

When the Inspector Knocks at the Door: Effects of Labor Inspections in Brazil¹

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1 Introduction

Labor regulations cover multiple aspects of working conditions and employment relationships, from child labor and forced labor to rules on occupational health and safety, working hours, social security contributions, informality, and other aspects. Complying with this broad set of requirements can impose costs on establishments, which may then strategically decide whether, and to what extent, to comply with regulations —especially in developing countries, where compliance tends to be low. To promote systemic compliance, governments rely heavily on on-site labor inspections as a key enforcement tool. In Brazil, for instance, around 5% of formal establishments¹ are inspected each year, and over a third are cited for labor violations. The scope of these inspections reflects the diversity of the underlying regulations: in 2017, 31% of citations concerned failures to collect social security contributions, 17% involved health and safety violations, and 11% related to irregularities in employment records (SIT, 2017).

The economic effects of labor inspections on inspected establishments and incumbent workers are, however, theoretically ambiguous. While enforcement may improve worker well-being by increasing compliance with legal protections and reducing irregular practices (Besley and Burgess, 2004; Ronconi, 2010), it can also introduce rigidities that distort establishments' employment decisions (Almeida and Carneiro, 2009; Heckman and Pagés, 2004). In settings with strict labor regulation and high payroll taxes, establishments may respond to enforcement either by formalizing contracts and improving compliance, downsizing to reduce costs, or shifting to informal arrangements (Cardoso and Lage, 2007). For incumbent workers with formal contracts, inspections may lead to immediate job loss, which is known to have long-term negative consequences (Schmieder et al., 2023). Yet, in contexts with search frictions and limited information, inspections that prompt reactions from low-productivity establishments may ultimately lead to better job matches and long-term improvements for these workers (Meghir et al., 2015). The net effect of labor inspections on both establishments and workers 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' 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. Importantly, the

¹ Computed as a share of the total establishments reported in RAIS per year.

timing of inspections —relative to the occurrence of infractions —is neither determined nor known by establishments. We exploit this feature in our empirical strategy to identify the effects of inspections.

We find that inspections reduce establishment employment by 7.8% within three years. This effect is driven mainly by a 9.5% decline in hirings, while separations also fall by 5.3%, indicating that establishments “freeze” workforce adjustments rather than expand or downsize aggressively. Unlike findings for Mexico (la Parra and Bujanda, 2024), where separations increase following inspections, our results suggest that Brazilian firms respond by cutting back on new hires while also limiting exits. These effects arise both from deterrence—non-penalized firms adjusting behavior after being inspected—and from punishment, as penalties raise compliance costs and slow wage growth. At the worker level, inspections do not reduce long-term employment. Instead, they lower wage growth for those who remain in inspected firms but increase wages for those who switch jobs, largely through moves to higher-wage-premium establishments. Together, these results highlight both the costs and potential benefits of enforcement.

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 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 these data, we implement two complementary strategies to examine the effects of labor inspections at both the establishment and worker levels. First, we adopt the staggered difference-in-differences approach of Callaway and Sant’Anna (2021) to assess how establishments adjust labor outcomes —employment, hiring, separations, and wages —in response to inspections. In our setting, inspections occur at different points in time. Hence, we restrict the analysis to a fixed window around each inspection and align all events relative to the inspection period. Under this design, units not yet treated serve as a control group for those already treated, helping to isolate the effect of inspections from broader business cycle trends. As discussed earlier, the unexpected timing of inspections ensures that the establishments inspected at different moments are comparable. This is confirmed in our sample by the presence of parallel trends in outcomes during the pre-treatment period.

Second, we use a matching difference-in-differences strategy at the individual level to examine how inspections affect incumbent workers —those employed in treated

establishments at the time of inspection. In this approach, treated individuals are those employed at inspected establishments,

while matched controls are similar workers employed at non-inspected establishments. To ensure longer pre- and post-treatment periods, we focus on inspections conducted in 2010, 2011, 2012, or 2013. This strategy allows us to follow workers over time and assess whether potential job displacement following inspection led to short- and long-term consequences.

