 (Tables 2.7 and 2.8 in Appendix). Larger establishments have a higher probability of exit following inspections compared to smaller ones. Specifically, establishments with 50 to 500 employees have an exit probability of 9.5%, whereas smaller establishments (10 to 49 employees) have a probability of 7.3%.[15]

Figure 2.1: The effects of inspection on exit - establishments



Note: This figure reports point estimates of the effects of inspection on exit variable using the establishment-level sample from RAIS data. Exit variable is a dummy equals 1 if the year t is the last observation of the establishments in the sample and $t \neq 2017$. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

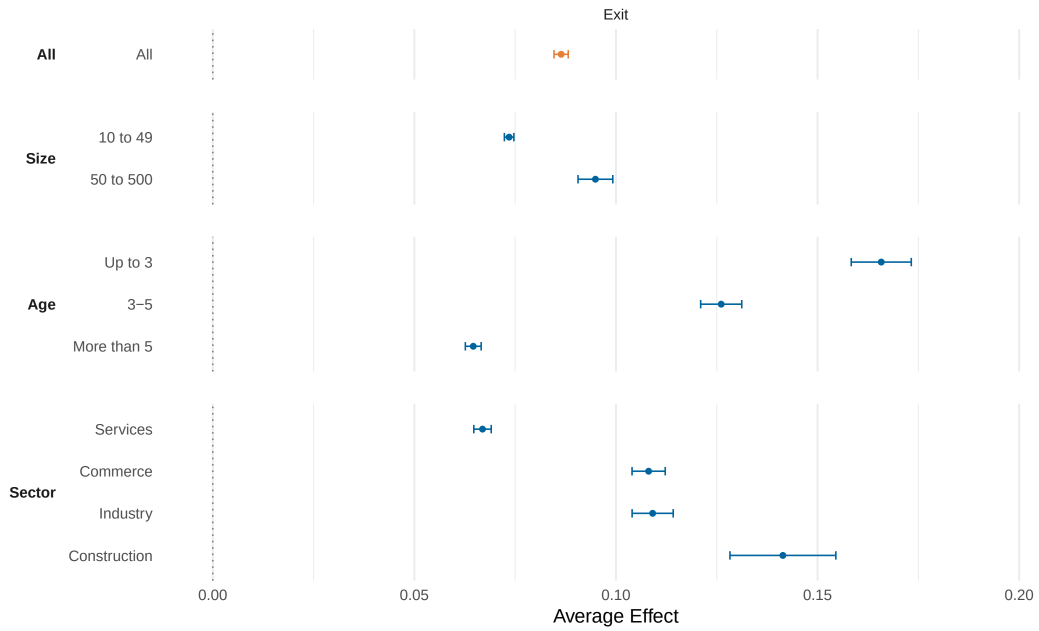
Younger establishments are more likely to exit following an inspection compared to the control group. Establishments aged 0 to 3 years have a 16.6% higher probability of exit than the control group. For establishments aged 3 to 5 years and those older than 5 years, the exit probabilities significantly drop to 12.6% and 6.5%, respectively. These results align with the typical entrepreneurial life-cycle: younger firms tend to represent a larger share of total establishments and have inherently higher exit rates compared to more mature firms. In this context, inspections may further push these vulnerable firms away from formality. The inspection might act as a wake-up call to an establishment already facing difficulties, clarifying

[15]Approximately 90% of the establishments in the sample fall within the 10 to 49 employee range. This is a result of the establishment size distribution and the fact that larger establishments typically face more frequent inspections due to their size and greater regulatory visibility, thus making them less likely to meet the inclusion criteria for the sample.

the costs associated with formalization. Consequently, establishments may either close or transition into informality.[16]

Exit probabilities also vary across sectors. Figure 2.2 (and Table 2.8 in Appendix) shows that establishments in the construction sector have the highest exit probability (14.1%), followed by industry (10.9%) and commerce (10.8%). Establishments in services exhibit the lowest exit likelihood, at 6.7%. The elevated exit risk in construction may reflect higher regulatory complexity and the sector's reliance on temporary labor, both of which complicate compliance.

Figure 2.2: Heterogeneous effects of inspections on establishments' exit



Note: This figure reports the aggregate effects of inspection on exit variable. Exit variable is a dummy equals 1 if the year t is the last observation of the establishments in the sample and $t \neq 2017$. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007-2017, and the control group. The size categories are: 10-49 employees and 50-500 employees. The age categories are: less than 3 years, 3-5 years, and more than 3 years. The sector categories are: industry, services, construction, and commerce. The number of establishments (total) and establishments inspected is reported in Appendix Tables (2.7, 2.8 and 2.9). The figure plots point estimates and the 95% confidence interval. Standard errors are clustered at the establishment level.

In summary, despite heterogeneous effects of inspections on establishment exit, we still observe a substantial impact, with at least a 6% increase in exit probability even among the least affected groups. These findings align with those of Prado *et al.* (2023), who also examine labor inspections in Brazil, but differ from Samaniego de la Parra & Bujanda (2024), who analyze inspections in Mexico and find no significant effect on establishment exit.

[16]We cannot distinguish between establishments that genuinely close and those that simply stop reporting to the Ministry of Labor, effectively joining the informal sector.

2.4.3 Employment, Hiring, Separation and Wages

In this section, we present the effects of inspections on establishments' employment levels, number of hires, number of separations, and average wages. The estimates are based on a balanced panel of establishments that were active throughout all 11 years of the sample, had between 10 and 500 employees, and experienced exactly one inspection between 2007 and 2017. The control group comprises establishments that have not yet been inspected.

Figure 2.3 presents the cohort-aggregated average treatment effects on the treated for each period relative to the inspection. In each graph, the interval between periods is one semester.[17] The four panels show a similar pattern: trends in outcomes before the inspection are similar for treated and control groups. After the inspection, establishments face a sharp and significant decline in the number of employees (panel A), in the number of hirings (panel B), in the number of separations (panel C), and in average wages (panel D).

Six semesters after an inspection, establishments have a 7.8% reduction in employment, relative to the control group (Table 2.9 in Appendix). This reduction in employment is a result of the larger reduction in hirings than in separations, and may indicate a contraction in business activities following labor inspections. This pattern suggests that establishments may adopt a 'freezing' strategy, significantly reducing operational decisions, especially regarding workforce adjustments. This behavior may stem from a shift in establishments' perception of an increased likelihood of future inspections, leading them to reduce hiring and separations to manage anticipated compliance costs.

Panel (b) presents the results for ln hiring. Similarly, there is a decline from one semester after inspection (6.5%), persisting until at least $t = +6$. The average effect, as shown in Column 3 of Table 2.9 indicates a drop of 9.5%. As in Samaniego de la Parra & Bujanda (2024), we examine whether there is a difference in hiring workers from the formal labor market and those previously outside it. To achieve this, we compute the total number of hires of workers who were employed in a formal establishment in the previous year and compare it with the total number of hires of workers who were outside the formal labor market in the previous year. Figure 2.7a in Appendix 2.7 provides evidence of an increase in the hiring of workers from outside the formal labor market at $t = +1$, followed by a decline from $t = +2$ onwards. In contrast, for the hiring of workers from the formal labor market, as shown in Figure 2.7b, the results suggest a steady decline over time. Despite the limitations

[17] Unlike the previous section, where intervals were annual.

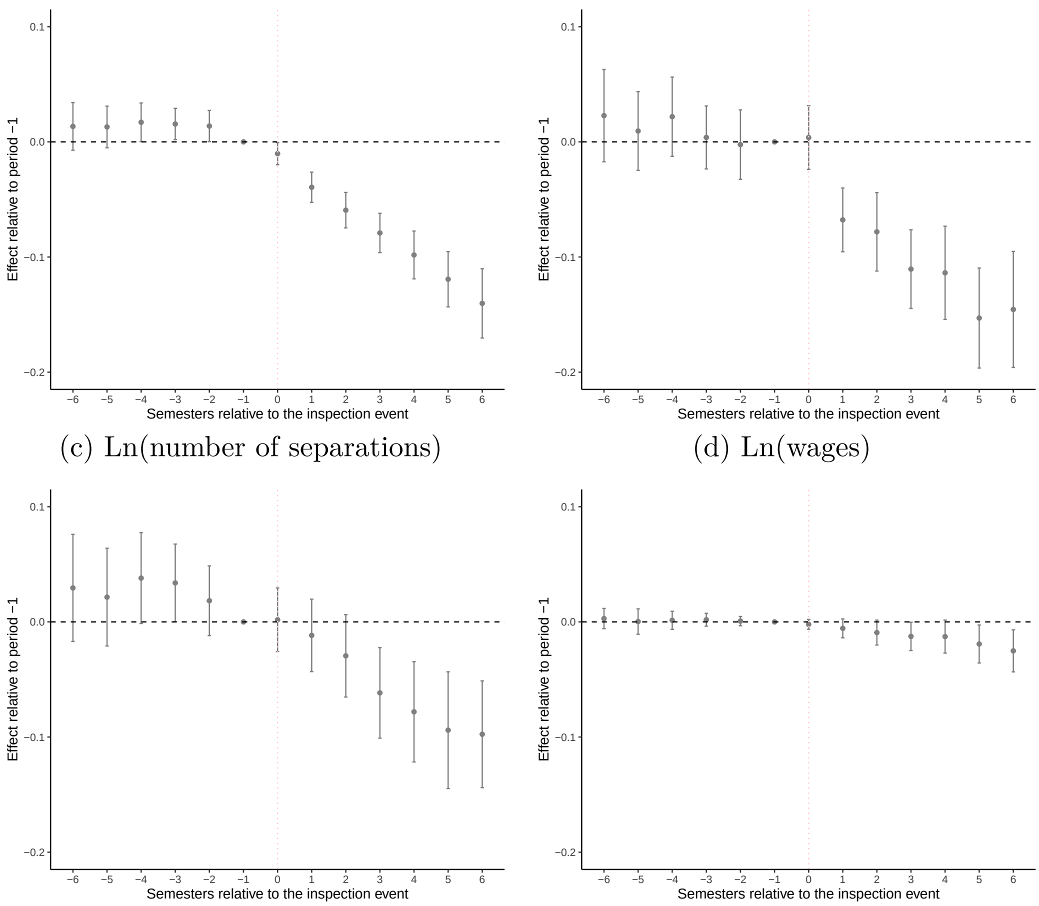
in the construction of the variables, this difference may suggest an increase in worker formalization in the short term as a result of the inspection.

Estimates for ln wage are documented in Panel (d). Similar to ln separation, there is a clear trend after inspections. However, the coefficient only becomes significant at $t = +5$ and $t = +6$. The average effect considering six semesters post-inspection is relatively small in magnitude (-1.2%), as shown in Column 5 of Table 2.9. Beyond the effect on average wages, we analyze the 10th and 90th percentiles to assess potential changes across the wage distribution. Figure 2.8 in Appendix shows a negative variation of approximately 7% at the 10th percentile of the wage distribution after the inspection, indicating a stronger effect on lower-income workers. In contrast, the 90th percentile exhibits a smaller decline of around 2%, suggesting that the impact of inspections is more pronounced among lower-wage employees.

Figure 2.3: The effects of inspection on Establishments' outcomes

(a) Ln(number of employees)

(b) Ln(number of hirings)



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

We also investigate heterogeneous effects based on establishments' characteristics, as size, sector of activity and establishment's age. Looking to size on baseline, Figure 2.4 (Table 2.10 in Appendix) show that the medium/large establishments, 50 to 500 employees, are the most affected by inspections in terms of employment, hiring e separations[18]. It is a surprise effect medium-sized companies (50-500 employees) be more affected than small establishments (10-50 employees). A possible hypothesis is that the medium-sized establishments that reacted to the inspection used labor legislation evasion as a lever for growth. When they were inspected and faced with the costs of evasion and the rules, they were compelled to adapt by downsizing and reducing their turnover.

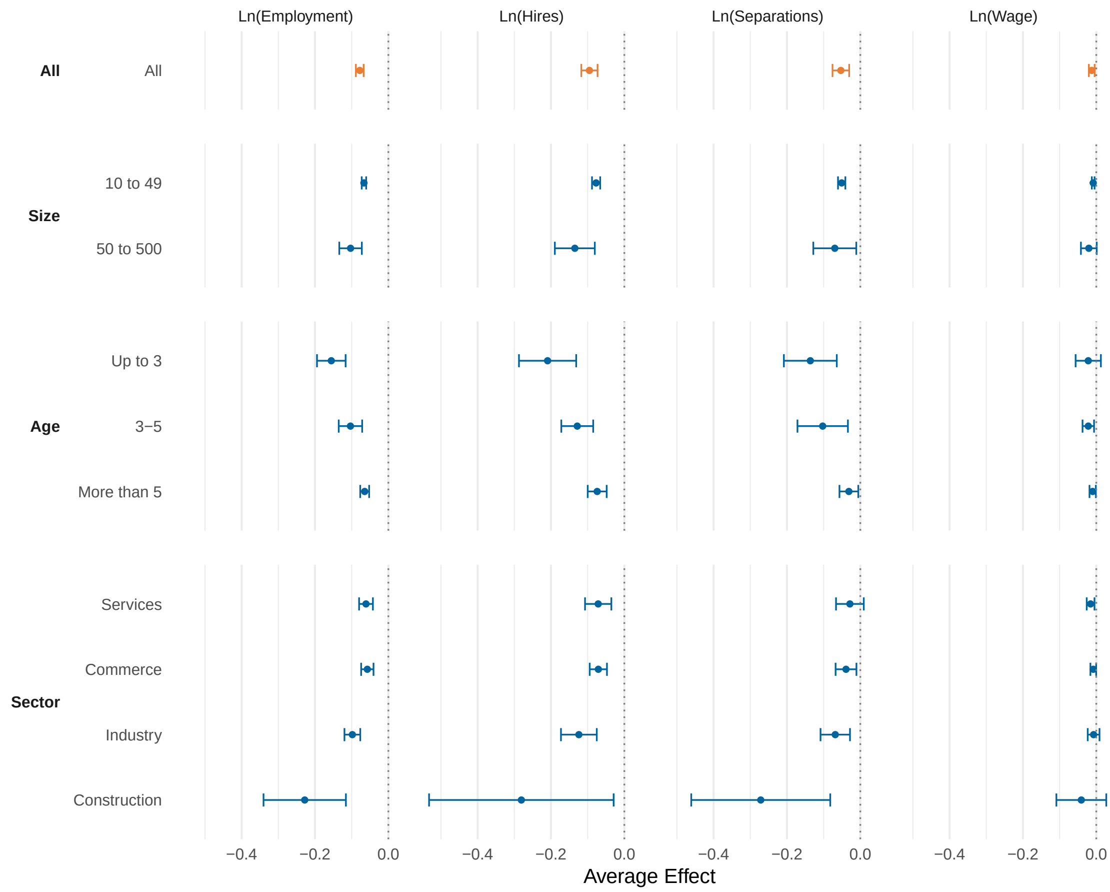
When we look at heterogeneity by age of the establishment, estimates show that younger establishments, those that in 2007 had up to 3 years of creation, react most negatively to inspections (Figure 2.4 and Table 2.11 in Appendix). Since our estimation sample covers the period from 2007 to 2017, we lack information on prior events. Therefore, we cannot guarantee that the inspection studied is the first one received by the establishment[19]. However, when focusing on younger establishments, the likelihood that the inspection is the first one received increases.

In terms of economic activity, establishments in the construction sector are more affected than those in the industry, services, and trade sectors (Figure 2.4 and Table 2.12 in Appendix). The more pronounced effect in the construction can be attributed to the greater compliance challenges faced by establishments in this sector. Due to the often temporary nature of construction projects, establishments in this sector frequently struggle with maintaining adequate safety equipment, ensuring proper working conditions, and raising awareness about the use of safety measures. Moreover, the dynamic environment of construction sites, with their fluctuating conditions, makes it particularly difficult to consistently meet minimum labor standards and ensure compliance with regulations.

[18]We consider the baseline size to be the size of the establishment in the first year of the sample, in this case, 2007.

[19]Although we restricted the analysis to establishments that received only one inspection between 2007 and 2017, we could not confirm it was the first.

Figure 2.4: Heterogeneous effects of inspections on establishments' outcomes



Note: The figure reports the aggregate effects of inspection on different outcomes: ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The size categories are: 10-49 employees and 50-500 employees. The age categories are: less than 3 years, 3-5 years, and more than 3 years. The sector categories are: industry, services, construction, and commerce. The number of establishments (total) and establishments inspected is reported in Appendix Tables (2.9, 2.10, 2.11 and 2.12). The figure plots point estimates and the 95% confidence interval. Standard errors are clustered at the establishment level.

The results of this study align with a growing body of literature that explores the effects of labor inspections on establishment performance. Previous studies consistently document that labor inspections, by increasing compliance costs and restricting access to informal labor, can negatively affect employment and related establishment-level outcomes.

In the context of Brazil, Almeida & Carneiro (2012) using municipality-level data found that increased enforcement leads to significant reductions in employment (-0.47%) and hiring (-0.38%), while the effect on separations was not statistically significant. The main explanation provided by the authors is that restricted access to informal workers - who are cheaper and more flexible - hurts establishment performance. Similarly, but using micro-data, Prado *et al.* (2023) and Brotherhood *et al.* (2024), examine the effects of inspections targeting informal labor in Brazil, finding an initial increase in employment followed by consecutive declines.

Meanwhile, Samaniego de la Parra & Bujanda (2024), analyzing random inspections in Mexico, found immediate declines in employment and hiring following inspections, coupled with an increase in separations during the inspection year ($t = 0$) and negative variations in subsequent years ($t = +1$). Samaniego de la Parra & Bujanda (2024) argues that the combination of increased separations and reduced hiring explains the observed drop in employment.

In sum, our results suggest an increase in the probability of establishment exit from the market following an inspection. For those establishments that remain active after the inspection, there appears to be a 'freeze' in operational activities, with a more pronounced decrease in hiring relative to separations, leading to a reduction in establishment size. Both extensive and intensive margin responses may stem from the shocks induced by inspections: the rise in compliance costs (in addition to penalties for those fined) and the heightened perceived probability of future inspections. These factors likely drive firms to adopt more cautious strategies, freezing operational activities to cope with increased compliance requirements and the uncertainty regarding future inspections.

This paper contributes to the literature in some aspects. We provide a broader analysis of labor inspections by examining not only inspections focused on combating informality but also including a wider range of inspections. This approach allows for a more comprehensive understanding of the overall effects of labor inspections, while also enabling the differentiation of these effects by inspection type. Additionally, the study seeks to distinguish between the effects of being inspected but not punished and the effects of being both inspected and punished, in order to better understand the mechanisms at play in the transmission of the effects, specifically the roles of punishment and deterrence. Furthermore, by extending the analysis to workers, a novel aspect using Brazilian data, the paper provides a deeper understanding of the consequences of labor inspections on the labor market.

2.4.4 Robustness

In Appendix 2.7 we show four additional estimations to verify the robustness of our main results. First, we use alternative variable definitions by applying inverse hyperbolic sine transformation to our four main outcomes. Figure 2.9 demonstrates that the conclusions remain largely consistent.

Second, we relax the restriction of the balanced panel and estimate using a panel with stayers, which includes establishments that were active in the [-6,6] window around the inspection date.his approach introduces more flexibility, but the panel size reduces significantly, from 51,806 establishments in the balanced panel to 25,103 establishments (a 51% reduction). Nonetheless, the results remain very similar to our main estimates, as indicated in Figure 2.10).

Finally, we drop establishments that were inspected between 2003 and 2007 to ensure that the inspection during the 2007-2017 period is the first within a substantial time interval. The results in Figures 2.11 and 2.12 remain consistent.

2.5
Potential Mechanisms: Punishment and Deterrence

In the theoretical framework, we assume that a profit-maximizing firm that chooses between complying with regulations or evading labor laws (Ashenfelter & Smith, 1979). Compliance entails fulfilling legal obligations, while evasion offers lower labor costs and greater flexibility, at the cost of potential fines if detected. In this context, an increase in inspections or the probability of being inspected raises the expected cost of non-compliance (Viollaz, 2018).

Figure 2.3 presents estimates of the impact of inspections on establishment-level outcomes. These effects may reflect both the consequences of punishment and other behavioral responses. We examine two potential channels through which establishments may respond to inspections.

First, inspected establishments may be more likely to incur fines. In this case, inspections function as a cost shock. Establishments facing higher expected costs may respond by adjusting their workforce size or reducing labor turnover.[20]

Second, even in the absence of formal penalties, inspections may shift establishments' beliefs about the probability of future monitoring, inducing greater compliance. This deterrence mechanism may trigger behavioral changes even when no violations are formally identified. This channel is consistent with our empirical findings and with prior evidence from Brazil (Almeida & Carneiro, 2009, 2012; Abras *et al.*, 2018), where city-level variation in inspection intensity suggests that enforcement can affect labor outcomes through both punishment and deterrence[21].

We empirically test these hypotheses by analyzing establishments' responses to the specific characteristics of the inspections they experience.

[20]Both hiring and firing entail adjustment costs, so declines in these margins may represent reductions in variable labor costs.

[21]By using city-level data and local variation in the intensity of inspections, these studies likely produce estimated effects that reflect a combination of deterrence and punishment mechanisms.

2.5.1 Extensive Margin: Inspected but Not Notified vs. Inspected and Notified

To analyze the mechanisms driving establishments' responses to inspections, we first examine the extensive margin by comparing three groups: establishments that were inspected but not notified, inspected and notified, and inspected and fined. This comparison helps distinguish whether responses are primarily driven by penalties or by the deterrence effects of inspections.

Table 2.2 shows that establishments inspected but not notified (Panel A) experience effects nearly as large as those observed among notified (Panel B) and fined (Panel C) establishments across key outcomes (Figures 2.13, 2.14, 2.16-2.20 in Appendix). For instance, the probability of exit increases by 8.1% for inspected but not notified firms, compared to 9.9% for notified and 9.2% for fined establishments. This suggests that inspections alone, even absent penalties, substantially raise the likelihood of exit.

Table 2.2: The average effects of inspection on Establishments' outcomes

	(1) Exit	(2) Ln(number of employees)	(3) Ln(number of hirings)	(4) Ln(number of separation)	(5) Ln(wages)
Panel A: Establishments inspected without notification					
Post ∗ Inspected	0.0806***	-0.0721***	-0.0900***	-0.0387**	-0.0057
	(0.0009)	(0.0086)	(0.0176)	(0.0150)	(0.0047)
Mean of the variable in the pre-treatment	-	30.93	7.17	6.37	1,311.09
Nº of Inspected Establishments	72,844	38,162	38,162	38,162	38,162
Nº of Establishments	261,480	38,162	38,162	38,162	38,162
Panel B: Establishments inspected with notification					
Post ∗ Inspected	0.0988***	-0.0972***	-0.1041***	-0.1171***	-0.0306***
	(0.0020)	(0.0159)	(0.0249)	(0.0342)	(0.0093)
Mean of the variable in the pre-treatment	-	34.46	7.18	6.48	1,310.81
Nº of Inspected Establishments	24,416	12,477	12,477	12,477	12,477
Nº of Establishments	216,052	12,477	12,477	12,477	12,477
Panel C: Establishments inspected with fines					
Post ∗ Inspected	0.0921***	-0.0834***	-0.0694**	-0.0796**	-0.0289**
	(0.0023)	(0.0157)	(0.0309)	(0.0331)	(0.0104)
Mean of the variable in the pre-treatment	-	34.21	7.17	6.37	1,290.35
Nº of Inspected Establishments	15,738	8,583	8,583	8,583	8,583
Nº of Establishments	204,374	8,583	8,583	8,583	8,583

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: exit, ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. All columns refer to Equation (1) restricted to one of the following characteristics: establishments inspected without notification, establishments inspected with notification, and establishments inspected with fines. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level. Event study estimates are presented in Figures 2.16-2.20 in the Appendix.

Employment and hiring show similar patterns to the exit variable, with reductions observed even among establishments that were inspected but not notified. This suggests that inspections alone may discourage hiring, potentially limiting establishments' expansion. In contrast, separations respond more strongly when penalties are applied: the effect is -3.9% for inspected but not notified establishments, compared to -11.7% and -7.9% for notified and fined establishments, respectively. This pattern implies that separation decisions are more closely tied to the financial burden of penalties and the costs associated with compliance.

For wages, statistically significant declines are observed only among notified or fined establishments (Column 5), suggesting that wage adjustments are more closely tied to the cost of penalties than to deterrence alone.

Taken together, the results indicate that both deterrence and punishment mechanisms shape establishments' responses to inspections. The sizable effects among establishments that are inspected but not penalized point to a strong role for deterrence, particularly in employment and hiring decisions. At the same time, the amplified effects among notified and fined establishments —especially in separations and wages —highlight the additional burden imposed by penalties. These findings imply that inspections affect establishment behavior not only through direct enforcement but also by altering beliefs about future inspections.

2.5.2 Intensive Margin: Punishment Intensity

In this section, we examine the intensive margin, exploring how different dimensions of punishment intensity influence establishment behavior.

2.5.2.0 Number of notifications

We first analyze the number of notifications to assess whether establishments respond differently to receiving a single notification compared to multiple notifications (Table 2.3 and Figures 2.13, 2.14, 2.21-2.25 in the Appendix). The results suggest that additional notifications do not generate substantially larger effects on establishments' behavior.

Table 2.3: The average effects of inspection on Establishments' outcomes

	(1) Exit	(2) Ln(number of employees)	(3) Ln(number of hirings)	(4) Ln(number of separation)	(5) Ln(wages)
Panel A: Establishments that received one notification					
Post ∗ Inspected	0.0866***	-0.1221***	-0.1105***	-0.1598***	-0.0349**
	(0.0015)	(0.0255)	(0.0353)	(0.0511)	(0.0120)
Mean of the variable in the pre-treatment	-	33.50	7.19	6.85	1,348.79
Nº of Inspected Establishments	11,948	5,422	5,422	5,422	5,422
Nº of Establishments	200,584	5,422	5,422	5,422	5,422
Panel B: Establishments that received more than one notification					
Post ∗ Inspected	0.0994***	-0.0843***	-0.1039**	-0.0872**	-0.0276**
	(0.0016)	(0.0201)	(0.0405)	(0.0415)	(0.0150)
Mean of the variable in the pre-treatment	-	35.18	7.17	6.21	1,282.77
Nº of Inspected Establishments	15,468	7,055	7,055	7,055	7,055
Nº of Establishments	204,104	7,055	7,055	7,055	7,055

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: exit, ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. All columns refer to Equation (1) restricted to one of the following characteristics: establishments that received one notification and establishments that received more than one notification. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level. Event study estimates are presented in Figures 2.21-2.25 in the Appendix.

Although average exit probabilities are similar between firms with one (8.6%) and multiple notifications (9.9%), the effect is slightly larger in the latter group. In contrast, effects on employment, hiring, separations, and wages are more pronounced among establishments that received only one type of notification. This suggests that receiving a notification – regardless of the number of infractions – is already sufficient to trigger behavioral adjustments. The marginal impact of additional infractions appears limited across these labor outcomes. One possible explanation is that establishments notified for multiple violations may be more aware of their noncompliance and better equipped to manage the regulatory consequences, either because they have anticipated enforcement or because they have developed internal capacity to deal with it.

2.5.2.0 Cause of notification

Different types of infractions are likely to lead to distinct reactions from establishments due to the varying financial and operational implications associated with each violation. For example, infractions related to safety regulations might lead to immediate compliance measures, such as adjustments in workplace conditions or investments in safety equipment, whereas infractions related to labor contracts or informal employment might primarily impact workforce structure or hiring practices. To isolate the effects of each infraction type and avoid confounding, the analysis focuses on establishments that were notified for a single type of irregularity.

By examining the differential effects of various infraction types, Table 2.4, we aim to identify whether the nature of the infraction influences the magnitude and direction of establishments' responses (Figures 2.13, 2.15, 2.26-2.30 in the Appendix).

Table 2.4: The average effects of inspection on Establishments' outcomes

	(1) Exit	(2) Ln(number of employees)	(3) Ln(number of hirings)	(4) Ln(number of separation)	(5) Ln(wages)
Panel A: Health and Safety					
Post ∗ Inspected	0.0763***	-0.1451*	-0.2258*	-0.6109	-0.0122
	(0.0039)	(0.0842)	(0.1156)	(0.4876)	(0.0235)
Mean of the variable in the pre-treatment	-	38.05	9.10	7.84	1,237.59
Nº of Inspected Establishments	920	363	363	363	363
Nº of Establishments	189,556	363	363	363	363
Panel B: Informal Woker					
Post ∗ Inspected	0.0449***	0.0394	-0.0061	-0.0440	-0.0079
	(0.0049)	(0.0649)	(0.0910)	(0.1291)	(0.0166)
Mean of the variable in the pre-treatment	-	23.35	5.88	5.05	983.93
Nº of Inspected Establishments	238	834	834	834	834
Nº of Establishments	188,874	834	834	834	834
Panel C: Remuneration					
Post ∗ Inspected	0.1022***	-0.0671	-0.0039	-0.3161	-0.0735
	(0.0034)	(0.074)	(0.1326)	(0.2411)	(0.0521)
Mean of the variable in the pre-treatment	-	30.99	6.69	6.32	1,273.29
Nº of Inspected Establishments	2,569	933	933	933	933
Nº of Establishments	191,205	933	933	933	933
Panel D: Working Time					
Post ∗ Inspected	0.0670***	-0.0528*	-0.0751	-0.0123	-0.0067
	(0.0025)	(0.0296)	(0.0637)	(0.0653)	(0.0131)
Mean of the variable in the pre-treatment	-	28.03	7.01	6.08	1,307.28
Nº of Inspected Establishments	2,145	1,046	1,046	1,046	1,046
Nº of Establishments	190,781	1,046	1,046	1,046	1,046
Panel E: Contributions					
Post ∗ Inspected	0.0723***	-0.1380	-0.1112	-0.2288	-0.0238
	(0.0090)	(0.1280)	(0.2655)	(0.1869)	(0.0381)
Mean of the variable in the pre-treatment	-	28.87	4.08	6.46	1,041.58
Nº of Inspected Establishments	133	335	335	335	335
Nº of Establishments	188,769	335	335	335	335

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: exit, ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. All columns refer to Equation (1) restricted to one of the following characteristics: notified of irregularities in workers' health and safety, notified for informal work, notified of irregularities in remuneration, notified of irregularities in working time, and notified of irregularities in contributions. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level. Event study estimates are presented in Figures 2.26-2.30 in the Appendix.

Establishment exit is most strongly associated with remuneration-related infractions, which increase the probability of exit by 10.2%. This likely reflects the high financial burden of wage compliance, including fines and payroll adjustments. In contrast, inspections for informal labor lead to a smaller increase in exit probability (4.5%), suggesting that these establishments may find it easier to regularize their workforce without compromising viability. Inspections related to health and safety (7.6%) and mandatory contributions (7.2%) also significantly raise exit rates. Violations related to working time show similar but slightly smaller effects.

Employment effects are most pronounced for establishments inspected for health and safety violations, which experience a statistically significant decline of 14.5%. This suggests that compliance with safety regulations may entail substantial labor force adjustments, possibly due to the costs of implementing safety protocols, upgrading equipment, or meeting regulatory standards. Moreover, compliance in this domain may require not only providing adequate protective equipment, but also ensuring that workers consistently use it as required. Since noncompliance by a single worker may still result in penalties, even when proper equipment is available, this introduces an additional layer of unpredictability and monitoring burden for establishments.

In contrast, inspections related to informal labor, remuneration, and mandatory contributions do not lead to significant changes in employment. For informal labor in particular, the estimated effect is small and positive (3.9%), albeit not statistically significant. This result may reflect the formalization of previously informal workers, suggesting that compliance with labor registration requirements does not necessarily lead to immediate workforce reductions and may even result in a net increase in recorded employment if workers are absorbed into formal contracts.

Hiring follows a similar pattern: health and safety inspections are associated with a significant decline of 22.6%, while no significant changes are observed for other types of infractions. For separations and wages, none of the coefficients are statistically significant across infraction types.

2.5.2.0
Punishment Amount

To analyze the role of penalty amount, we consider two metrics. The first is the total value of fines imposed on establishments following the inspection. The second ponder this total by the number of infractions cited, capturing the average fine per violation (Figures 2.13, 2.14, 2.31-2.35 in the Appendix).

Panels A and B (Table 2.5) report the effects for establishments receiving fines above and below the median total amount. While exit probabilities exhibit greater variation among those fined the most, effects on other outcomes are mixed and lack a consistent pattern.

Panels C and D compare establishments based on the average fine per infraction. Interestingly, the exit effect is stronger among those fined less per infraction (9%), while other variables again show no clear directional pattern. Overall, we find no systematic evidence that larger penalties are associated with stronger behavioral responses.

Table 2.5: The average effects of inspection on Establishments' outcomes

	(1) Exit	(2) Ln(number of employees)	(3) Ln(number of hirings)	(4) Ln(number of separation)	(5) Ln(wages)
Panel A: Amount of Fine - Less than R$ 3,000					
Post ∗ Inspected	0.0904***	-0.0977***	-0.0107	-0.1654**	-0.0308*
	(0.0020)	(0.0261)	(0.0461)	(0.0690)	(0.0170)
Mean of the variable					
in the pre-treatment	-	28.71	5.61	5.48	1,287.46
Nº of Inspected Establishments	11,948	3,717	3,717	3,717	3,717
Nº of Establishments	195,217	3,717	3,717	3,717	3,717
Panel B: Amount of Fine - More than R$ 3,000					
Post ∗ Inspected	0.0922***	-0.0763***	-0.1168**	-0.0096	-0.0264*
	(0.0016)	(0.0179)	(0.0422)	(0.0355)	(0.0144)
Mean of the variable					
in the pre-treatment	-	38.41	8.37	7.04	1,292.56
Nº of Inspected Establishments	9,157	4,866	4,866	4,866	4,866
Nº of Establishments	197,793	4,866	4,866	4,866	4,866
Panel C: Amount of Fine per Notification - Less than R$ 1,600					
Post ∗ Inspected	0.0942***	-0.0381*	-0.0234	-0.1450**	-0.0403**
	(0.0019)	(0.0217)	(0.0408)	(0.0691)	(0.0192)
Mean of the variable					
in the pre-treatment	-	25.32	4.87	4.81	1,210.07
Nº of Inspected Establishments	7,503	4,255	4,255	4,255	4,255
Nº of Establishments	196,139	4,255	4,255	4,255	4,255
Panel D: Amount of Fine per Notification - More than R$ 1,600					
Post ∗ Inspected	0.0895***	-0.0949***	-0.0846**	-0.0516	-0.0205
	(0.0015)	(0.0193)	(0.0383)	(0.0362)	(0.0143)
Mean of the variable					
in the pre-treatment	-	42.94	9.44	7.91	1,369.27
Nº of Inspected Establishments	8,235	4,328	4,328	4,328	4,328
Nº of Establishments	196,871	4,328	4,328	4,328	4,328

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: exit, ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. All columns refer to Equation (1) restricted to one of the following characteristics: total fine amount less than R\$3,000, total fine amount more than R\$3,000, fine amount for notification less than R\$1,600, and fine amount for notification more than R\$1,600. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level. Event study estimates are presented in Figures 2.31-2.35 in the Appendix.

As in the analysis by infraction type, even small fines can trigger substantial adjustments if establishments are unprepared for the cost shock. As previously discussed, beyond the financial penalty itself, establishments must also absorb the cost of complying with the specific regulation violated. The lack of a clear relationship between penalty size and behavioral response suggests that enforcement severity and perceived severity, rather than monetary amount alone, may be key drivers of establishment reactions.

2.6 Effects of Labor Inspections on Workers Outcome

In the previous sections, we presented evidence that many establishments either exit or reduce their size, as well as their levels of hiring and separation following an inspection. In this section, we investigate whether the effects of inspections on establishments have implications for workers' trajectories in the labor market.

2.6.1 Matching Differences-in-Differences Approach

We implement a matching difference-in-differences approach to investigate worker-level outcomes. The treated group consists of individuals who were working at inspected establishments at the time the inspection took place. We first describe the matching algorithm used to identify a comparable control group and then discuss the implementation of the event study.

2.6.1.1 Matching algorithm

For simplicity, we restrict the worker-level analysis to individuals employed at establishments that experiences an inspection in 2011. This ensures pre- and post-treatment windows of six years. We then define a potential control group consisting of workers never employed at establishments inspected between 2007 and 2017.

We adopt an exact matching approach to select control individuals from the potential control group. In this approach, each treated worker is matched to a control worker, and when multiple eligible control workers are available, we randomly select one. The algorithm selects control workers within exact matching categories defined by the following characteristics: employment status in 2011; number of years employed in a formal job between 2007 and 2010; age group (7 categories); education level (3 categories); occupation in 2010 (4 categories); wage quartile in 2010; establishment size quartile in 2010; sector of activity in 2010 (17 categories); and workplace state in 2010 (27 categories).

Using this approach, we successfully match 37,250 treated workers to control units. Table 2.13 in Appendix 2.7 presents descriptive statistics for the treated and control groups in 2010, demonstrating that both groups are balanced across demographic and labor market characteristics.

2.6.1.2 Event-Study

We estimate the following specification using worker-level data from RAIS, considering the treatment and control groups defined above:

$$y_{it} = \sum_{\substack{k=-6 \\ k\neq 0}}^{k=6} \beta_k 1[t = 2011 + k] \times Treated_t + \alpha_i + \alpha_t + \epsilon_{it}, \tag{2-2}$$

where subscripts i and t represent worker and year, respectively; $1[t = 2011+k]$ is an indicator variable that equal one for each year, except the year of the inspection;

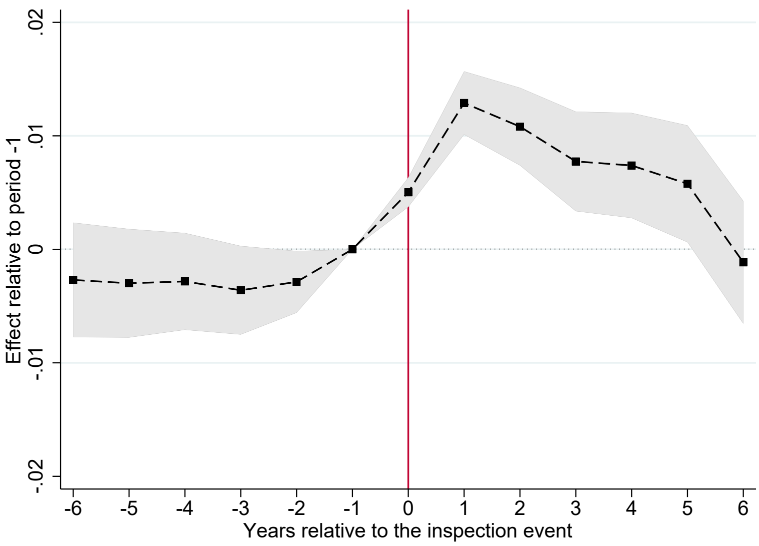
$Treated_t$ is an indicator variable equal to one for individuals employed at inspected establishments during the event year and zero for individuals in the control group, as defined in the previous section; α_i denotes worker fixed effects; α_t denotes year fixed effects; and y_{it} represents the labor market outcome of interest, such as employment or wages. In this analysis, standard errors are clustered at the worker level.

Under the identifying assumptions, the coefficients β_k for $k > 0$, capture the average treatment effect of labor inspections on the treated workers for each period year following the inspection. The key identifying assumption is that, in the absence of an inspection, the outcomes of workers in treated and control establishments would have followed parallel trends. We test this assumption by verifying that the coefficients for the pre-inspection periods are not statistically different from zero.

2.6.2
Effects of Labor Inspections on Employment

Figure 2.5 presents the estimated dynamic effects of labor inspections on workers' employment status, using Equation 2-2. The pre-inspection period (k<0) exhibits no significant trend in employment differences between treated and control groups, with coefficients fluctuating around zero. This suggests that the parallel trends assumption holds, supporting the validity of our identification strategy. The absence of systematic pre-trends indicates that, in the absence of inspections, treated and control workers would have followed similar employment trajectories.

Figure 2.5: The effects of inspection on Employment



Note: This figure reports point estimates of the effects of inspection on employment variable using the worker-level sample from RAIS data. Employment variable is a dummy equals 1 if the worker was employed in k, and 0 otherwise. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Following the inspection, treated workers experience a modest but statistically significant short-term increase in employment probability relative to the control group. One year after the inspection, the probability of employment increases by approximately 1%, but this effect gradually fades, converging to zero within six years.

The relative employment growth in the first period after the inspection may directly reflect establishments' decisions to reduce separations, as documented in Section 2.4. Inspections prompt firms to adjust their labor practices, reducing both hiring and separations in response to compliance pressures. As a result, workers employed at these establishments may experience greater job stability in the short run, contributing to the observed increase in employment probability. Over time, as establishments adapt to regulatory constraints, this effect diminishes, and employment probabilities gradually return to pre-inspection levels.

The key takeaway from these results is that there is no evidence of a negative impact of inspections on workers' employment status. As shown in Section 2.4, establishments experience substantial employment reductions following an inspection. However, these reductions do not adversely affect incumbent workers, as those who are dismissed or choose to leave are just as likely to find another job as their counterparts in the control group. These findings are consistent with evidence from labor inspections in Mexico, which also show that inspections do not lead to negative employment effects for incumbent workers (Samaniego de la Parra & Bujanda, 2024), easing concerns that regulatory enforcement could cause widespread job losses.[22]

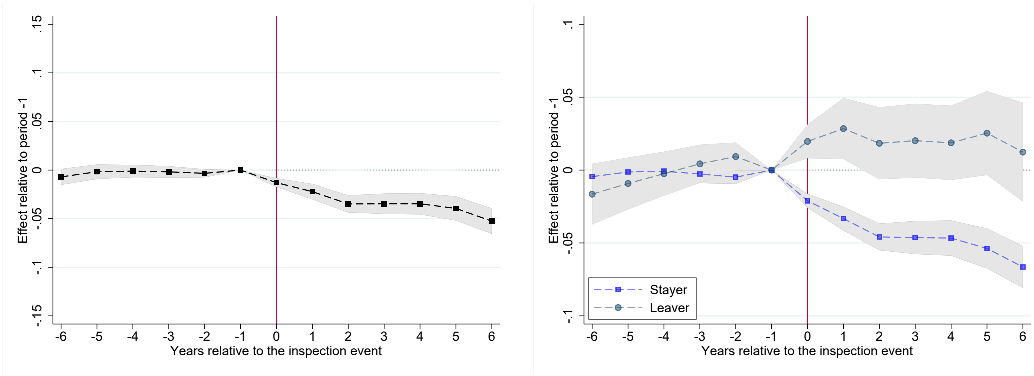
2.6.3 Effects of Labor Inspections on Earnings

Figure 2.6 presents the dynamic effects of labor inspections on workers' wages, estimated using the specification in Equation 2-2. Panel A shows a statistically significant decline in wages following the inspection, with a persistent downward trend throughout the analysis period. Six years after the inspection, treated workers earn 5% less than their counterparts in the control group. Since real wages are rising over the analysis period, this result should be interpreted not as an absolute decline in real wages, but as slower wage growth relative to the control group. The estimates reflect the wage dynamics of workers who remained employed in the formal sector, either by staying at the same establishment or by transitioning to a different employer.

[22]In (Samaniego de la Parra & Bujanda, 2024), workers initially employed in formal jobs experienced a sustained increase in the probability of remaining in formal employment after an inspection.

Figure 2.6: The effects of inspection on Earnings

(a) All workers (b) Stayers *vs* leavers



Note: This figure reports point estimates of the effects of inspection on earnings variables using the worker-level sample from RAIS data. Panel A plots the estimates considering all workers. Panel B plots the separate estimates for stayers and leavers. Stayers are the workers who remained in the same job they were in at $t = -1$. Leavers are the workers who changed jobs (employers). The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Panel B of Figure 2.6 presents the estimated effects of inspections on wages separately for stayers—workers who remain with the same employer—and leavers—workers who transition to a different employer after the inspection. The control group for stayers consists of workers who also remained with their employer from 2011 onward. The control group for leavers consists of workers who switched to a different employer after 2011.

Stayers experience a significant and persistent decline in wages. As before, we interpret this not as an actual wage decrease, but as slower wage growth, as showed by Appendix Figure 2.36. This pattern may arise for several reasons, two of which we highlight. First, establishments may freeze human resource decisions following an inspection—including hiring, separations, promotions, and wage adjustments. As a result, wage growth may be higher in control group establishments, leading to a relative decline in wages for stayers in inspected firms. Second, workers who choose to remain at an inspected establishment may be negatively selected relative to stayers in non-inspected firms. These workers may be less motivated and, as a result, experience slower wage growth.

Conversely, leavers exhibit a different pattern, with a small but statistically significant increase in wages in the period following the inspection, that appears to persist in the following periods.

In summary, despite the significant and statistically robust effects at the establishment level, the evidence does not support the hypothesis that these impacts translate into higher unemployment or lower wages for workers. Instead, the findings suggest a small short-term increase in employment, no effect on the wages of workers who change jobs, and a slower wage growth —around 4% lower —for workers who stay in the inspected establishment.

2.7 Final Considerations

This paper examines the effects of labor inspections on establishment and worker outcomes in Brazil. Using rich administrative data and a staggered difference-in-differences design, we document how regulatory enforcement shapes establishment dynamics and employment trajectories. Inspections significantly reduce employment, driven primarily by declines in hiring rather than separations, and increase the likelihood of firm exit—especially among young and medium-sized establishments. At the worker level, inspections do not reduce the employability of affected workers but lead to slower wage growth for those who remain in inspected firms, while workers who transition to new jobs experience stable or slightly improved earnings.