Labor inspections lead to significant changes in key establishment-level outcomes, particularly in turnover and employment. Establishments reduce employment primarily by cutting back on hiring, while separations also decrease, suggesting that firms respond by freezing workforce adjustments. We also find a modest decline in establishment-level wages. These patterns may reflect a shift in establishments' perception of the costs associated with labor and turnover. They may also indicate a tendency for establishments to slow human resource decisions following an inspection.

We further analyze the outcomes of inspections—whether establishments received a notification or a fine—and find that inspections affect establishment behavior through both deterrence and penalties. Even firms that are not penalized adjust their employment and hiring decisions, underscoring the strong role of deterrence. In other words, the inspection itself raises the perceived likelihood of future enforcement, prompting preemptive changes in labor practices to avoid potential sanctions. At the same time, penalties amplify the response—particularly with respect to wage adjustments—highlighting the additional burden imposed by direct sanctions.² Taken together, these results suggest that inspections matter not only by enforcing compliance ex-post, but also by shaping firms' expectations about future oversight.

Responses also vary by violation type. Health and safety infractions, as well as failures to pay social contributions, lead to meaningful employment reductions, though through different mechanisms. Health and safety violations often trigger regulatory stop-work orders or hiring restrictions, while unpaid contributions impose financial burdens that force establishments to cut payroll—an unintended effect of enforcement. By contrast, establishments fined for informality show small employment increases, largely explained by the formalization of previously unregistered workers rather than net job creation. Overall, most establishment-level effects appear deliberately induced by inspectors to improve working conditions, with unpaid contributions standing out as the main source of unintended adverse consequences.

So far, we have shown that inspections significantly affect establishments' size. 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

² While fines are typically small, establishments may be required to pay back payroll taxes owed, which can represent substantial costs for the establishment.

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 formal employment for workers in inspected establishments, with an estimated effect of 1% one semester after the inspection. This effect gradually declines, converging 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 employers experience wage gains relative to the control group. These gains are largely driven by transitions to higher wage-premium establishments, as captured by establishment fixed effects from an AKM estimation.

In sum, while our results show 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 effects. It remains possible that inspections have broader aggregate consequences — for example, by affecting 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.

This paper contributes to the growing body of literature on the economic trade-offs associated with stricter enforcement of labor regulations. Until very recently, this literature focused on examining firms' decisions when facing higher probabilities of enforcement. The central idea is that firms, knowing the probability of inspection, incorporate this information into their hiring, wage-setting, entry, and formalization decisions. This mechanism appears in the models proposed by Meghir et al. (2015), Ulyssea (2018), and Haanwinckel and Soares (2021). The estimation, or model fit, in these studies is partly based on Almeida and Carneiro (2012), who, using reduced-form estimation, show that increases in municipal-level enforcement lead to reductions in employment at that same level³.

Our approach differs from the previous literature by focusing on ex-post decisions —those made after an establishment is inspected—rather than ex-ante decisions based on the probability of inspection. Together with la Parra and Bujanda (2024), we provide evidence that labor inspections induce substantial changes in establishment behavior, including reductions in employment, hiring, and separations. Importantly, while la Parra and Bujanda (2024) also find declines in employment and hiring following inspections in Mexico, they report an increase in separations, in contrast with our results. These findings may be informative for future

³ Other relevant studies using regional or municipal variation include (Abrás et al., 2018; Almeida and Carneiro, 2009; Ronconi, 2012, 2010).

structural models assessing the effects of increased enforcement, as they suggest that firms' decision-making is dynamic and may warrant being modeled accordingly. Moreover, they indicate that the municipal-level results in Almeida and Carneiro (2012) could reflect a combination of two mechanisms: ex-ante responses from all establishments and ex-post adjustments from inspected ones.

We also extend the literature by examining compliance with labor legislation in a more nuanced way. Rather than adopting a binary classification that labels establishments and workers as either formal or informal, we use the richness of our data to show that establishments may evade labor regulations in multiple ways⁴. Examples include failing to pay social contributions, committing health and safety violations, engaging in forced labor, or lacking employee records. Recent work by Feinmann et al. (2025) has taken a similar approach, examining partial informality through payments under the table. Based on conversations with inspectors and business owners, we believe this perspective—where establishments partially comply with labor laws—may better reflect the decision-making environment in developing countries such as Brazil.