A key contribution of this study is the identification of two primary mechanisms through which inspections affect firm behavior. First, punishment effects arise when establishments receive fines or formal notifications, leading to immediate compliance costs and labor adjustments. Second, deterrence effects emerge even in inspected but not penalized establishments, as they internalize the risk of future enforcement and adjust their hiring and employment practices. These findings suggest that inspections influence firms not only through direct sanctions but also by shaping their expectations about future inspections.

Although labor inspections play a crucial role in enforcing compliance with labor regulations, their broader economic repercussions are more complex. The reduction in hiring and increased firm exit highlight potential unintended consequences, particularly for young and medium-sized firms that may face greater challenges in adjusting to compliance costs. Nevertheless, the lack of persistent negative employment effects at the worker level suggests that labor market transitions are relatively smooth, allowing displaced workers to reallocate without long-term adverse consequences.

This study contributes to the literature on labor regulation in developing economies by providing evidence on how enforcement mechanisms influence establishment dynamics and worker outcomes. By documenting the trade-offs in labor inspections, these findings offer valuable insights for the design of regulatory policies that improve worker protections while mitigating unintended economic distortions.

Appendix

Table 2.6: Types of violations verified during inspections

In portuguese	In english
Alteração contratual	Contract amendment
Aprendizagem profissional	Professional apprenticeship
Aviso prévio	Notice of termination/Prior notice
Cadastro Geral de Empregados e Desempregados	General Register of Employed and Unemployed Persons
Combate ao trabalho infantil	Combating child labor
Contrato individual de trabalho	Individual employment contract
Cooperativa de trabalho	Workers' cooperative
Da fiscalização	Related to labor inspection
Descanso	Rest period
Fundo de Garantia do Tempo de Serviço	Guarantee Fund for Length of Service
Férias	Vacation/Paid leave
Gratificação de natal	Christmas bonus/13th salary
Instituição sindical	Union institution
Jornada de trabalho	Working hours
Motorista profissional	Professional driver
Normas regulatórias	Occupational Health and Safety
Pessoas com deficiência	Persons with disabilities
Quadro horário	Work schedule
RAIS	Annual Social Information Report
Registro e CTPS	Registration and work card
Remuneração	Wage/Compensation
Salário mínimo	Minimum wage
Seguro desemprego	Unemployment insurance
Suspensão e da interrupção	Suspension and interruption of contract
Terceirização	Outsourcing
Trabalho da mulher	Women's work
Trabalho doméstico	Domestic work
Trabalho noturno	Night work
Trabalho rural	Rural work
Vale transporte	Transportation voucher

Note: This table presents the types of violations identified during labor inspections, with corresponding terms in both Portuguese and English. Categories are available at: https://sit.trabalho.gov.br/radar/

Table 2.7: The average effects of inspection on exit

	Size		Age		
	10-49	50-500	Up to 3	3-5	More than 5
Post × Inspected	0.0735***	0.0949***	0.1658***	0.1261***	0.0646***
	(0.0006)	(0.0022)	(0.0038)	(0.0026)	(0.0010)
Nº of Inspected Establishments	88,503	11,510	11,632	16,958	71,670
Nº of Establishments	259,593	28,729	33,031	44,420	211,445

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on the exit variable. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007-2017, and the control group. All columns refer to Equation (1) restricted to one of the following sizes and age categories: 10-49 employees, 50-500 employees, less than 3 years, 3-5 years, and more than 3 years. The number of establishments (total) and establishments inspected is reported. Standard errors are clustered at the establishment level.

Table 2.8: The average effects of inspection on exit

	Industry	Commerce	Services	Construction
Post × Inspected	0.1091***	0.1081***	0.0669***	0.1414***
	(0.0026)	(0.0021)	(0.0011)	(0.0067)
Nº of Inspected Establishments	26,468	34,385	42,301	4,797
Nº of Establishments	72,955	88,978	132,516	13,408

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on the exit variable. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007-2017, and the control group. All columns refer to Equation (1) restricted to one of the following sectors: industry, services, construction, and commerce. The number of establishments (total) and establishments inspected is reported. Standard errors are clustered at the establishment level.

Table 2.9: The average effects of inspection on Establishments' outcomes

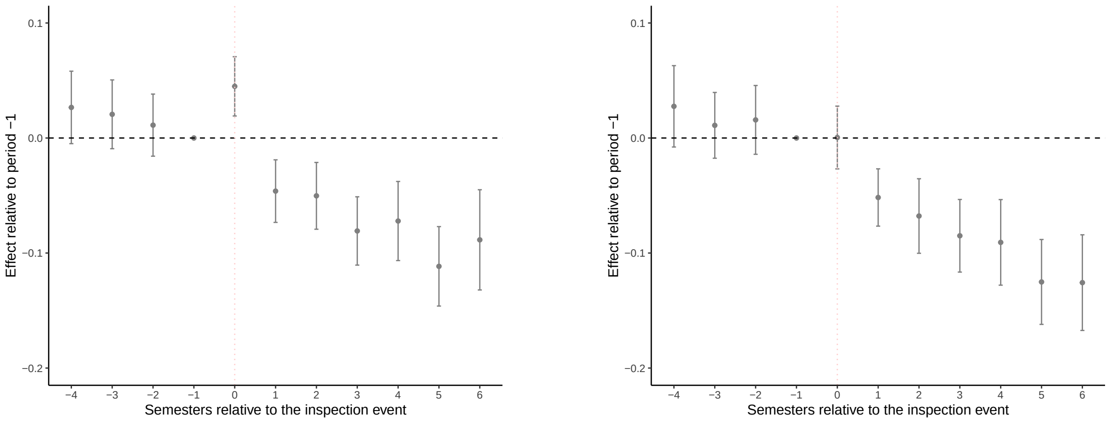
	(1)	(2)	(3)	(4)	(5)
	Exit	Ln(number of employees)	Ln(number of hirings)	Ln(number of separation)	Ln(wages)
Post × Inspected	0.0864***	-0.0780***	-0.0952***	-0.0531***	-0.0124***
	(0.0009)	(0.0054)	(0.0113)	(0.0116)	(0.0040)
Mean of the variable					
in the pre-treatment	-	30.11	6.28	5.83	1,333.85
Nº of Inspected Establishments	100,260	50,639	50,639	50,639	50,639
Nº of Establishments	288,896	50,639	50,639	50,639	50,639

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: exit, ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level.

Figure 2.7: The effects of inspection on establishments' number of hired workers

(a) Ln(number of hires)
Out of formal labor market in $t = -1$

(b) Ln(number of hires)
Active in the formal labor market in $t = -1$

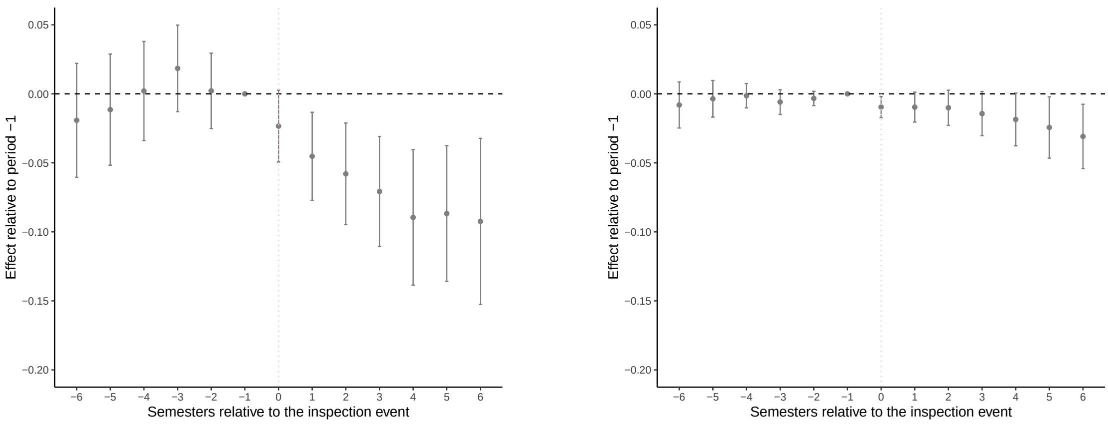


Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The outcome variable in Panel A refers to the number of workers hired in the year who were not in the formal market in the previous year. The outcome variable in Panel B refers to the number of workers hired in the year who were in the formal market in the previous year. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.8: The effects of inspection on Establishments' outcomes

(a) 10th percentile of the wage distribution

(b) 90th percentile of the wage distribution



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Table 2.10: The average effects of inspection on Establishments' outcomes

	(1)	(2)	(3)	(4)
	Ln(number of employees)	Ln(number of hirings)	Ln(number of separation)	Ln(wages)
Panel A: Establishment with size between 10 to 49				
Post × Inspected	-0.0666***	-0.0770***	-0.0508***	-0.0084***
	(0.0031)	(0.0057)	(0.0051)	(0.0019)
Mean of the variable				
in the pre-treatment	22.72	5.44	4.93	1,262.42
Nº of Inspected Establishments	45,684	45,684	45,684	45,684
Panel B: Establishment with size between 50 to 500				
Post × Inspected	-0.1034***	-0.1350***	-0.0696***	-0.0203**
	(0.0157)	(0.0278)	(0.0299)	(0.0101)
Mean of the variable				
in the pre-treatment	116,49	23,28	20,00	1,763.87
Nº of Inspected Establishments	4,833	4,833	4,833	4,833

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. All columns refer to Equation (1) restricted to one of the following size categories: 10-49 employees and 50-500 employees. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level.

Table 2.11: The average effects of inspection on Establishments' outcomes

	(1)	(2)	(3)	(4)
	Ln(number of employees)	Ln(number of hirings)	Ln(number of separation)	Ln(wages)
Panel A: Establishment with ages up to 3 years				
Post × Inspected	-0.1556***	-0.2093***	-0.1363***	-0.0219
	(0.0199)	(0.0398)	(0.0368)	(0.0176)
Mean of the variable				
in the pre-treatment	32.51	9.77	8.55	1,321.16
Nº of Inspected Establishments	4,310	4,310	4,310	4,310
Panel B: Establishment aged between 3 and 5 years				
Post × Inspected	-0.1034***	-0.1284***	-0.1026**	-0.0218**
	(0.0163)	(0.0222)	(0.0351)	(0.0080)
Mean of the variable				
in the pre-treatment	27.78	8.11	7.48	1,195.33
Nº of Inspected Establishments	7,630	7,630	7,630	7,630
Panel C: Establishment older than 5 years				
Post × Inspected	-0.0646***	-0.0740***	-0.0312**	-0.0099**
	(0.0062)	(0.0132)	(0.0131)	(0.0044)
Mean of the variable				
in the pre-treatment	32.54	6.70	5.95	1,332.51
Nº of Inspected Establishments	38,699	38,699	38,699	38,699

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. All columns refer to Equation (1) restricted to one of the following age categories: less than 3 years, 3-5 years, and more than 3 years. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level.

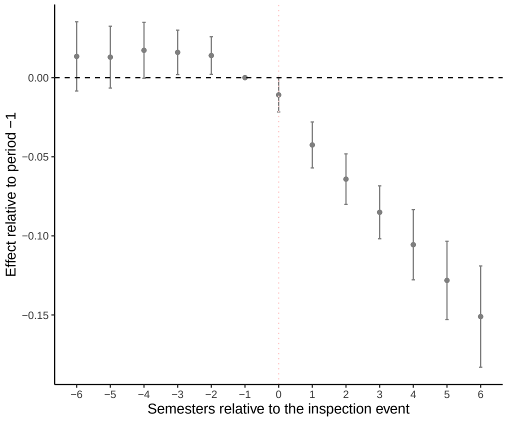
Table 2.12: The average effects of inspection on Establishments' outcomes

	(1) Ln(number of employees)	(2) Ln(number of hirings)	(3) Ln(number of separation)	(4) Ln(wages)
Panel A: Industry				
Post × Inspected	-0.0982***	-0.1240***	-0.0683***	-0.0073
	(0.0110)	(0.0249)	(0.0205)	(0.0082)
Mean of the variable				
in the pre-treatment	23.27	5.43	5.27	1,131.09
Nº of Inspected Establishments	13,296	13,296	13,296	13,296
Panel B: Commerce				
Post × Inspected	-0.0574***	-0.0709***	-0.0390**	-0.0082**
	(0.0086)	(0.0120)	(0.0145)	(0.0040)
Mean of the variable				
in the pre-treatment	21.53	5.43	5.04	1,096.89
Nº of Inspected Establishments	17,184	17,184	17,184	17,184
Panel C: Services				
Post × Inspected	-0.0610***	-0.0713***	-0.0284	-0.0155**
	(0.0096)	(0.0183)	(0.0193)	(0.0054)
Mean of the variable				
in the pre-treatment	40.28	7.91	6.87	1,553.47
Nº of Inspected Establishments	23,024	23,024	23,024	23,024
Panel D: Construction				
Post × Inspected	-0.2281***	-0.2808**	-0.2715**	-0.0408
	(0.0573)	(0.1284)	(0.0967)	(0.0347)
Mean of the variable				
in the pre-treatment	31.68	12.24	10.74	1,229.51
Nº of Inspected Establishments	1,430	1,430	1,430	1,430

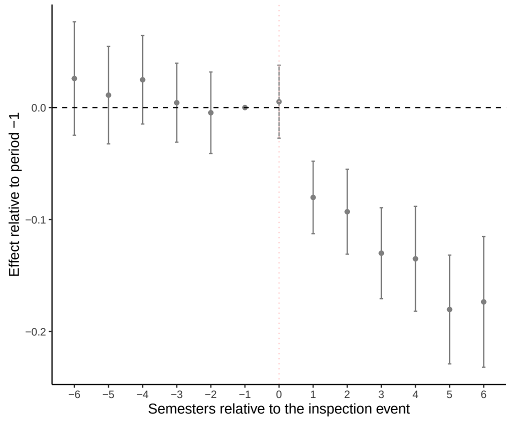
Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. All columns refer to Equation (1) restricted to one of the following sectors: industry, services, construction, and commerce. The number of establishments (total) and establishments inspected is reported. Means of dependent variables are computed from $t = -1$. Standard errors are clustered at the establishment level.

Figure 2.9: The effects of inspection on Establishments' outcomes - IHS transformation

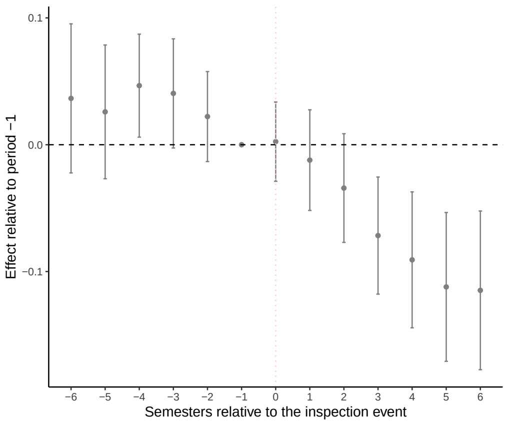
(a) IHS employment



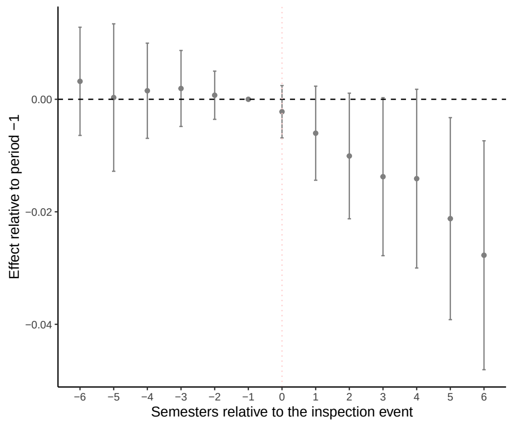
(b) IHS hiring



(c) IHS separation

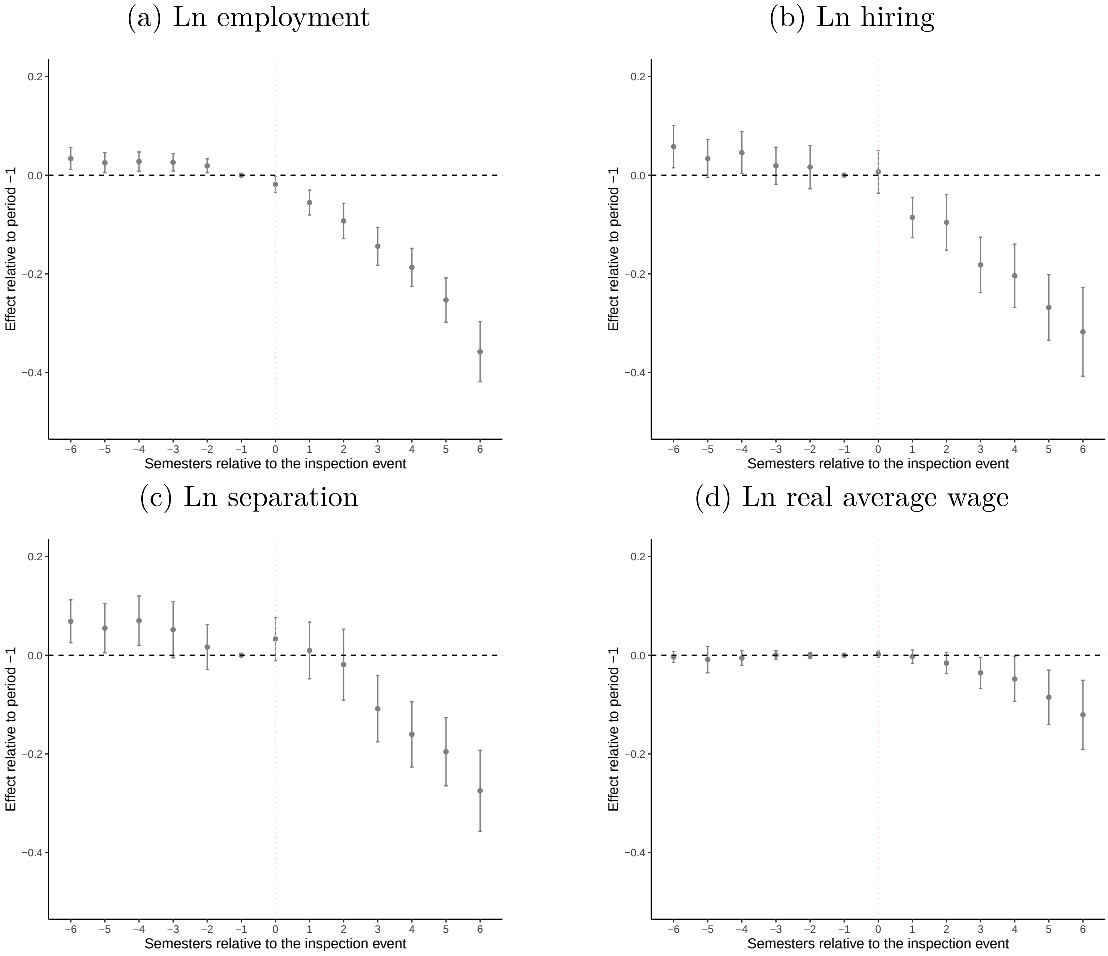


(d) IHS real average wage



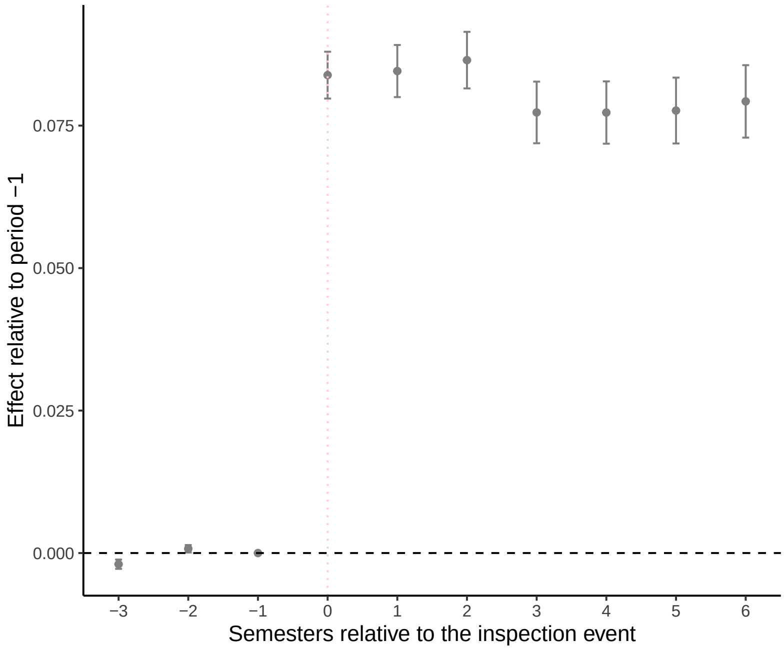
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. In this robustness, we apply the IHS transformation to the variables. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.10: The effects of inspection on Establishments' outcomes - stayers



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The sample is restricted to establishments that remain in the dataset within a [-6; +6] window around the inspection date. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

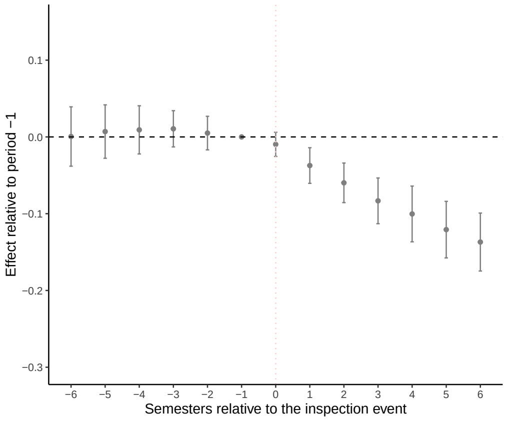
Figure 2.11: The effects of inspection on Exit - drop treated pre 2007



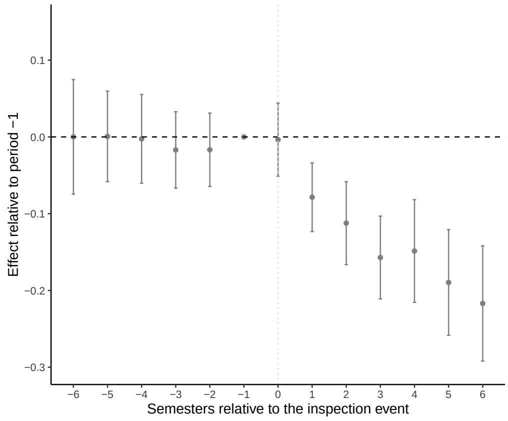
Note: This figure reports point estimates of the effects of inspection on exit variable using the establishment-level sample from RAIS data. The sample is restricted to establishments that were not inspected between 2003 and 2007, in addition to the construction restrictions of the main sample. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.12: The effects of inspection on Establishments' outcomes - drop treated pre 2007

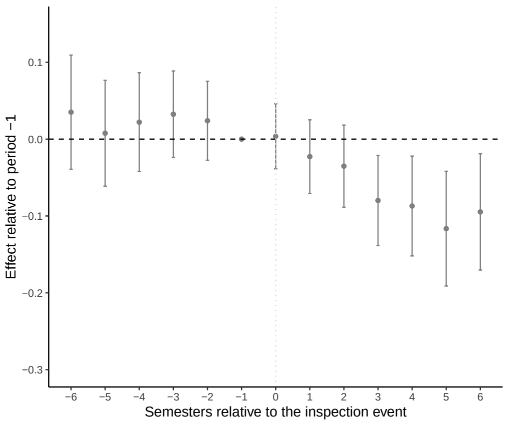
(a) Ln employment



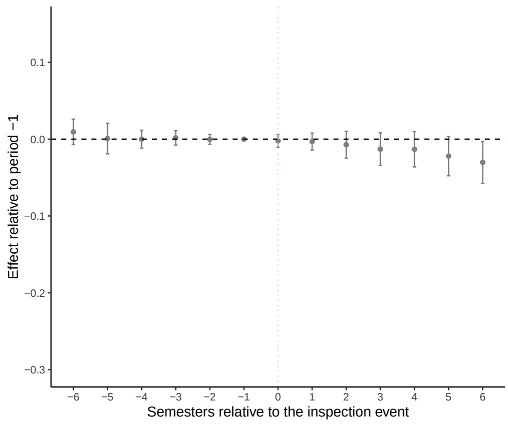
(b) Ln hiring



(c) Ln separation

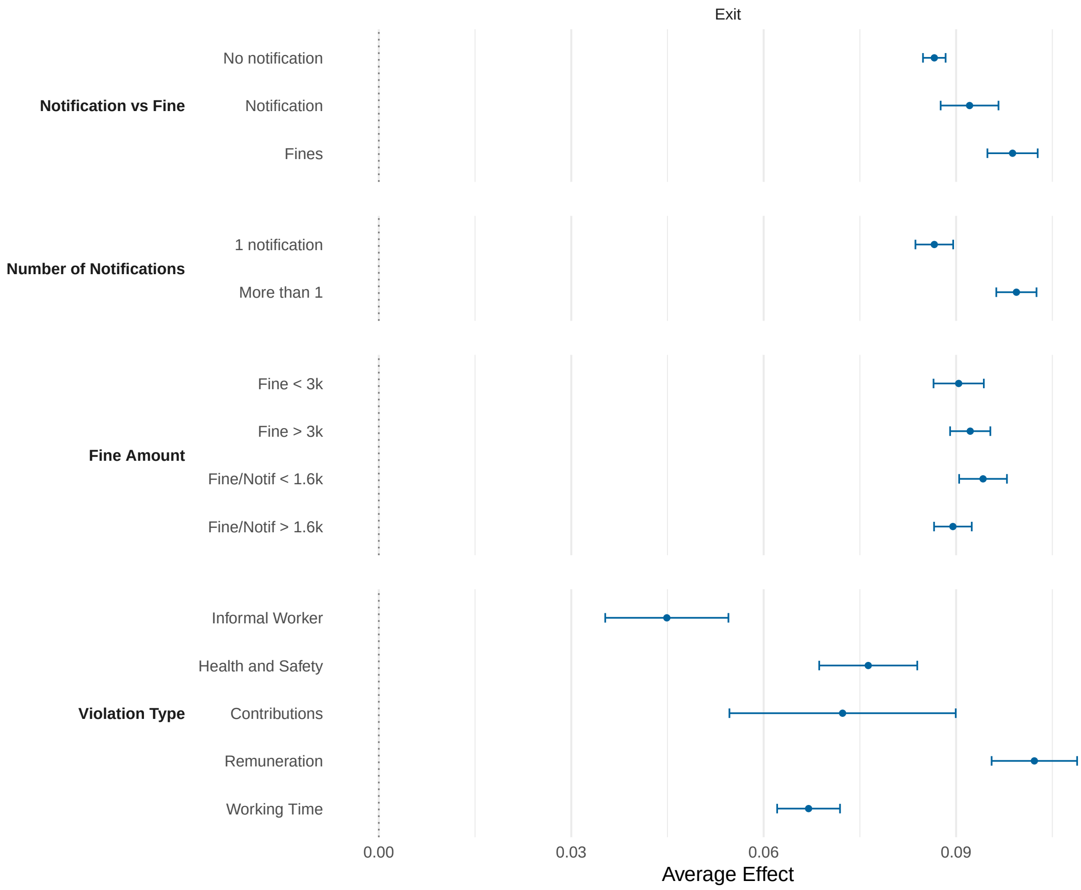


(d) Ln real average wage



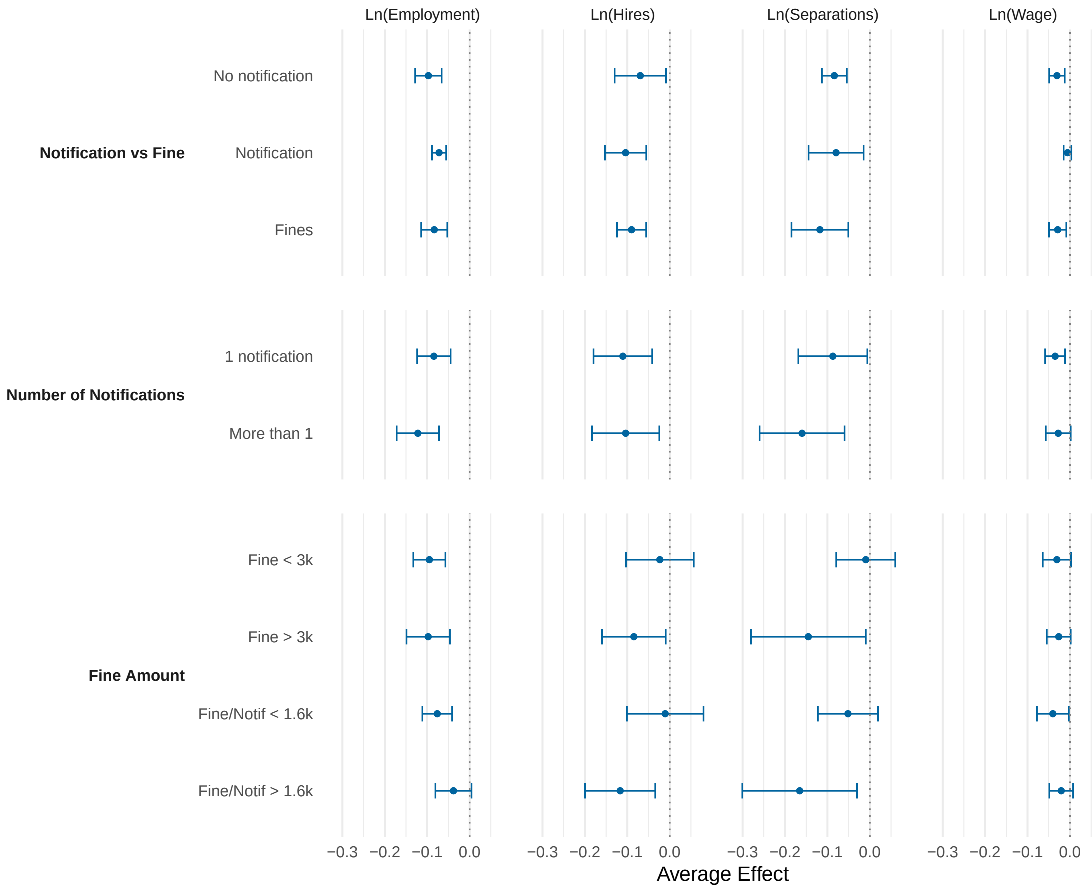
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The sample is restricted to establishments that were not inspected between 2003 and 2007, in addition to the construction restrictions of the main sample. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.13: Heterogeneous effects of inspections on establishments' outcomes



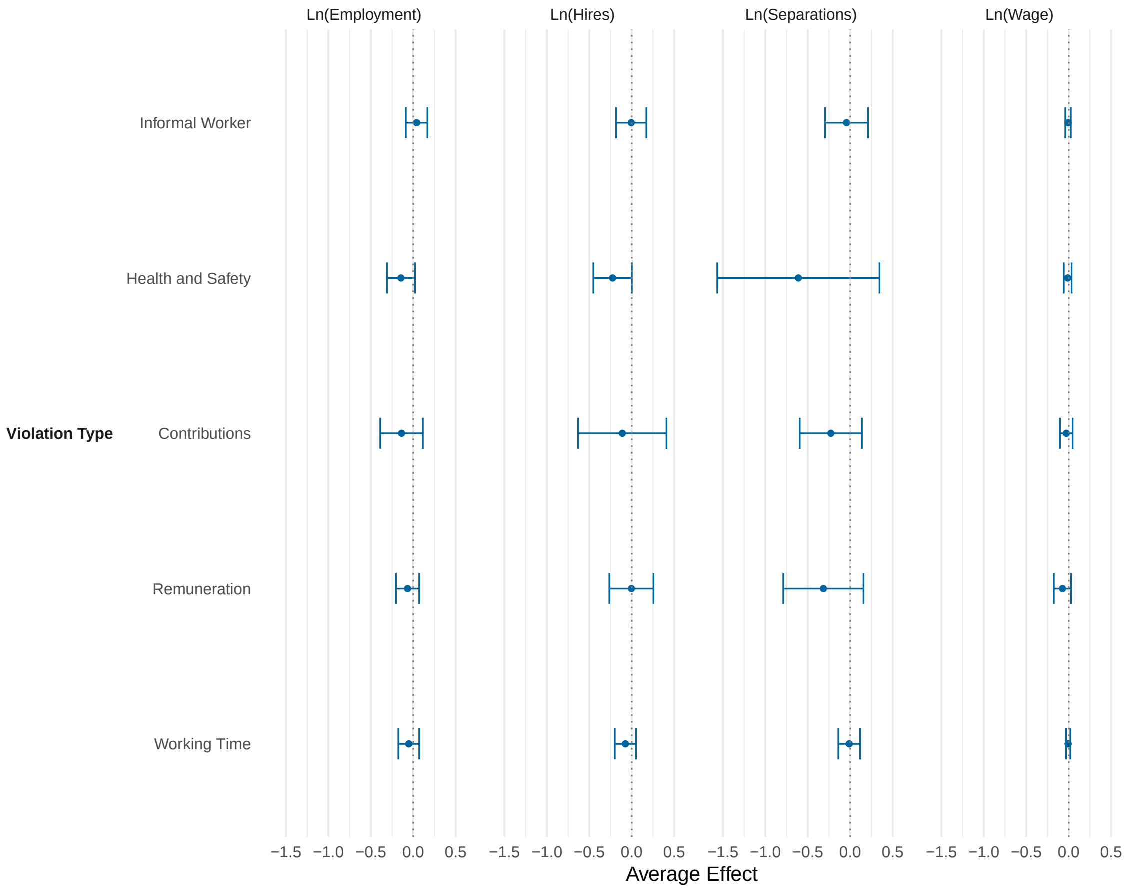
Note: This figure reports the aggregate effects of inspection on exit variable. Exit variable is a dummy equals 1 if the year t is the last observation of the establishments in the sample and $t \neq 2017$. The estimation sample consists of an annual panel of establishments from 10 to 500 inspected only once or never inspected between 2007 and 2017. 'Notification vs Fine' is divided into: establishments inspected without notification, establishments inspected with notification, and establishments inspected with fines. 'Number of Notification' is divided into: establishments that received one notification and establishments that received more than one notification. 'Fine Amount' is divided into: total fine amount less than R\$3,000, total fine amount more than R\$3,000, fine amount for notification less than R\$1,600, and fine amount for notification more than R\$1,600. 'Violation Type' is divided into: notified of irregularities in workers' health and safety, notified for informal work, notified of irregularities in remuneration, notified of irregularities in working time, and notified of irregularities in contributions. The figure plots point estimates and the 95% confidence interval. Standard errors are clustered at the establishment level.

Figure 2.14: Heterogeneous effects of inspections on establishments' outcomes



Note: The figure reports the aggregate effects of inspection on different outcomes: ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. 'Notification vs Fine' is divided into: establishments inspected without notification, establishments inspected with notification, and establishments inspected with fines. 'Number of Notification' is divided into: establishments that received one notification and establishments that received more than one notification. 'Fine Amount' is divided into: total fine amount less than R\$3,000, total fine amount more than R\$3,000, fine amount for notification less than R\$1,600, and fine amount for notification more than R\$1,600. The figure plots point estimates and the 95% confidence interval. Standard errors are clustered at the establishment level.

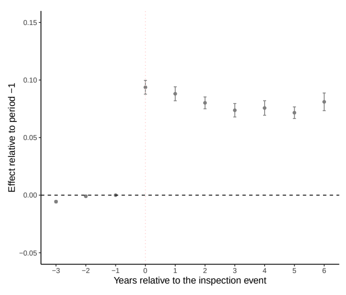
Figure 2.15: Heterogeneous effects of inspections on establishments' outcomes



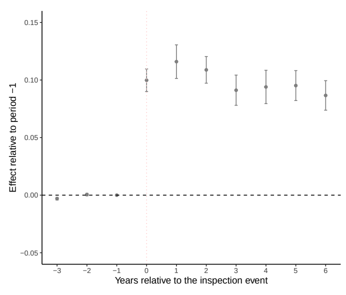
Note: The figure reports the aggregate effects of inspection on different outcomes: ln employment, ln hiring, ln separation, and ln real average wage. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. 'Violation Type' is divided into: notified of irregularities in workers' health and safety, notified for informal work, notified of irregularities in remuneration, notified of irregularities in working time, and notified of irregularities in contributions. The figure plots point estimates and the 95% confidence interval. Standard errors are clustered at the establishment level.

Figure 2.16: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Exit

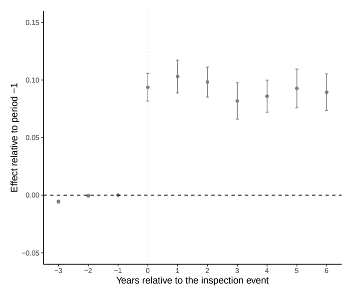
(a) Establishments inspected without notification



(b) Establishments inspected with notification



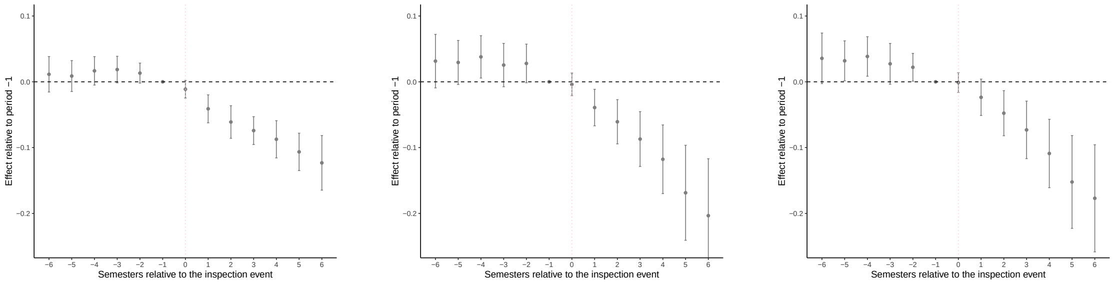
(c) Establishments inspected with fines



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.17: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of employees)

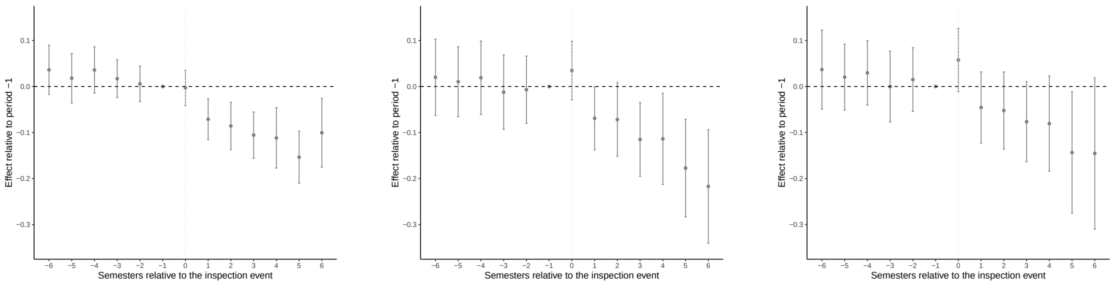
(a) Establishments inspected without notification (b) Establishments inspected with notification (c) Establishments inspected with fines



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.18: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of hirings)

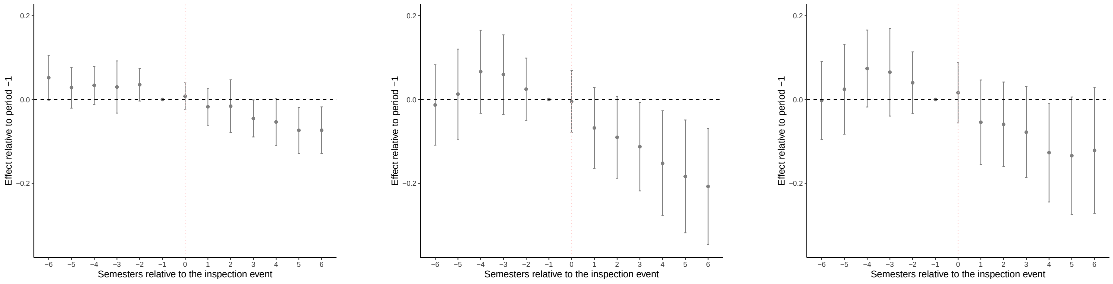
(a) Establishments inspected without notification (b) Establishments inspected with notification (c) Establishments inspected with fines



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.19: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of separations)

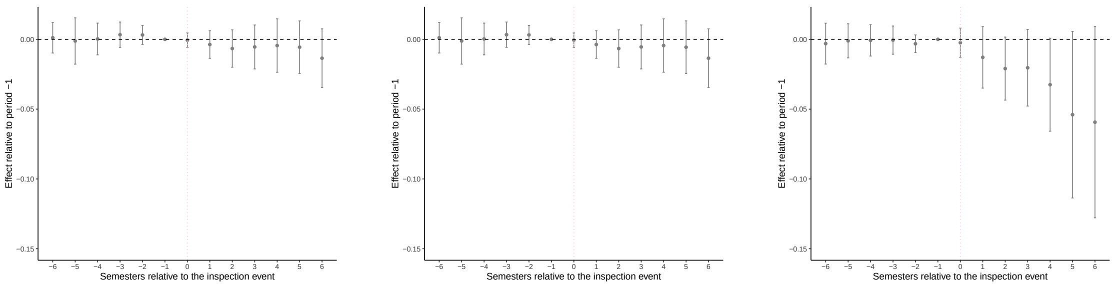
(a) Establishments inspected without notification (b) Establishments inspected with notification (c) Establishments inspected with fines



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.20: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(wages)

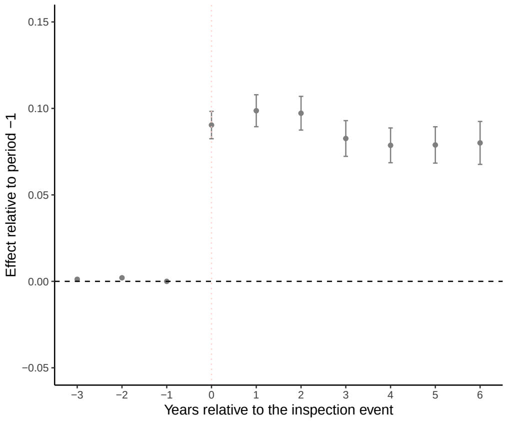
(a) Establishments inspected without notification (b) Establishments inspected with notification (c) Establishments inspected with fines



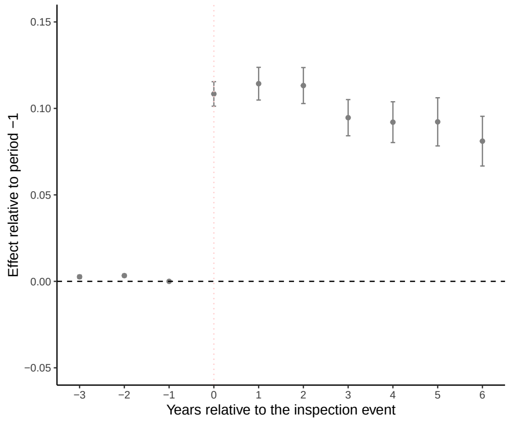
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.21: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Exit

(a) Establishments that received one notification



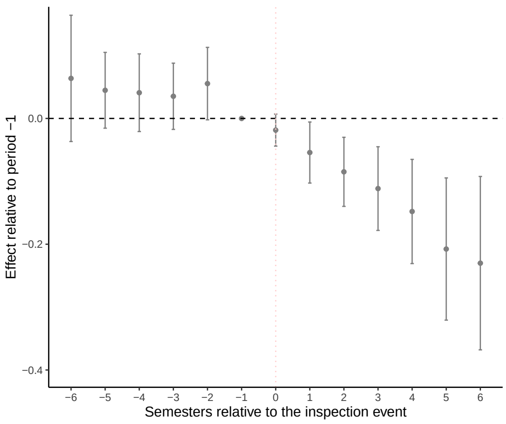
(b) Establishments that received more than one notification



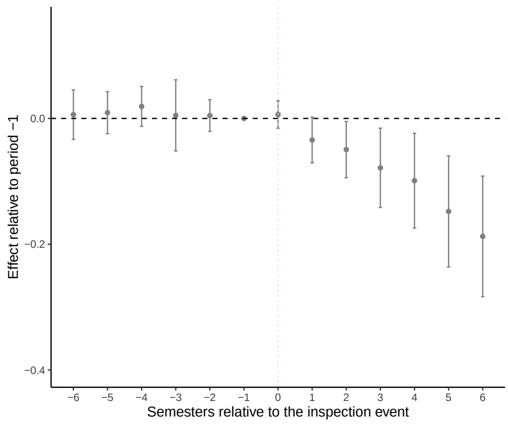
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.22: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of employees)

(a) Establishments that received one notification



(b) Establishments that received more than one notification

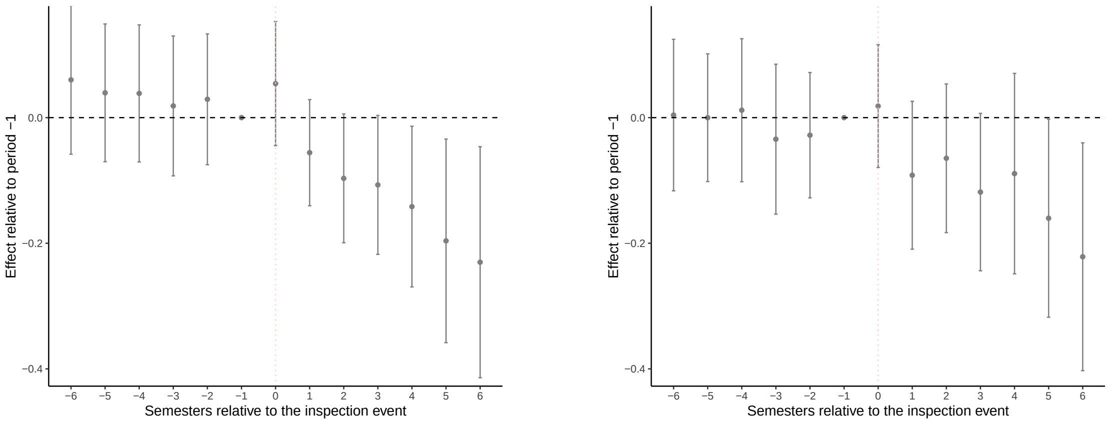


Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.23: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of hirings)

(a) Establishments that received one notification

(b) Establishments that received more than one notification

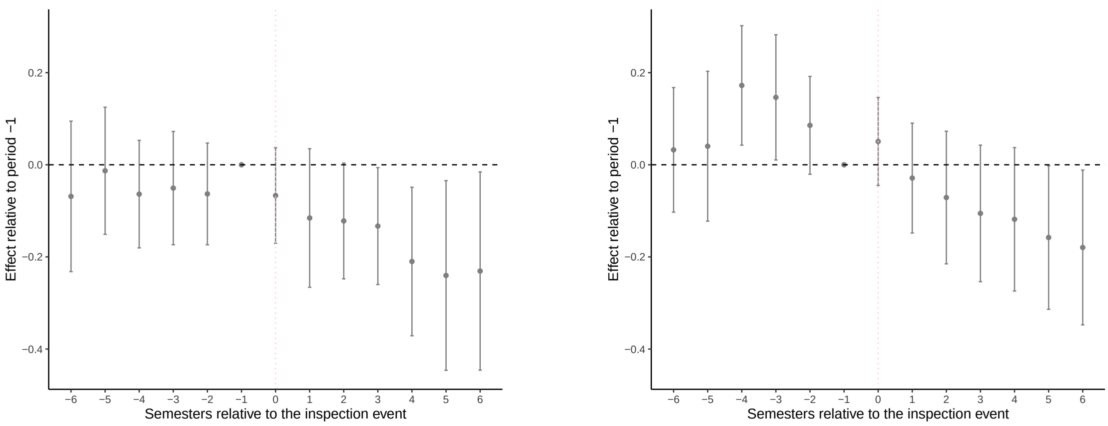


Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.24: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of separation)

(a) Establishments that received one notification

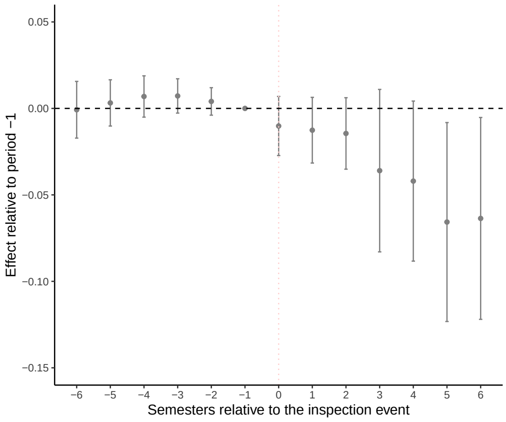
(b) Establishments that received more than one notification



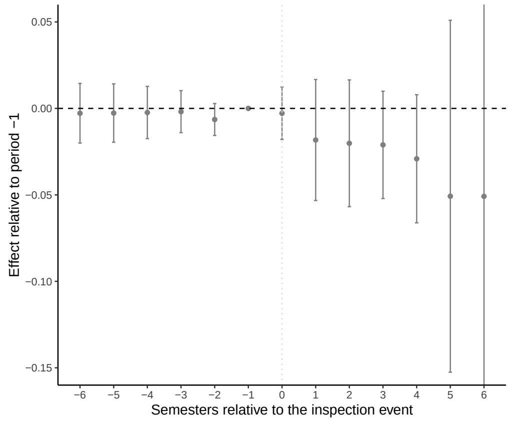
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.25: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(wages)

(a) Establishments that received one notification



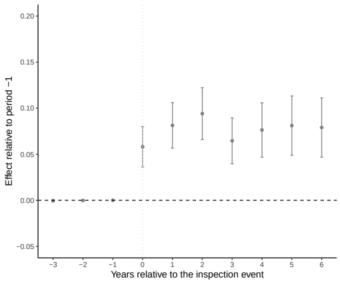
(b) Establishments that received more than one notification



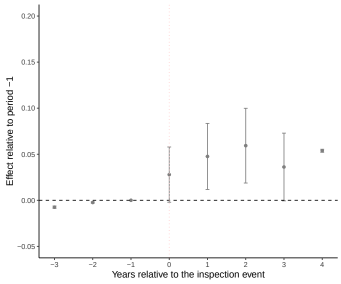
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.26: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Exit

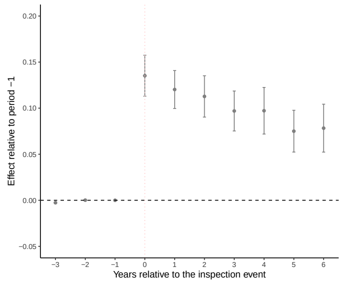
(a) Health and safety



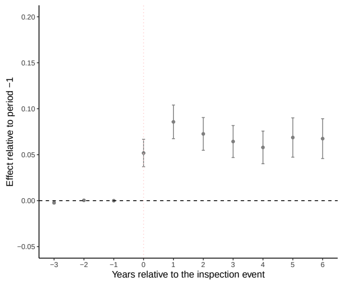
(b) Informal worker



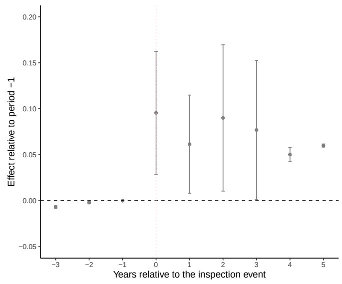
(c) Remuneration



(d) Working time



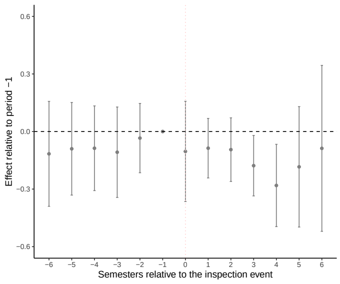
(e) Contributions



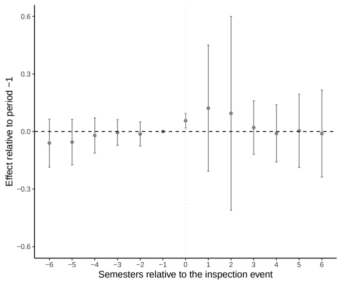
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.27: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of employees)

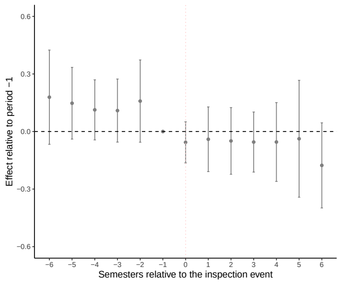
(a) Health and safety



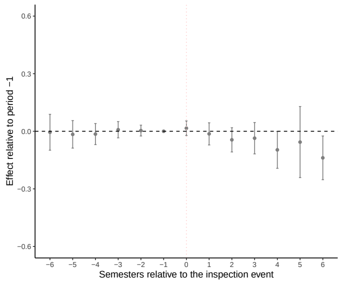
(b) Informal worker



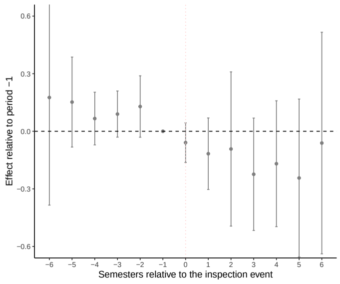
(c) Remuneration



(d) Working time



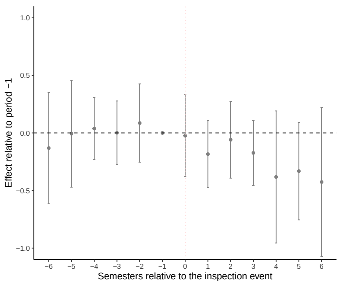
(e) Contributions



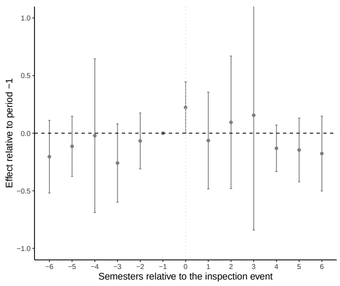
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.28: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of hirings)

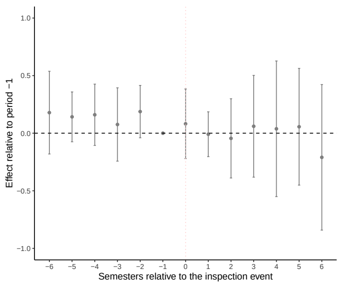
(a) Health and safety



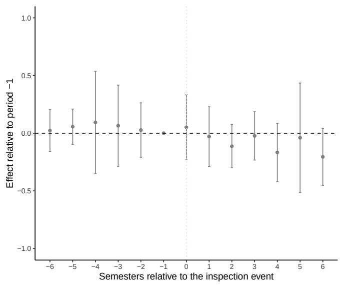
(b) Informal worker



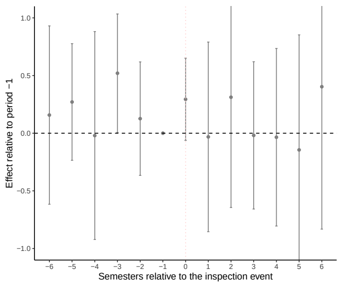
(c) Remuneration



(d) Working time



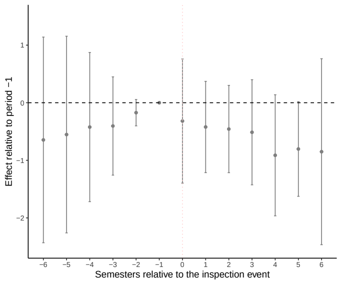
(e) Contributions



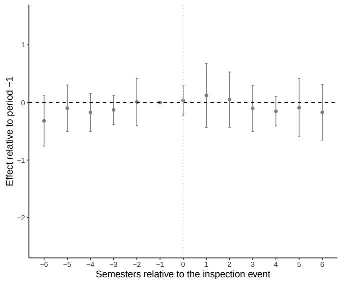
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.29: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of separation)

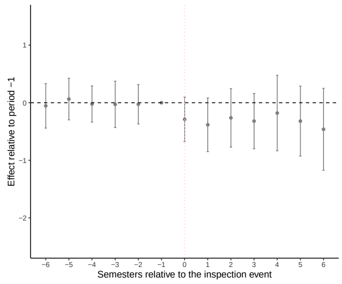
(a) Health and safety



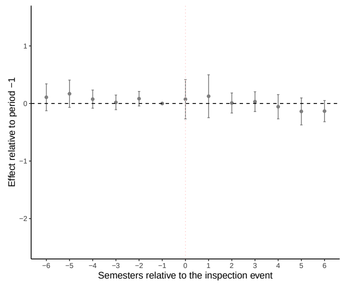
(b) Informal worker



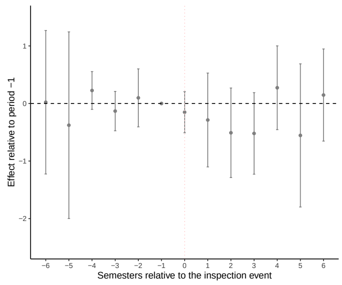
(c) Remuneration



(d) Working time



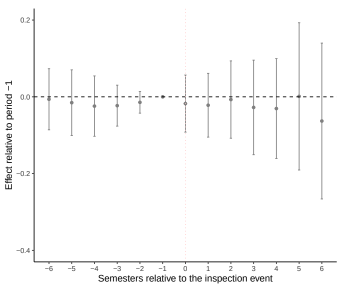
(e) Contributions



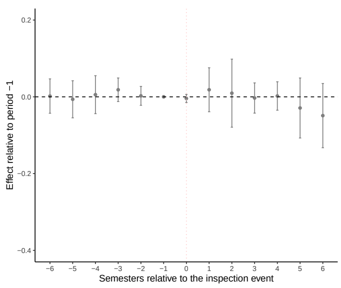
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.30: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(wages)

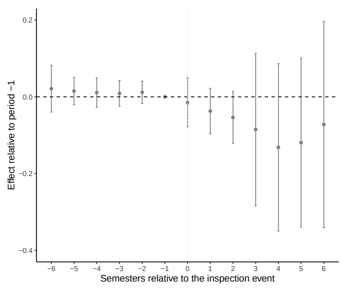
(a) Health and safety



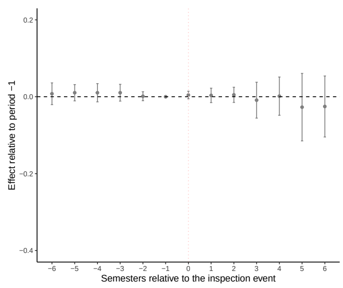
(b) Informal worker



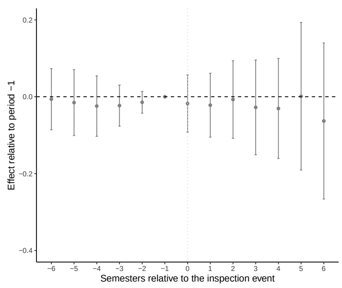
(c) Remuneration



(d) Working time



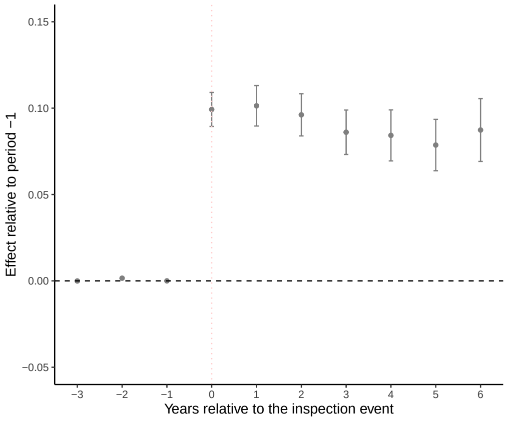
(e) Contributions



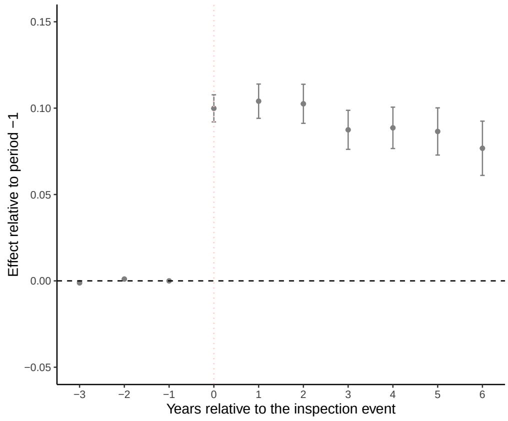
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.31: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Exit

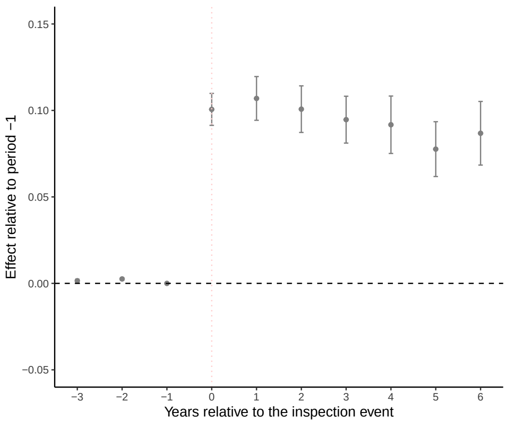
(a) Amount of fine - less than R$ 3,000



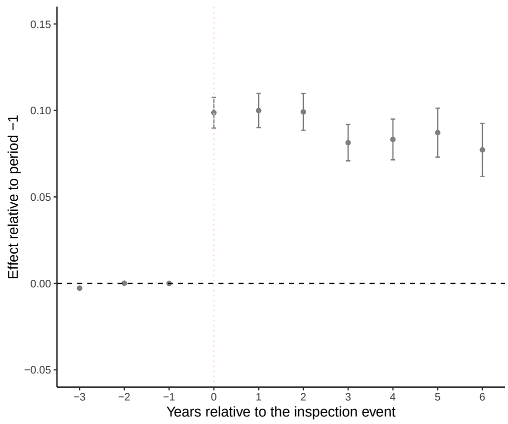
(b) Amount of fine - more than R$ 3,000



(c) Amount of fine per notification - less than R$ 1,600



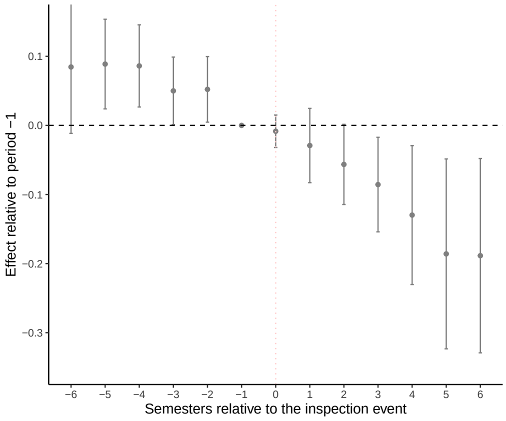
(d) Amount of fine per notification - more than R$ 1,600



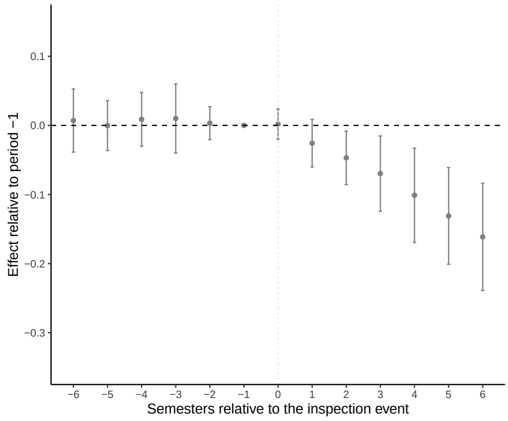
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.32: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of employees)

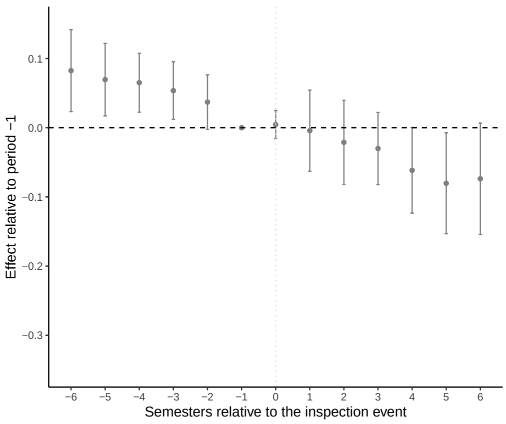
(a) Amount of fine - less than R$ 3,000



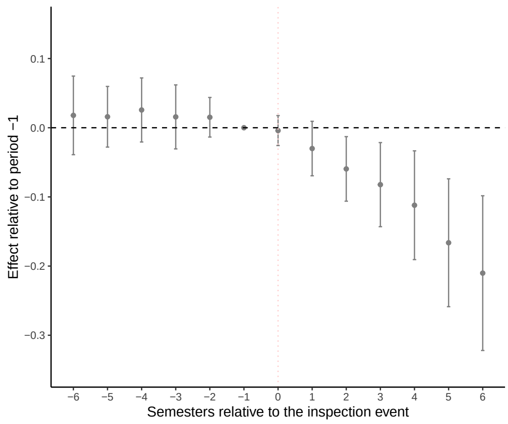
(b) Amount of fine - more than R$ 3,000



(c) Amount of fine per notification - less than R$ 1,600



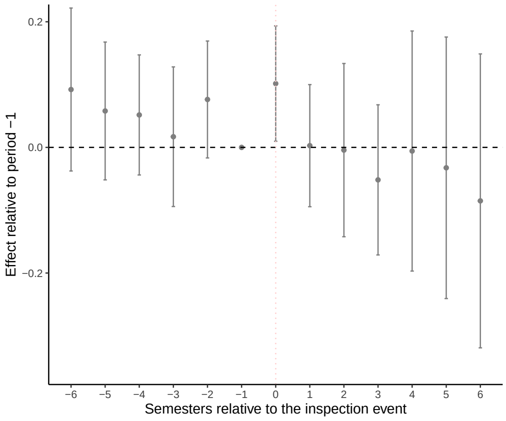
(d) Amount of fine per notification - more than R$ 1,600



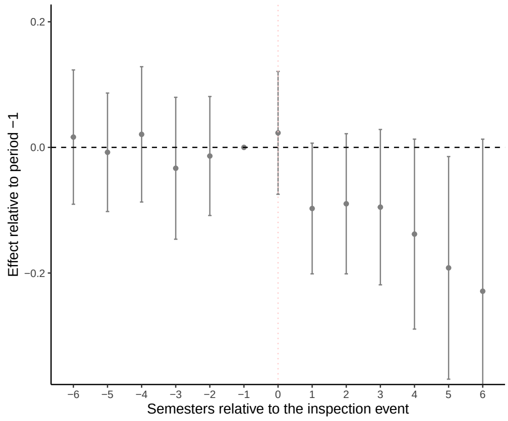
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.33: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of hirings)

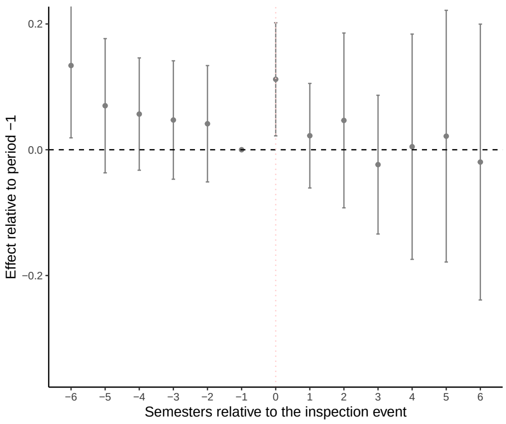
(a) Amount of fine - less than R$ 3,000



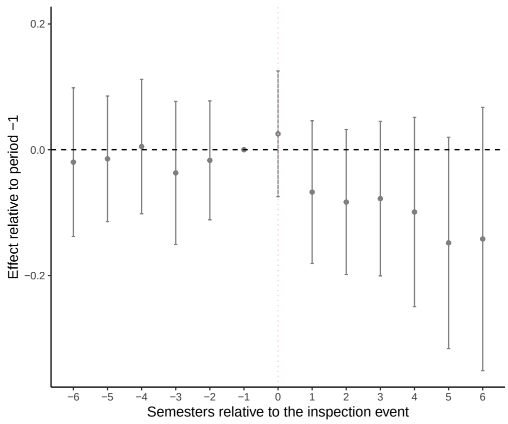
(b) Amount of fine - more than R$ 3,000



(c) Amount of fine per notification - less than R$ 1,600



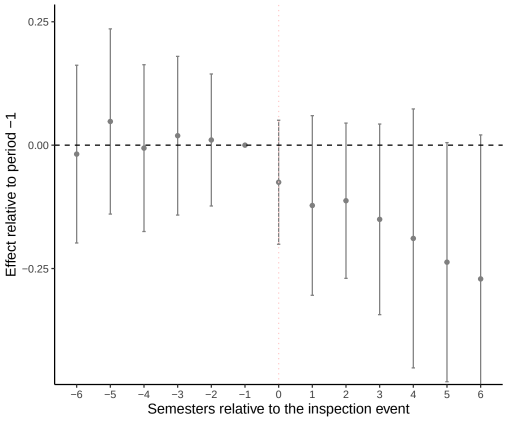
(d) Amount of fine per notification - more than R$ 1,600



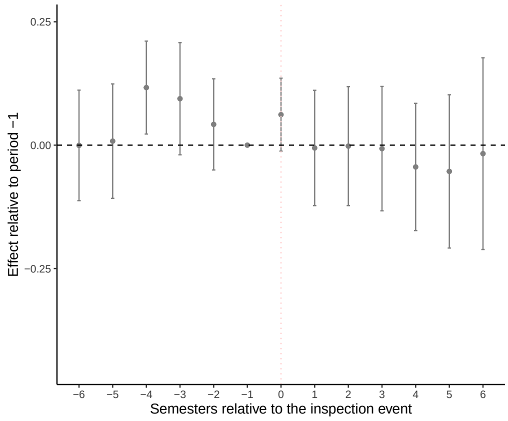
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.34: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(number of separation)

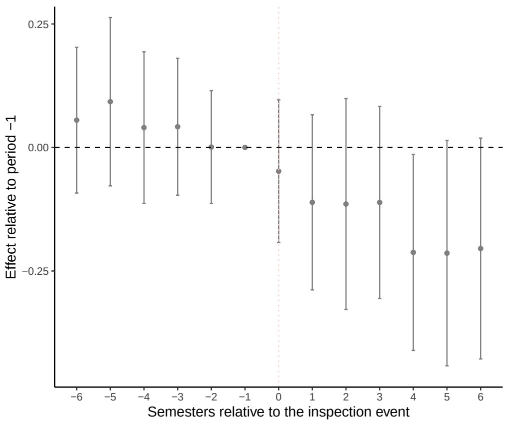
(a) Amount of fine - less than R$ 3,000



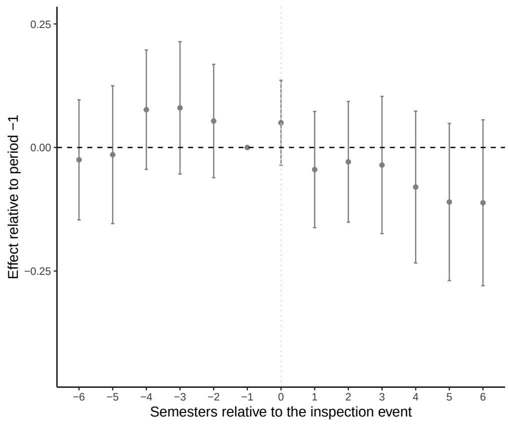
(b) Amount of fine - more than R$ 3,000



(c) Amount of fine per notification - less than R$ 1,600



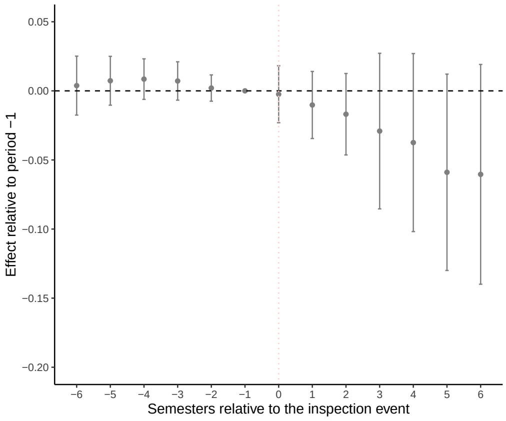
(d) Amount of fine per notification - more than R$ 1,600



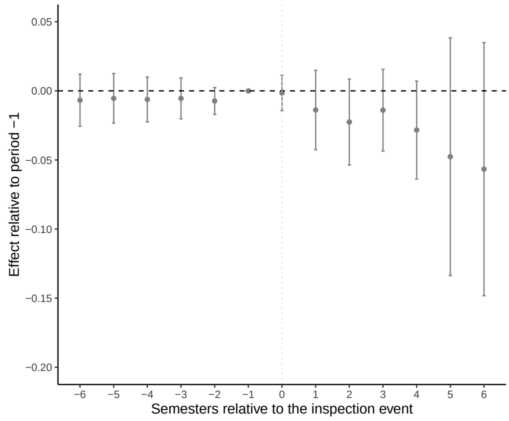
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure 2.35: Potential Mechanisms: The effects of inspection on Establishments' outcomes - Ln(wages)

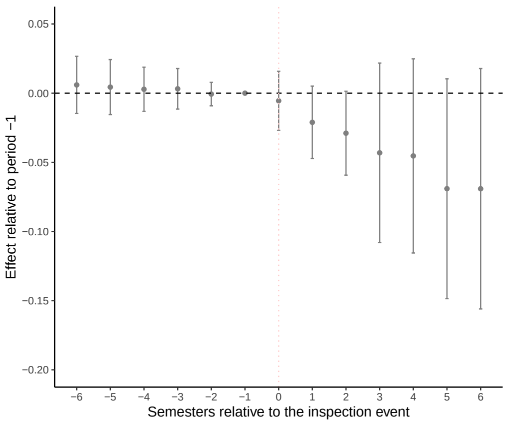
(a) Amount of fine - less than R$ 3,000



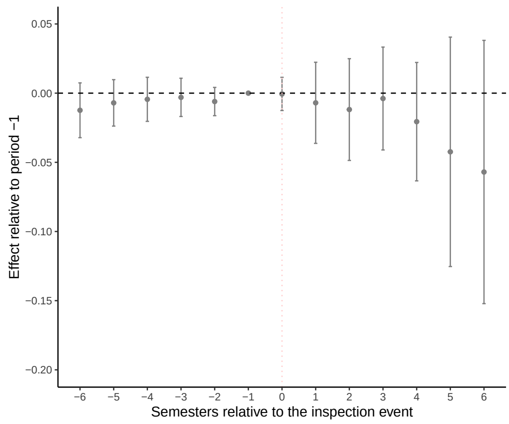
(b) Amount of fine - more than R$ 3,000



(c) Amount of fine per notification - less than R$ 1,600



(d) Amount of fine per notification - more than R$ 1,600



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS data. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

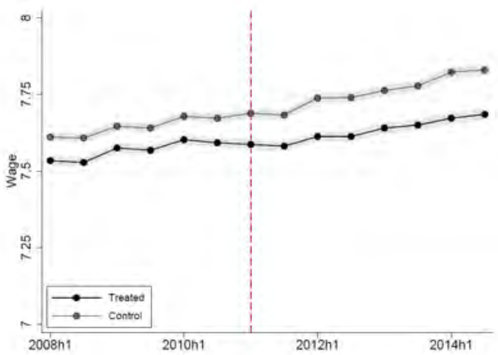
Table 2.13: Descriptive Statistics - Worker Sample

	Treated	Control
Earnings	3,065	3,199
	(4,338)	(4,324)
Ln Earnings	7.54	7.56
	(1.16)	(1.21)
Age	40.22	40.45
	(11.62)	(11.60)
Male	0.595	0.518
	(0.491)	(0.50)
Education		
Less than Primary	0.181	0.206
	(0.385)	(0.404)
Primary	0.199	0.174
	(0.399)	(0.379)
High School	0.405	0.402
	(0.491)	(0.49)
College	0.189	0.193
	(0.392)	(0.394)
Occupation		
Manager	0.054	0.047
	(0.226)	(0.212)
Professional	0.198	0.201
	(0.398)	(0.400)
White Collar Lower Level	0.205	0.202
	(0.404)	(0.401)
Blue Collar	0.544	0.551
	(0.498)	(0.0497)
Number of Observation	37,250	37,250

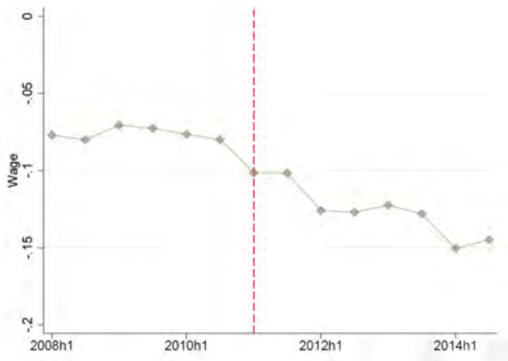
Note: The table reports descriptive statistics for workers using information from RAIS and inspection data. Column 1 presents statistics for the group of treated workers, while Column 2 reports statistics for the group of control workers. Summary statistics are computed using values from $t = -1$.

Figure 2.36: Evolution Ln Earnings - Stayers

(a) Ln Earnings (level)



(b) Ln Earnings (difference)



Note: The figure illustrates the evolution of earnings for treated workers (in black) and control workers (in gray). Panel A displays the earnings trajectory in levels, while Panel B shows the difference between treated and control workers.

3
The Determinants of Labor Inspections in Brazil

Abstract: This paper provides a descriptive analysis of the determinants of labor inspections in Brazil, focusing on how establishment characteristics, enforcement history, and geographic factors relate to inspection probabilities. Using establishment-level data from RAIS (2017 – 2018) linked to labor inspection records (2007 – 2018), we find that inspections are more frequent among larger firms and those with higher turnover. Prior enforcement actions, including past inspections and fines, are positively associated with the likelihood of reinspection. In addition, proximity to enforcement offices is correlated with higher inspection rates, consistent with logistical constraints. These results contribute to the literature on regulatory enforcement by documenting systematic patterns in inspection allocation.
KEYWORDS: Labor inspections, Firm size, Inspection Probabilities, Establishment Characteristics.

3.1 Introduction

Compliance with labor laws is essential for ensuring fair working conditions in the labor market. However, due to financial and logistical constraints, labor inspections are limited, with only a fraction of establishments being inspected each year. In Brazil, the number of labor inspectors is insufficient to cover all firms, raising a question: which establishments are more likely to be inspected?

In this study, we investigate whether larger and more complex establishments face higher inspection probabilities and how prior enforcement measures (such as prior inspections, notifications, and fines) influence the probability of being inspected. To this end, we combined RAIS data (2017-2018) with inspection records (2007-2018), and performed an exploratory analysis.

Our results indicate that, beyond firm size and internal dynamics (turnover, tenure and worker composition), historical interactions with labor inspection agencies and spatial factors play a role in shaping inspection practices.

The results of this study contribute to a growing literature that examines predictors of specific behaviors among establishments, individuals, and governments, ranging from compliance and criminal involvement to tax evasion and corruption (Duque, 2024; Colonnelli & Prem, 2022; Bergolo *et al.*, 2020; Makofske, 2019; Telle, 2009). Building on the insights provided by Almeida & Carneiro (2012), who demonstrated the significance of geographic distance in municipal-level data, our work extends this analysis by utilizing establishment-level data to map the determinants of inspection probability from the perspective of labor auditors.

Furthermore, this study offers a framework for understanding the determinants of labor inspections in Brazil. While previous studies have extensively examined the impact of inspections on establishments and local labor market, considerably less attention has been devoted to understanding the criteria that drive the selection of firms for inspection[1]. By highlighting the dynamic interaction between firm characteristics, past enforcement actions, and logistical constraints, our findings provide new insights for policymakers seeking to design more effective and adaptive strategies. This contribution is particularly relevant for emerging market contexts where regulatory resources are limited and enforcement must be strategically targeted to maximize compliance outcomes.

This paper is organized as follows. Section 3.2 discusses the data sources, provides the institutional framework of labor inspections in Brazil, and presents

[1] For example: Ronconi (2010); Almeida & Carneiro (2012); Abras *et al.* (2018); Prado *et al.* (2023); Samaniego de la Parra & Bujanda (2024).

descriptive statistics. Section 3.3 outlines the logit model. Section 3.4 examines the determinants of labor inspections. Finally, Section 3.5 offers concluding remarks.

3.2 Data and Context

3.2.1 Data Sources

We use three data sources to analyze the determinants of labor inspections in Brazil: administrative records from the Secretariat of Labor Inspection (SIT), establishment-level data from the Annual Social Information Report (RAIS), and geographic information on the distance between municipalities and labor enforcement offices, constructed by Almeida & Carneiro (2012).

The first dataset consists of labor inspection records obtained from SIT, which is responsible for enforcing labor laws and monitoring workplace compliance across Brazil. The SIT dataset includes detailed information on labor inspections, such as the inspection date, establishment identifiers (CNPJ), which infraction was committed (if any), and the amount of the fine (if any).

The second dataset, RAIS (Brazilian Ministry of Labor), contains detailed establishment-level information on all formally registered establishments and their employees. RAIS provides key variables such as the number of employees, sector of activity, hiring and separation rates, and characteristics of workers (such as education level, gender, age, among others). This dataset is widely used in labor economics due to its comprehensive coverage of the formal labor market and allows for the construction of establishment-level characteristics that may explain the likelihood of being inspected.

The third data source incorporates geographic information on the distance between municipalities and labor enforcement offices, using the data provided by Almeida & Carneiro (2012). The inclusion of this variable allows us to assess the role of logistical constraints in labor law enforcement. Given that inspections require on-site visits by labor auditors, firms located farther from enforcement offices may face a lower probability of being audited due to the costs and difficulties associated with monitoring remote areas.

To construct the final dataset, we merge inspection data with RAIS record from 2017 and 2018, using CNPJ identifier[2]. The enforcement office distances are then matched to the establishment based on the municipality code (IBGE)[3]. Our

[2]We restrict only to the private sector, defined by the legal form.

[3]Distance data were obtained through coordinate matching. To integrate these data with the

final dataset covers 3,775,383 million establishments that existed in 2018.

3.2.2 Labor Inspections in Brazil

Labor inspections are a key mechanism for enforcing labor regulations and ensuring compliance with workplace standards. In Brazil, these inspections are conducted by the Secretariat of Labor Inspection (SIT), which supervises labor law enforcement nationwide through a system of decentralized regional units. Inspections may be initiated based on worker complaints, routine audits, or targeted enforcement strategies. To ensure the unpredictability of inspections, establishments do not receive prior notification.

During an inspection, labor auditors evaluate adherence to labor laws, detect potential violations, and issue notices for infractions such as informal employment, wage irregularities, or breaches of occupational safety regulations. Once notified, establishment have ten days to contest the charges, after which the case is reviewed by an independent authority. If the violation is confirmed, fines are imposed, with the possibility of appeal. Failure to contest the decision within the stipulated timeframe results in the automatic enforcement of the penalty.

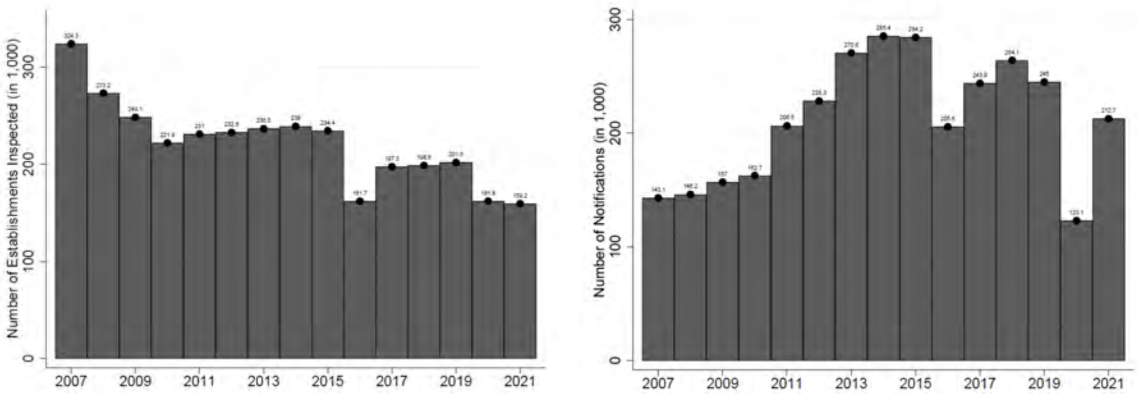
To illustrate the evolution of labor inspections over time, Figure 3.1 presents the annual number of establishments inspected and violation notifications recorded. Panel (a) shows a decreasing trend in the number of inspected establishments between 2007 and 2021. The number of inspections dropped significantly between 2007 and 2009, followed by a relatively stable period from 2011 to 2016. Between 2017 and 2019, the number of inspections increased compared to 2016 levels, suggesting a partial recovery. In 2018, for example, about 198,600 establishments across the country received a visit from labor inspectors. The lower inspection levels in 2020–2021 are potentially related to the Covid-19 pandemic.

In contrast, the number of violation notifications shows an overall upward trend, peaking in 2014, with 285,400 notifications (Panel (b), Figure 3.1). In the following years, two periods of decline followed by rapid recovery occurred in 2016 and 2020. In 2021, a total of 212,700 notifications were recorded, 25% less than in 2014 and 48% more than the first year of analysis, 2007.

RAIS database, we employed the IBGE database to link geographic coordinates with corresponding city codes. However, in this process, distances could not be determined for 1,114,842 establishments, which corresponds to 29.5% of the final dataset.

Figure 3.1: Evolution of the number of establishments inspected and notifications applied

(a) Establishment Inspected (b) Notification



Note: Panel (a) displays the number of establishments inspected per year between 2007 and 2021. Panel (b) displays the number of notifications applied per year between 2007 and 2021. The data is provided by the Secretariat of Labor Inspection (SIT).

Several factors may explain the trends observed in Figure 3.1. One of the most significant changes during the analyzed period was the sharp reduction in the number of labor inspectors, which declined from 3,123 in 2008 to approximately 2,015 in 2021 SIT (2008, 2022). This substantial decrease in enforcement capacity likely contributed to the downward trend in the number of establishments inspected annually. With fewer inspectors available, the ability to conduct on-site audits may have been constrained, leading to a natural decline in overall inspection coverage.

However, the increase in the number of violation notifications suggests that the efficiency of inspections may have improved over time. This trend could be attributed to internal strategic adjustments within the Secretariat of Labor Inspection (SIT), as well as changes in the incentive structure for labor auditors. Notably, in the end of 2016, the Brazilian government implemented a performance-based bonus system for labor inspectors - Bônus de Eficiência e Produtividade, which may have influenced enforcement priorities (Brasil, 2016, 2017, 2024)[4]. This reform aimed to enhance productivity and effectiveness in labor law enforcement, potentially leading to a shift towards more targeted inspections, where a higher proportion of visits resulted in detected violations.

One relevant aspect of labor inspection strategies in Brazil is the tendency to prioritize larger establishments. Since labor inspectors have limited resources,

[4]The total value of the bonus for labor inspectors is calculated based on a percentage of the amount collected for the FGTS (Severance Indemnity Fund for Employees), as determined by labor auditors during compliance verification procedures. Additionally, the final bonus amount is defined by the institutional efficiency index, which is measured through performance indicators and targets established in the strategic planning of the Ministry of Labor.

targeting larger establishments allows them to maximize coverage by reaching a higher number of workers per inspection. Based on the 2018 RAIS database merged with labor inspection records, we observe a significant difference in the average size of inspected and non-inspected establishments. In 2018, the average number of employees in inspected firms was 75.42, whereas non-inspected establishments had an average of 6.21 employees. Despite this difference, only 163,697 establishments underwent inspection in 2018, while 3,613,327 establishments were not inspected. As a result, labor inspections covered approximately 35% of formally employed workers that year despite reaching only 4% of formal establishments[5].

These findings suggest that firm size plays a crucial role in determining which establishments are selected for labor inspections. However, it remains an open question whether other firm-level characteristics also serve as strong predictors of inspection likelihood. In this study, we aim to assess whether additional factors, such as sector of activity, wage levels, turnover rates, and geographic location, contribute to explaining the probability of a firm being inspected. Identifying these determinants aims to enhance the understanding of the enforcement strategy.

3.2.3 Descriptive Statistics

3.2.3.1 Establishments inspected in 2018

Table 3.1 presents the descriptive statistics of the establishments inspected in 2018. Establishments inspected for the first time in that year are in Column (1) and those who had already undergone other inspections in other years are in Columns (2)-(5).

First, analyzing the firm size variable, we observe that: while the average inspected firm has approximately 75 employees, first-time inspected firms are significantly smaller, with an average workforce of 18.74 employees. In contrast, establishments that were in the fourth inspection have an average size of 171 employees. This suggests that larger firms are more likely to be repeatedly inspected.

The little variation in turnover rates suggests that there is no significant link between workforce volatility and inspection frequency. However, tenure varies considerably: employees at first-time inspected establishments have, on average, shorter tenures (27.44 months), whereas firms inspected multiple times exhibit longer employment durations. This could reflect differences in employment stability,

[5] Workers reached by inspection: $163,697*75.42 = 12,346,027$; workers not covered by inspection during the year: $3,613,327*6.21 = 22,438,760$

with more established firms facing repeated inspections, and even the age of the establishment.

Table 3.1: Descriptive Statistics - Establishments inspected in 2018

	Inspected in 2018 (1)	First inspection in 2018 (2)	Second inspection in 2018 (3)	Third inspection in 2018 (4)	Fourth inspection in 2018 (5)
Establishments' Characteristics					
Size	75.43	18.74	34.40	52.03	171.40
	(288.30)	(61.38)	(114.90)	(148.60)	(468.90)
Turnover/Separation rate	0.754	0.759	0.776	0.752	0.736
	(1.225)	(1.524)	(0.944)	(0.720)	(1.106)
Tenure (in months)	41.08	27.44	40.49	45.82	55.52
	(35.46)	(30.45)	(34.20)	(34.75)	(35.67)
Average wage	1,810	1,526	1,796	1,922	2,215
	(1,377)	(1,142)	(1,277)	(1,301)	(1,643)
% of female workers	0.3765	0.378	0.4013	0.389	0.3570
	(0.3159)	(0.355)	(0 319)	(0.299)	(0.2660)
% of workers with high school or more	0.6616	0.64	0.689	0.685	0.6636
	(0.3159)	(0.368)	(0.3057)	(0.284)	(0.2592)
% of workers disabilities	0.0072	0.0027	0.0048	0.0067	0.0139
	(0.0251)	(0.0197)	(0.0239)	(0.0251)	(0.0295)
% of manager workers	0.0474	0.0469	0.0529	0.0502	0.0439
	(0.010)	(0.1187)	(0.1072)	(0.0880)	(0.0722)
% of professional workers	0.1202	0.1028	0.1207	0.1231	0.1391
	(0.2167)	(0.2225)	(0.2254)	(0.2157)	(0.2033)
% of white-collar lower level workers	0.1862	0.1675	0.1868	0.1967	0.2039
	(0.2183)	(0.2431)	(0.2205)	(0.2057)	(0.1870)
% of blue-collar workers	0.5954	0.5707	0.6171	0.6169	0.6045
	(0.3365)	(0.3833)	(0.3312)	(0.3090)	(0.2848)
Age					
- Less than 1 year	0.0338	0.0834	0.0037	0.0002	0.0036
	(0.181)	(0.277)	(0.0611)	(0.0484)	(0.0601)
- 1-3 years	0.142	0.301	0.114	0.0359	0.0089
	(0.349)	(0.459)	(0.318)	(0.186)	(0.0943)
- 4-5 years	0.103	0.147	0.138	0.110	0.0300
	(0.304)	(0.355)	(0.345)	(0.313)	(0.171)
- 6-9 years	0.192	0.196	0.245	0.240	0.143
	(0.394)	(0.397)	(0.430)	(0.427)	(0.350)
- 10 or more years	0.529	0.272	0.499	0.611	0.814
	(0.499)	(0.445)	(0.500)	(0.488)	(0.389)
Activity					
- Industry	0.162	0.116	0.138	0.167	0.225
	(0.386)	(0.320)	(0.345)	(0.373)	(0.418)
- Construction	0.0653	0.0542	0.0517	0.0583	0.0882
	(0.247)	(0.226)	(0.221)	(0.234)	(0.284)
- Commerce	0.411	0.433	0.438	0.433	0.364
	(0.492)	(0.495)	(0.496)	(0.496)	(0.481)
- Services	0.362	0.397	0.372	0.342	0.322
	(0.481)	(0.489)	(0.483)	(0.474)	(0.467)
Region					
- North	0.0517	0.0454	0.0497	0.0529	0.0595
	(0.221)	(0.208)	(0.217)	(0.224)	(0.237)
- Northeast	0.232	0.223	0.235	0.239	0.238
	(0.422)	(0.416)	(0.424)	(0.426)	(0.426)
- Southeast	0.439	0.435	0.447	0.434	0.441
	(0.496)	(0.496)	(0.497)	(0.496)	(0.497)
- South	0.181	0.179	0.176	0.186	0.184
	(0.385)	(0.383)	(0.381)	(0.389)	(0.388)
- Central West	0.118	0.121	0.0917	0.0885	0.0769
	(0.295)	(0.322)	(0.289)	(0.284)	(0.266)
Distance to the nearest enforcement office	0.966	1.049	1.000	0.937	0.859
	(1.780)	(1.772)	(1.845)	(1.785)	(1.744)
Inspections History					
Inspected before 2018	0.621	-	1	1	1
	(0.485)	(-)	(0)	(0)	(0)
# inspections before 2018	2.27	-	1	2	5.669
	(2.826)	(-)	(0)	(0)	(2.423)
Distance between inspections (in years)	4.431	-	4.276	6.307	8.991
	(4.384)	(-)	(3.168)	(2.976)	(2.306)
Notified before 2018	0.364	-	0.328	0.50	0.759
	(0.481)	(-)	(0.469)	(0.50)	(0.428)
# notifications before 2018	3.167	-	0.944	1.919	8.472
	(10.74)	(-)	(2.582)	(3.925)	(17.30)
Firm inspected before 2018	0.677	0.149	1	1	1
	(0.467)	(0.356)	(0)	(0)	(0)
Fined before 2018	0.344	-	0.305	0.461	0.722
	(0.475)	(-)	(0.460)	(0.498)	(0.448)
Value of fines	75,791	-	11,465	17,604	103,420
	(1,006,529)	(-)	(154,911)	(215,496)	(1,204,639)
Number of Observations	163,697	62,022	29,604	18,321	53,750

Note: This table reports descriptive statistics for establishment using RAIS and SIT data. The variables are: establishments size in December 2017, turnover rate in 2017 ($\frac{\text{hiring+separation}}{2\times\text{employment}}$), tenure in 2017 (average employment time in months), average wage in 2017 (expressed in Brazilian *reais*), share of female workers in December 2017, share of workers with high school or more in December 2017, share of workers with disabilities in December 2017, share of menager workers in December 2017, share of professional workers in December 2017, share of white-collar lower level workers in December 2017, share of blue collar workers in December 2017, indicator variables for whether the establishment has up to 1 year of age in December 2017, 1-3 years in December 2017, 4-5 years in December 2017 and 10 or more years of age in December 2017, indicator variables for economic sector the establishment belong to (industry, construction, commerce or services), indicator variables for the region where the establishment is located (North, Northeast, Southeast, South and Central West), the distance variable from the municipality to the nearest enforcement office (measured in hours), indicator variable whether the establishments was inspected between 2007 and 2017, number of inspections the establishment received between 2007 and 2017, distance between inspections (in years), number of notifications the establishment received between 2007 and 2017, indicator variable whether the firm was inspected between 2007 and 2017, indicator variable whether the establishment was fined between 2007 and 2017, and the total value of fines received.

Regarding workforce composition, the share of female workers and high-school workers remains stable across all groups. Additionally, establishments inspected more frequently tend to employ a higher proportion of managerial and professional workers, whereas first-time inspected firms rely more heavily on blue-collar labor.

As expected, the age distribution of inspected firms shows that older establishments are more likely to undergo repeated inspections. While 52.9% of all inspected firms have been operating for ten or more years, this share rises from 27.2% among first-time inspected firms to 81.4% among those inspected four or more times. In contrast, firms less than a year old represent 8.34% of first-time inspections but only 0.36% of those with four or more inspections.

Sectoral and regional distributions further highlight key differences. First-time inspected firms are concentrated in the commerce and services sectors, with nearly half operating in these industries. In contrast, firms inspected multiple times show a higher presence in the industrial sector. Geographically, inspection exposure does not vary substantially. The Southeast region accounts for the largest share of inspections (43%), followed by the Northeast (23%), the South (18%), the Central-West (10%), and the North (5%).

We incorporated into our database the measure constructed by Almeida & Carneiro (2012), which estimates the travel time in hours from each municipality to the nearest enforcement office. On average, inspected establishments are located approximately one hour away from the nearest office. However, as the number of prior inspections increases, the average travel time decreases. This pattern aligns with the findings of Almeida & Carneiro (2012), who argue that municipalities—and consequently, establishments—located further from enforcement offices receive fewer inspections due to higher travel costs and logistical challenges.

Examining inspection history reveals key characteristics of establishments. Among the establishments inspected in 2018, 62.1% had undergone prior inspections, while 67.7% were part of firms with at least one previously inspected unit. Even among the establishments inspected for the first time in 2018, 15% were part of firms that had already been inspected before, suggesting that enforcement efforts often extend beyond individual establishments to the broader firm level.

Considering establishments inspected in 2018, 36.4% had received prior notifications. However, this proportion varies considerably across groups. Among establishments inspected for the second time, 32.8% had prior notifications, while this figure rises to 50% for those inspected three times and 75.9% for those inspected four or more times. This pattern suggests that establishments with a history of infractions are more likely to face repeated inspections, potentially reflecting a targeted enforcement approach.

The number of notifications received before 2018 follows a similar pattern.

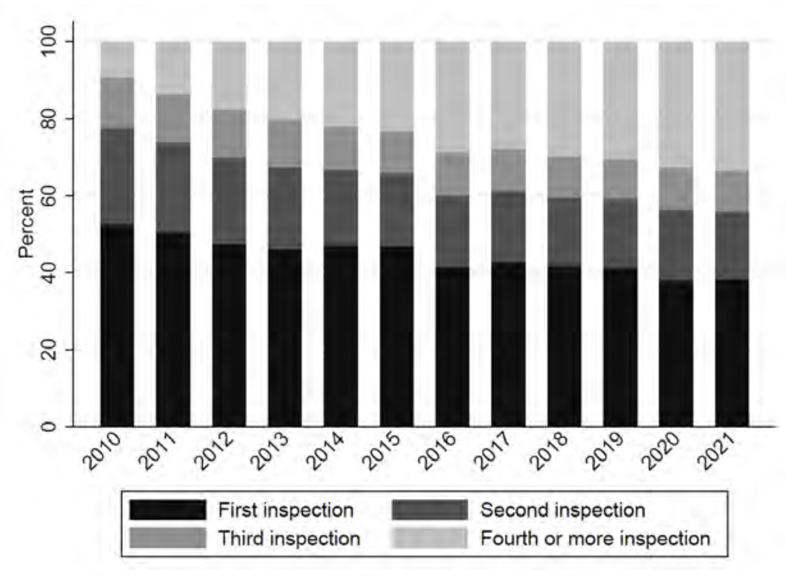
Firms inspected for the second time in 2018 had received, on average, 0.944 notifications before that year. In contrast, establishments inspected three times had received nearly twice as many notifications (1.919), while those inspected four times had accumulated an average of 8.472 notifications. The increasing number of prior notifications for repeatedly inspected establishments reinforces the idea that regulatory authorities prioritize firms with a history of noncompliance.

In terms of fines, approximately 34% of establishments inspected in 2018 had already been fined. However, this proportion increases with the number of prior inspections. Among firms inspected for the second time in 2018, 30% had previously been fined, while this share rises to 46% for those inspected for the third time and 72% for those inspected four or more times. A similar pattern is observed for the monetary value of fines. The average fine amount among establishments inspected in 2018 that had been previously inspected was 75,791 *reais*, whereas for establishments undergoing their fifth or more inspection in 2018, this average increased to 103,420 *reais*.

this descriptive analysis showed that the profile of inspected establishments varies depending on the number of previous inspections they have undergone. To complement this analysis, Figure 3.2 illustrates the yearly distribution of inspections from 2010 to 2021, categorizing establishments based on their prior inspection history[6]. This allows us to examine the pattern in the share of first, second, third, and multiple inspections over time.

[6]Since the data begins in 2007, establishments could only undergo a fourth inspection starting in 2010. For this reason, Figure 3.2 displays the period from 2010 to 2021.

Figure 3.2: Evolution of Inspection Distribution by Number of Prior Inspections



Note: The figure displays the percentage of establishments inspected in a given year, categorized by whether they were undergoing their first, second, third, or fourth or more inspection. The graph covers the period from 2010 to 2021, but inspection data since 2007 is considered. The data is provided by the Secretariat of Labor Inspection (SIT).

Examining the most recent years, we observe a stable pattern in the distribution of inspections across different groups. On average, approximately 40% of inspected establishments each year were being inspected for the first time, 19% for the second time, 10% for the third time, and 31% for the fourth time or more. The division of inspection actions suggests a structured approach in the enforcement strategy, where labor auditors balance the need to expand inspections to new establishments while maintaining monitoring of those already inspected.

3.2.3.2 Establishments not inspected in 2018

So far, the analysis has focused on the characteristics of establishments that received labor inspections in 2018. However, to better understand labor enforcement, it is also important to examine the establishments that were not inspected during this period. By comparing these two groups, we can assess whether inspected firms differ systematically from non-inspected ones.

Tables 3.2 and 3.3 presents descriptive statistics for establishments that were not inspected in 2018 (Column 1) and those that were never inspected between 2007 and 2021 (Column 2). Distinguishing between establishments inspected in previous years and those never inspected from 2007 to 2021 enables an analysis of whether certain types of establishments are systematically excluded from the inspected group.

Table 3.2: Descriptive Statistics - Establishments not inspected in 2018

	Non-Inspected in 2018 (1)	Never Inspected (2007-2018) (2)
Inspections History		
Inspected before 2018	0.170	-
	(0.375)	(-)
# inspections before 2018	0.297	-
	(0.864)	(-)
Distance between inspections (in years)	-	-
	(-)	(-)
Notified before 2018	0.0592	-
	(0.236)	(-)
# notifications before 2018	0.230	-
	(1.835)	(-)
Firm inspected before 2018	0.231	-
	(0.422)	(-)
Fined before 2018	0.0527	-
	(0.223)	(-)
Value of fines	14,338	-
	(112,495)	(-)
Number of Observations	3,613,327	3,000,453

Note: This table reports descriptive statistics for establishment using RAIS and SIT data. The variables are: number of notifications the establishment received between 2007 and 2017, indicator variable whether the firm was inspected between 2007 and 2017, indicator variable whether the establishment was fined between 2007 and 2017, and the total value of fines received.

Table 3.3: Descriptive Statistics - Establishments not inspected in 2018

	Non-Inspected in 2018 (1)	Never Inspected (2007-2018) (2)
Establishments' Characteristics		
Size	6.217	4.105
	(30.87)	(13.48)
Turnover/Separation rate	0.622	0.607
	(0.764)	(0.684)
Tenure (in months)	36.38	33.01
	(44.99)	(44.11)
Average wage	1,331	1,267
	(1,186)	(1,163)
% of female workers	0.3762	0.3654
	(0.4063)	(0.4135)
% of workers with high school or more	0.5702	0.5517
	(0.4298)	(0.4416)
% of workers disabilities	0.0015	0.00122
	(0.0237)	(0.0233)
% of manager workers	0.0422	0.3963
	(0.1428)	(0.1442)
% of professional workers	0.0941	0.0920
	(0.2422)	(0.2454)
% of white-collar lower level workers	0.1736	0.1702
	(0.3053)	(0.3130)
% of blue-collar workers	0.5228	0.5035
	(0.4360)	(0.4467)
Age		
- Less than 1 year	0.120	0.144
	(0.325)	(0.352)
- 1-3 years	0.207	0.241
	(0.405)	(0.428)
- 4-5 years	0.110	0.121
	(0.313)	(0326)
- 6-9 years	0.186	0.184
	(0.389)	(0.387)
- 10 or more years	0.377	0.310
	(0.485)	(0.462)
Activity		
- Industry	0.0922	0.0839
	(0.289)	(0.277)
- Construction	0.0397	0.0393
	(0.195)	(0.194)
- Commerce	0.427	0.419
	(0.495)	(0.493)
- Services	0.441	0.458
	(0.496)	(0.498)
Region		
- North	0.0434	0.0398
	(0.204)	(0.195)
- Northeast	0.157	0.147
	(0.363)	(0.354)
- Southeast	0.486	0.490
	(0.500)	(0.500)
- South	0.215	0.222
	(0.411)	(0.416)
- Central West	0.0997	0.101
	(0.300)	(0.301)
Distance to the nearest enforcement office	1.132	1.160
	(1.641)	(1.629)
Number of Observations	3,613,327	3,000,453

Note: This table reports descriptive statistics for establishment using RAIS and SIT data. The variables are: establishments size in December 2017, turnover rate in 2017 ($\frac{\text{hiring+separation}}{2\times\text{employment}}$), tenure in 2017 (average employment time in months), average wage in 2017 (expressed in Brazilian *reais*), share of female workers in December 2017, share of workers with high school or more in December 2017, share of workers with disabilities in December 2017, share of menager workers in December 2017, share of professional workers in December 2017, share of white-collar lower level workers in December 2017, share of blue collar workers in December 2017, indicator variables for whether the establishment has up to 1 year of age in December 2017, 1-3 years in December 2017, 4-5 years in December 2017 and 10 or more years of age in December 2017, indicator variables for economic sector the establishment belong to (industry, construction, commerce or services), indicator variables for the region where the establishment is located (North, Northeast, Southeast, South and Central West) and the distance variable from the municipality to the nearest enforcement office (measured in hours).

In terms of size, never-inspected establishments are slightly smaller, with an average of 4.1 employees, compared to 6.2 employees in establishments that were not inspected in 2018. This pattern also suggests that firm size may play a role in determining the likelihood of inspection, as larger firms may be more visible to auditors.

Turnover rates are similar across both groups, averaging around 1.13–1.15. However, tenure (the average duration of employment relationships) is slightly higher in non-inspected firms (36.3 months) compared to never-inspected firms (34.1 months). Regarding workforce composition, both groups exhibit comparable shares of female workers (approximately 32%) and employees with at least a high school education (47% in non-inspected firms versus 45% in never-inspected firms). The proportion of managerial and professional workers is also similar.

The age distribution reveals that never-inspected establishments tend to be slightly younger. While 14.4% of never-inspected establishments are less than one year old, among establishments that were not inspected in 2018 this share is 12%. Additionally, 35% of never-inspected establishments have been in operation for at least 10 years, compared to 39% for non-inspected ones.

The sectoral composition is similar across both groups. Establishments in commerce and services account for nearly half of the total in each category, while those in industry and construction are less represented. Geographically, both groups follow a similar distribution. The Southeast and Northeast regions account for the largest shares, followed by the South, Central-West, and North.

For both establishments that were not inspected in 2018 and those that were never inspected throughout the analysis period, the average travel time to the nearest enforcement office is approximately one hour and ten minutes. As shown in Table 3.1, the overall average for establishments inspected in 2018 was one hour, but for those that had been inspected four or more times previously, this average decreased to 50 minutes.

By definition, establishments in the "never-inspected" category have had no prior audits. However, among establishments that were not inspected in 2018, 17% had been inspected in previous years, indicating that some establishments experience inspections sporadically rather than regularly. Furthermore, 6.2% of these establishments had received notifications before 2018, suggesting that some had previous compliance issues but were not targeted for inspection in 2018.

In terms of fines, only 5% of establishments that were not inspected in 2018 had previously been both inspected and fined. Among these, the average fine amount was 14,338 *reais*.

The comparison indicates that never-inspected establishments tend to be smaller and slightly younger than those that had been inspected in previous years but not in 2018. Sectoral and regional distributions remain similar across both groups, suggesting that firm size and age are more relevant factors in determining inspection probability. Additionally, a subset of establishments that were not inspected in 2018 had been previously audited or notified, suggesting that labor inspections may occur sporadically rather than systematically for some establishments.

3.3 Logit Model

In this paper, we aim to identify the characteristics correlated with the probability of an establishment being inspected. To do so, we use a logit model, which accounts for the binary nature of the dependent variable. The baseline specification is defined as follows:

$$Pr(\text{Inspection}_i) = \Phi(\beta_0 + \beta_1 X_i + \beta_2 Z_i + \varepsilon_i) \tag{3-1}$$

where $Pr(\text{Inspection}_i)$ equal to 1 if the establishment i was inspected in 2018 and 0 otherwise; $\Phi(\cdot)$ is the cumulative distribution function of the logistic distribution; X_i is a vector of establishment-level characteristics, including size, wage levels, and turnover rates; Z_i is a vector with previous inspection history; and ε_i is the error term.

As a robustness check, we extend our baseline model by incorporating fixed effects for municipality, and sector. This allows us to control for unobserved heterogeneity that might systematically affect inspection probability across locations or industries. The results are presented in the next section.

The inclusion of establishment characteristics allows us to assess how structural and workforce-related factors are associated with the likelihood of being inspected. Specifically, we consider variables related to firm size, workforce composition, and location, as these factors are likely to be relevant for enforcement agencies when selecting establishments for inspections.

First, employment is used as a proxy for establishment size, as larger establishments may attract more regulatory attention due to their potential for more frequent labor law violations or their higher visibility. Similarly, turnover rate and average tenure capture workforce stability, which may indicate potential noncompliance with labor regulations. High turnover, for instance, might reflect informal employment or frequent contract terminations, both of which could raise red flags for inspectors.

We also account for the age of the establishment. Newer establishments may be less familiar with labor regulations and, therefore, more likely to be inspected, whereas older establishments may have more established compliance routines. Earnings levels are also included, as low wages can be indicative of informal employment relationships or wage underreporting, both of which may increase the likelihood of an inspection.

Workforce composition is further captured by the share of workers with at least a high school education, the share of female workers, and the share of workers with disabilities. A higher proportion of educated workers may be associated with greater awareness of labor rights, potentially affecting reporting behavior. The presence of more female workers could influence inspection likelihood if enforcement agencies prioritize sectors with higher female employment, particularly in industries with a history of compliance challenges. Likewise, firms employing a larger share of workers with disabilities may face a higher probability of inspection due to legal requirements regarding workforce inclusion.

Lastly, we include the distance to the nearest enforcement office as a key geographic constraint. Establishments located farther from enforcement agencies may be less likely to be inspected due to logistical challenges and limited inspector resources. This variable helps capture potential regional disparities in enforcement intensity.

It is important to highlight that many of the explanatory variables are potentially correlated, particularly with establishment size. Larger establishments tend to exhibit lower turnover, longer average tenure, and higher wages. Variables related to workforce composition, such as the share of educated workers, the share of female workers, and the share of workers with disabilities, are also correlated with firm size, as larger firms are more likely to adopt structured hiring practices and face stronger incentives to comply with labor regulations.

We also include variables related to inspection history to account for how prior enforcement activity relates to the probability of being inspected. The number of inspections prior to 2018 captures the extent to which a firm has already been subject to regulatory attention. Firms with a history of frequent inspections may be perceived as higher risk by enforcement agencies or, alternatively, may be deprioritized if past inspections have led to improved compliance.

A dummy variable indicating whether an establishment was ever inspected prior to 2018 distinguishes establishments with no enforcement history from those with any past interaction, allowing us to capture potential persistence in regulatory targeting.

Similarly, the number of notifications issued before 2018 captures the frequency of formal warnings received by the firm. A higher count may reflect repeated compliance issues and be associated with a greater likelihood of future inspections.

We also include a dummy variable indicating whether the establishment was fined prior to 2018, which differentiates firms that have faced financial penalties from those that were inspected but not sanctioned. Being fined may signal more serious past violations and increase the probability of re-inspection.

Together, these variables allow us to examine whether enforcement agencies prioritize firms with a history of noncompliance, and whether past penalties serve as deterrents or markers for continued regulatory attention.

3.4
The Determinants of Labor Inspections

Table 3.4 reports average marginal effects from the logit model described in Equation 3-1, estimating the determinants of the probability of being inspected. We explore a series of model specifications to assess the contribution of firm characteristics and prior enforcement history. Column 1 includes only establishment-level covariates; Column 2 includes only variables related to inspection history; Column 3 combines both. From Column 4 onward, we progressively add fixed effects. Column 5 presents our preferred specification, which includes both state and sector fixed effects. While the estimated effects are modest in magnitude, they are highly statistically significant.

Table 3.4: Average Marginal Effects from Logit Model on the Probability of Inspection

Dependent Variable:	$\mathbf{I}(Inspection_{2018})$										
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Variables											
Employment	0.0005***		0.0002***	0.0002***	0.0002***	0.0003***	0.0002***	0.0002***	0.0002***	0.0002***	0.0002***
	(0.000008)		(0.000006)	(0.000006)	(0.000007)	(0.000009)	(0.000007)	(0.000007)	(0.000007)	(0.000007)	(0.000007)
Turnover rate	0.0036***		0.0027***	0.0028***	0.0024***	0.0037***	0.0025***	0.0025***	0.0025***	0.0025***	0.0025***
	(0.0004)		(0.0004)	(0.0005)	(0.0005)	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Tenure (in months)	-0.0001***		-0.0001***	-0.0001***	-0.0001***	-0.0002***	-0.0002***	-0.0002***	-0.0001***	-0.0002***	-0.0002***
	(0.00002)		(0.00003)	(0.00003)	(0.00003)	(0.00004)	(0.000004)	(0.000003)	(0.000003)	(0.000003)	(0.000003)
Establishments' age	0.0032***		-0.0005***	-0.0001***	-0.0001***	-0.00007	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0,000003)		(0.00003)	(0.00003)	(0.00003)	(0.00004)	(0.00004)	(0.00009)	(0.00004)	(0.00004)	(0.00004)
Average earnings	0.000002***		0.000001***	0.000002***	0.000002***	0.000003***	0.000003***	0.000003***	0.000003***	0.000003***	0.000003***
	(0.0000007)		(0.0000006)	(0.0000007)	(0.0000007)	(0.0000001)	(0.00000007)	(0.00000007)	(0.00000007)	(0.00000007)	(0.00000007)
Share of workers with high school or more	0.0139***		0.0091***	0.0087***	0.0095***	0.0089***	0.0093***	0.0094***	0.0094***	0.0094***	0.0094***
	(0.0002)		(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Share of female workers	-0.0108***		-0.0114***	-0.0114***	-0.0092***	-0.0097***	-0.0089***	-0.0091***	-0.0091***	-0.0091***	-0.0091***
	(0.0002)		(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Share of workers with disabilities	0.0808***		0.0553***	0.0608***	0.0625***	0.0656***	0.0617***	0.0623***	0.0623***	0.0628***	0.0628***
	(0.0017)		(0.0023)	(0.0022)	(0.0022)	(0.0029)	(0.0023)	(0.0022)	(0.0023)	(0.0022)	(0.0022)
Distance to the nearest enforcement office						-0.0021***					
						(0.0001)					
# Inspections before 2018		0.0154***	0.0133***	0.0123***	0.0122***	0.0125***	0.0120***	0.0120***	0.0120***	0.0120***	0.0119***
		(0.00007)	(0.00009)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
I(Firm inspected)		0.0391***	0.0399***	0.0364***	0.0358***	0.0355***	0.0364***	0.0362***	0.0363***	0.0362***	0.0363***
		(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
# Notifications before 2018		0.0002***	-0.0002***	-0.0001***	-0.0001***	-0.0001***					
		(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00003)					
I(Establishment fined before 2018)		0.0143***	0.0128***	0.0123***	0.0118***	0.0119***	0.0140***	0.0119***	0.0125***	0.0119***	0.0115***
		(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
I(Establishment notified for informality before 2018)							-0.0110***				
							(0.0005)				
I(Establishment notified for health and safety before 2018)								-0.0026***			
								(0.0006)			
I(Establishment notified for contributions before 2018)									-0.0089***		
									(0.0007)		
I(Establishment notified for remuneration before 2018)										-0.0020***	
										(0.0006)	
I(Establishment notified for working time before 2018)											0.0007
											(0.0005)
Fixed-Effects											
State	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics											
Observations	3,775,383	3,775,383	3,775,383	3,775,383	3,334,900	2,220,058	3,334,900	3,334,900	3,334,900	3,334,900	3,334,900
Adjusted R^2	0.1068	0.1715	0.1894	0.2020	0.2031	0.2046	0.2034	0.2030	0.2031	0.2030	0.2030

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the marginal effects of different specifications using Equation 3-1. Robust standard-errors in parentheses. The dependent variable equals one if the establishment was inspected in 2018 and zero otherwise.

Column 5 indicates that each additional worker is associated with an increase of approximately 0.02 percentage points in the probability of inspection. In practical terms, an increase of 50 workers corresponds to a 1 percentage point higher likelihood of being inspected. This pattern suggests that larger establishments, due to greater visibility, are more likely to be targeted by enforcement agencies. This relationship is well documented in the literature (Cardoso & Lage, 2007; Almeida & Carneiro, 2012) and likely reflects enforcement strategies aimed at maximizing coverage. As discussed in Section 3.2.2, inspecting larger establishments allows auditors to reach a greater number of workers per visit.

The turnover rate is positively associated with the probability of inspection. A one percentage point increase in the turnover rate corresponds to an increase of approximately 0.24 percentage point in the likelihood of being inspected. This result suggests that establishments with higher turnover may be perceived as less stable and at greater risk of noncompliance with labor regulations. High turnover may also indicate a larger number of former or dissatisfied employees, who are more likely to file formal complaints or initiate legal action, potentially increasing the chance of regulatory action.

In contrast, average employment tenure, measured as the mean duration of worker employment in months, is negatively associated with the probability of inspection. This finding is consistent with theoretical expectations: longer tenure may reflect a more stable workforce and stronger internal compliance practices, reducing the likelihood of regulatory intervention.

An unexpected result is the negative relationship between establishment age and the probability of inspection. While older firms might be expected to face more inspections due to longer market presence and potential accumulated risk (as seen in Column 1), the estimates suggest that they are, conditional on the other characteristics, less likely to be inspected. One possible explanation is that older firms have already been subject to multiple inspections, building a compliance history that reduces the urgency for further regulatory action. This dynamic may be partially captured by the variable measuring the number of inspections prior to 2018, which likely absorbs part of the effect otherwise attributed to firm age.

Higher average wages are associated with a modest increase in the probability of inspection. Although the effect is small, this result may reflect the fact that higher wages tend to be observed in larger or more established firms, which are more likely to attract regulatory attention due to their size and market visibility.

Among the workforce composition variables, a one percentage point increase in the share of workers with at least a high school education is associated with a 0.95 percentage point increase in the probability of inspection. This pattern may reflect the fact that more educated workforces are concentrated in sectors with greater visibility. In contrast, a one percentage point increase in the share of female workers is associated with a 0.92 percentage point decrease in inspection probability, potentially reflecting industry composition or reputational factors that reduce the perceived risk of noncompliance.

A one percentage point increase in the share of workers with disabilities is associated with a substantial 6 percentage point increase in the probability of inspection. This robust association suggests that the presence of vulnerable worker groups serves as a salient signal for labor inspectors, prompting increased monitoring. In the Brazilian context, this finding is particularly relevant given the Quota Law, which requires that establishments with more than 100 employees allocate at least 2 percent of positions to persons with disabilities (Berlinski & Gagete-Miranda, 2024). Larger firms are more likely to be subject to this regulation and may therefore face more frequent inspections to verify compliance. The result likely reflects targeted enforcement efforts aimed at upholding social inclusion policies.

The analysis of inspection history variables reveals a strong and consistent association with subsequent regulatory actions. Each additional prior inspection is associated with a 1.23 percentage point increase in the probability of future inspections. This finding suggests that labor enforcement may give greater attention to firms with a documented inspection history, potentially viewing repeated interactions as indicators of ongoing compliance concerns or elevated regulatory risk. As shown in Section 3.2.3, approximately 60% of inspections conducted in 2018 occurred in establishments that had already been inspected at least once in the previous 11 years.

Firms that have experienced at least one prior inspection exhibit, on average, a 3.6 percentage point higher probability of being inspected again. For clarity, "firms" here refers to an aggregation of establishments, which may include one or more individual units. This cumulative effect highlights the role of past enforcement at the firm level in reinforcing regulatory targeting. Once a firm enters the enforcement system, the likelihood that one of its establishments will be selected for inspection increases.

While the number of past notifications is negatively associated with the probability of reinspection, establishments that have been fined in the past exhibit a 1.23 percentage point higher likelihood of inspection. Fines, as tangible and punitive sanctions, appear to prompt a stronger regulatory response. They likely signal more serious or confirmed violations, leading regulators to monitor these firms more closely in subsequent periods.

In the alternative specifications represented by Columns 6 to 11, we tested the addition of explanatory variables. The inclusion of variables does not significantly alter the effects of the other determinants, which remain consistent across specifications.

Column 6 introduces distance to the nearest enforcement office as an additional explanatory variable. The results show that establishments located farther from these offices are less likely to be inspected. This finding is consistent with Almeida & Carneiro (2012), who emphasize the role of logistical constraints in shaping regulatory activity. Proximity to enforcement infrastructure facilitates more frequent inspections, while remote establishments are less exposed to enforcement activity.

Columns 7–11 refine the analysis by substituting the cumulative number of notifications issued before 2018 with indicator variables that identify whether an establishment has been notified for specific irregularities. The results reveal that establishments previously notified for issues related to informality, worker health and safety, contributions, and remuneration are slightly less likely to be inspected (Columns 7, 8, 9, and 10). In contrast, establishments notified for irregularities in working hours do not display a statistically significant difference in inspection probability (Column 11). These findings suggest that the type of past infraction plays a limited role in predicting future inspections.

Our findings contribute to the literature in some ways. First, by quantifying how firm characteristics such as workforce size, turnover, and wage levels relate to inspection probabilities, we provide empirical evidence that larger and more complex firms are more likely to be inspected. Second, the differentiated effects of workforce composition, particularly the strong association with the share of workers with disabilities, highlight how labor market demographics shape enforcement strategies.

Third, our analysis highlights the importance of historical enforcement actions. The distinct effects of prior inspections, notifications, and fines indicate that not all enforcement measures carry the same weight in shaping future regulatory behavior. This finding reinforces the value of incorporating dynamic enforcement history into models of labor regulation. Finally, the inclusion of spatial factors, such as the distance to the nearest enforcement office, underscores the role of logistical constraints in shaping inspection patterns.

Collectively, these findings enrich the literature by demonstrating the interplay between establishment characteristics, historical regulatory interactions, and geographic considerations. They offer new insights into the targeting mechanisms of regulatory agencies. These contributions provide a foundation for future research aimed at refining regulatory policies and improving compliance outcomes.

3.5 Final Considerations

This paper examines the determinants of labor inspections in Brazil, emphasizing how firm characteristics, enforcement history, and spatial factors shape inspection probabilities.

The findings indicate that inspections are not randomly assigned. Rather, they follow a pattern influenced by both establishments' characteristics and logistical constraints. Establishments previously inspected or penalized face a higher likelihood of reinspection, pointing to a persistent focus on firms with known compliance risks. Geographic proximity to enforcement offices also matters, underscoring the influence of operational limitations on inspection allocation.

These results have some policy implications. In contexts of limited regulatory capacity, improving the targeting of inspections may enhance effectiveness without requiring substantial resource expansion. Incorporating predictive models to identify high-risk establishments, for example, could support more efficient allocation and reinforce compliance incentives. By documenting the correlates of inspection selection, this study provides a foundation for more strategic and evidence-based enforcement approaches aimed at improving labor compliance.

Bibliography

Abras, Ana, Almeida, Rita K, Carneiro, Pedro, & Corseuil, Carlos Henrique L. 2018. Enforcement of labor regulations and job flows: evidence from Brazilian cities. *IZA Journal of Development and Migration*, **8**(1), 24.

Almeida, Eloiza, & Narita, Renata. 2024. *A diferença de gênero nas perdas salariais após a saída do emprego formal no Brasil.* Ph.D. thesis, USP.

Almeida, Rita, & Carneiro, Pedro. 2009. Enforcement of labor regulation and firm size. *Journal of comparative Economics*, **37**(1), 28–46.

Almeida, Rita, & Carneiro, Pedro. 2012. Enforcement of labor regulation and informality. *American Economic Journal: Applied Economics*, **4**(3), 64–89.

Amorim, Guilherme, GC Britto, Diogo, Fonseca, Alexandre de Andrade, & Sampaio, Breno. 2023. Job loss, unemployment insurance and health: Evidence from brazil. *BAFFI CAREFIN Centre Research Paper*.

Ashenfelter, Orley, & Smith, Robert S. 1979. Compliance with the minimum wage law. *Journal of Political Economy*, **87**(2), 333–350.

Becker, Gary S. 1975. *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education.* Chicago, IL: University of Chicago Press.

Bergolo, Marcelo L, Leites, Martin, Perez-Truglia, Ricardo, & Strehl, Matias. 2020. *What makes a tax evader?* Tech. rept. National Bureau of Economic Research.

Berlinski, Samuel G, & Gagete-Miranda, Jessica. 2024. *Enforcement spillovers under different networks: The case of quotas for persons with disabilities in Brazil.* Tech. rept. IDB Working Paper Series.

Bertheau, Antoine, Acabbi, Edoardo Maria, Barceló, Cristina, Gulyas, Andreas, Lombardi, Stefano, & Saggio, Raffaele. 2023. The unequal consequences of job loss across countries. *American Economic Review: Insights*, **5**(3), 393–408.

Bertrand, Marianne, Duflo, Esther, & Mullainathan, Sendhil. 2004. How much should we trust differences-in-differences estimates? *The Quarterly journal of economics*, **119**(1), 249–275.

Besley, Timothy, & Burgess, Robin. 2004. Can labor regulation hinder economic performance? Evidence from India. *The Quarterly journal of economics*, **119**(1), 91–134.

Bhalotra, Sonia R, GC Britto, Diogo, Pinotti, Paolo, & Sampaio, Breno. 2021. Job displacement, unemployment benefits and domestic violence. *Working Paper*.

Blau, Francine, & Kahn, Lawrence M. 2013. Female labor supply: Why is the United States falling behind? *American Economic Review*, **103**(3), 251–256.

Blau, Francine D, & Kahn, Lawrence M. 2017. The gender wage gap: Extent, trends, and explanations. *Journal of economic literature*, **55**(3), 789–865.

Booth, Alison L, & Van Ours, Jan C. 2013. Part-time jobs: What women want? *Journal of Population Economics*, **26**, 263–283.

BRASIL. 2002. Decreto nº 4.552, de 27 de dezembro de 2002. Aprova o Regulamento da Inspeção do Trabalho. *Diário Oficial da União*.

Brasil. 2016. Medida Provisória nº 765, de 29 de dezembro de 2016. *Diário Oficial da União*, **1**(dec), 1. Disponível em: https://www.planalto.gov.br/ccivil_03/_Ato2015-2018/2016/Mpv/mpv765.htm. Acesso em: 14 fev. 2025.

Brasil. 2017. Lei nº 13.464, de 10 de julho de 2017. *Diário Oficial da União*, **1**(jul), 1. Disponível em: https://www.planalto.gov.br/ccivil_03/_ato2015-2018/2017/lei/L13464.htm. Acesso em: 14 fev. 2025.

Brasil. 2024. Decreto nº 11.971, de 1º de abril de 2024. *Diário Oficial da União*, **1**(apr), 1. Disponível em: https://www.planalto.gov.br/ccivil_03/_Ato2024-2024/2024/Decreto/D11971.htm. Acesso em: 14 fev. 2025.

Britto, Diogo GC, Pinotti, Paolo, & Sampaio, Breno. 2022. The effect of job loss and unemployment insurance on crime in Brazil. *Econometrica*, **90**(4), 1393–1423.

Brotherhood, Luiz Mário, Da Mata, Daniel, Guner, Nezih, Kircher, Philipp, & Santos, Cezar. 2024. *Labor market regulation and informality*. Tech. rept. IDB Working Paper Series.

Callaway, Brantly, & Sant'Anna, Pedro HC. 2021. Difference-in-differences with multiple time periods. *Journal of econometrics*, **225**(2), 200–230.

Card, David, Cardoso, Ana Rute, & Kline, Patrick. 2016. Bargaining, sorting, and the gender wage gap: Quantifying the impact of firms on the relative pay of women. *The Quarterly journal of economics*, **131**(2), 633–686.

Cardoso, Adalberto, & Lage, Telma. 2005. A inspeção do trabalho no Brasil. *Dados*, **48**(3), 451–489.

Cardoso, Adalberto Moreira, & Lage, Telma. 2007. *As normas e os fatos: desenho e efetividade das instituições de regulação do mercado de trabalho no Brasil.* FGV Editora.

Colonnelli, Emanuele, & Prem, Mounu. 2022. Corruption and firms. *The Review of Economic Studies*, **89**(2), 695–732.

Corado, Natália Rodrigues. 2023. *Leavers and Stayers after Mass Layoffs: Evidence from Brazil.* Ph.D. thesis, PUC–Rio.

Cuco, Ihorana Aguilar, & Souza, Kênia Barreiro de. 2019. Informalidade no mercado de trabalho: uma abordagem da transição ocupacional no Brasil entre 2012 e 2019. *XVII ENABER, Rio de Janeiro.*

Dal-Ri, Fabiano. 2024. What Does it Take to Be a Business Owner? Evidence from Transitions from Job Loss. *Working Paper.*

Dell, Melissa, Feigenberg, Benjamin, & Teshima, Kensuke. 2019. The violent consequences of trade-induced worker displacement in mexico. *American Economic Review: Insights*, **1**(1), 43–58.

Duque, Bernardo Rennó. 2024. *Who Becomes a Criminal? Evidence From Brazil.* Ph.D. thesis, PUC-Rio.

Fields, Gary S. 2011. Labor market analysis for developing countries. *Labour economics*, **18**, S16–S22.

Foguel, Miguel Nathan, & Corseuil, Carlos Henrique. 2024. *Labor enforcement and formal employment: The effects of communication and punishment.* Tech. rept. Texto para Discussão.

Gathmann, Christina, Helm, Ines, & Schönberg, Uta. 2020. Spillover effects of mass layoffs. *Journal of the European Economic Association*, **18**(1), 427–468.

Goldin, Claudia. 2006. The quiet revolution that transformed women's employment, education, and family. *American economic review*, **96**(2), 1–21.

Goldin, Claudia. 2014. A grand gender convergence: Its last chapter. *American economic review*, **104**(4), 1091–1119.

Goldin, Claudia, Katz, Lawrence F, & Kuziemko, Ilyana. 2006. The homecoming of American college women: The reversal of the college gender gap. *Journal of Economic perspectives*, **20**(4), 133–156.

Gonzaga, Gustavo M, Menezes Filho, Naércio Aquino, & Camargo, José Márcio. 2003. Os efeitos da redução da jornada de trabalho de 48 para 44 horas semanais em 1988. *Revista Brasileira de Economia*, **57**(2), 369–400.

Heckman, James J, & Pagés, Carmen. 2004. Law and Employment: Lessons from Latin America and the Caribbean–An Introduction. *Law and employment: lessons from Latin America and the Caribbean.*

Ho, Lisa, Jalota, Suhani, & Karandikar, Anahita. 2024. *Bringing work home: Flexible arrangements as gateway jobs for women in west bengal.* Tech. rept. STEG Working Paper.

Illing, Hannah, Schmieder, Johannes, & Trenkle, Simon. 2024. The gender gap in earnings losses after job displacement. *Journal of the European Economic Association*, jvae019.

ILO, International Labour Organization. 2020. *ILOSTAT database.* Tech. rept. Available from https://ilostat.ilo.org/data/.

Ivandić, Ria, & Lassen, Anne Sophie. 2023. Gender gaps from labor market shocks. *Labour economics*, **83**, 102394.

Jacobson, Louis S, LaLonde, Robert J, & Sullivan, Daniel G. 1993. Earnings losses of displaced workers. *The American economic review*, 685–709.

Kleven, Henrik, Landais, Camille, & Søgaard, Jakob Egholt. 2019. Children and gender inequality: Evidence from Denmark. *American Economic Journal: Applied Economics*, **11**(4), 181–209.

Kunze, Astrid, & Troske, Kenneth R. 2012. Life-cycle patterns in male/female differences in job search. *Labour Economics*, **19**(2), 176–185.

Le Barbanchon, Thomas, Rathelot, Roland, & Roulet, Alexandra. 2021. Gender differences in job search: Trading off commute against wage. *The Quarterly Journal of Economics*, **136**(1), 381–426.

Lira, Isis. 2025. The Determinants of Labor Inspections in Brazil. *Unpublished manuscript.*

Makofske, Matthew Philip. 2019. Inspection regimes and regulatory compliance: How important is the element of surprise? *Economics Letters*, **177**, 30–34.

Meekes, Jordy, & Hassink, Wolter HJ. 2022. Gender differences in job flexibility: Commutes and working hours after job loss. *Journal of Urban Economics*, **129**, 103425.

MTE, Ministério do Trabalho e Emprego. 2010. *Relação Anual de Informações Sociais - RAIS*. Available at: https://bi.mte.gov.br/bgcaged/. Accessed at 11/12/2024.

OIT. 2010. *As boas práticas da inspeção do trabalho no Brasil*.

Ponczek, Vladimir, & Ulyssea, Gabriel. 2022. Enforcement of labour regulation and the labour market effects of trade: Evidence from Brazil. *The Economic Journal*, **132**(641), 361–390.

Prado, Thaline, Santos, Marcelo, & Van Doornik, Bernardus. 2023. Informality Detection and Firm Outcomes: Evidence from Brazil. *Working Paper*.

Ronconi, Lucas. 2010. Enforcement and compliance with labor regulations in Argentina. *ILR Review*, **63**(4), 719–736.

Ronconi, Lucas. 2012. Globalization, domestic institutions, and enforcement of labor law: Evidence from Latin America. *Industrial Relations: A Journal of Economy and Society*, **51**(1), 89–105.

Samaniego de la Parra, Brenda, & Bujanda, León Fernández. 2024. Increasing the cost of informal employment: Evidence from Mexico. *American Economic Journal: Applied Economics*, **16**(1), 377–411.

Schmieder, Johannes F, Von Wachter, Till, & Heining, Jörg. 2023. The costs of job displacement over the business cycle and its sources: evidence from Germany. *American Economic Review*, **113**(5), 1208–1254.

Sharma, Garima. 2023. Monopsony and gender. *Unpublished manuscript*.

SIT, Secretaria de Inspeção do Trabalho. 2008. Relátorio de Gestão - 2007. *Diário Oficial da União*.

SIT, Secretaria de Inspeção do Trabalho. 2011. Relátorio de Gestão - 2010. *Diário Oficial da União*.

SIT, Secretaria de Inspeção do Trabalho. 2017. Relátorio de Gestão - 2016. *Diário Oficial da União*.

SIT, Secretaria de Inspeção do Trabalho. 2022. Relátorio de Gestão - 2021. *Diário Oficial da União*.

Szerman, Christiane. 2023. The Employee Costs of Corporate Debarment in Public Procurement. *American Economic Journal: Applied Economics*, **15**(1), 411–441.

Szerman, Christiane. 2024. The labor market effects of disability hiring quotas. *Available at SSRN 4267622*.

Telle, Kjetil. 2009. The threat of regulatory environmental inspection: impact on plant performance. *Journal of Regulatory Economics*, **35**, 154–178.

Viollaz, Mariana. 2018. Are labour inspections effective when labour regulations vary according to the size of the firm? Evidence from Peru. *International Labour Review*, **157**(2), 213–242.

Von Wachter, Till, Song, Jae, & Manchester, Joyce. 2009. Long-term earnings losses due to mass layoffs during the 1982 recession: An analysis using US administrative data from 1974 to 2004. *unpublished paper, Columbia University*.