Finally, by showing that workers leaving inspected establishments move to higher wagepremium establishments, we add empirical evidence consistent with search frictions, as in Burdett and Mortensen (1998); Meghir et al. (2015), and others. In such settings, search frictions can lead workers to accept jobs at low-wage, and likely low-productivity establishments. Inspections may act as a push for some of these workers, prompting them to seek better opportunities. Similar mechanisms appear in studies of minimum wage increases (Engbom and Moser, 2022) and are consistent with the findings in Jäger et al. (2024), which show that workers can remain trapped in jobs under the perception that no better alternatives exist. This mechanism suggests that labor inspections could play a role in improving job matches and, in turn, overall productivity.

This paper is organized as follows. Section 2 describes the institutional framework of labor inspections in Brazil. Section 3 discusses the data sources used in the analysis, outlines the sample construction, and presents descriptive statistics. Section 4 examines the effects of inspections at the establishment level, while Section ⁵ explores the heterogeneous effects of inspections based on inspection characteristics. Section 6 extends the analysis to worker-level outcomes. Finally, Section 7 offers concluding remarks.

⁴ See also related studies on labor inspections: Ponczek and Ulyssea, 2022; Prado et al., 2023; Berlinski and Gagete-Miranda, 2024; Szerman, 2024; Foguel and Corseuil, 2024.

⁵ Also referred to as the Ministry of Labor and Employment or the Ministry of Economy, depending on the administration in place.

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 Labor⁵, 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 examination 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 taken place on a substantial scale. Between 2007 and 2017, labor inspectors conducted more than 2.6 million inspections nationwide, averaging 236,000 establishments per year, which represents approximately 5% of the number of establishments in Brazil. In about 40% of the cases each year, the visit marked the establishment's first recorded interaction with labor enforcement (Lira, 2025). These inspections resulted in 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* (the Brazilian severance savings account), or violations of health and safety standards—the establishment is issued a notification for labor code violations (Brasil, 2002)⁶. More severe infractions—such as child labor, forced labor, or situations posing an immediate risk to workers' lives—may result in immediate stop-work orders.

Upon notification, the establishment has ten days from the date of receipt to contest the charges. 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 decision⁷. For establishments with up to ten employees, as well as newly opened businesses, inspections follow the double-visit criterion, which allows

⁶ See Table B.1 in Appendix B for more details on the irregularities identified during the inspections.

⁷ *Coordenação Geral de Recursos da SIT*.

employers to correct irregularities between the first and second visit without facing immediate penalties⁸.

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, other citizens, or even anonymous sources in cases involving forced labor (Cardoso and Lage, 2005; Almeida and Carneiro, 2012). Given that the number of labor inspectors has been insufficient to meet the demand for inspections, it is likely that local agencies use a range of establishment-level and local labor market data to optimize the selection process⁹. For example, Lira (2025) shows that distance to the nearest enforcement office, establishment size, and high levels of turnover are strong predictors of labor inspections.

In the empirical analysis of this paper, we focus on establishments that were inspected only once during the period covered by our 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 is less than 5%. For entrepreneurs who have never been inspected before, the perceived or estimated probability is likely even lower. Moreover, establishments are not notified in advance of an upcoming inspection. Second, an irregularity may persist for an extended period before an inspection occurs. Several steps leading up to an inspection are beyond the establishment's control. For example, establishments have no influence over whether a worker files a complaint, when that complaint is made, 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 backlog of pending cases. This unpredictability in timing provides a valuable source of variation, which we exploit to analyze the behavior of inspected establishments relative to those not yet inspected.

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

⁸ Except in cases where an infraction involves lack of registration of workers.

⁹ In 2016, approximately 2,400 inspectors were in operation, representing a reduction of more 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. For comparison, in developing countries such as South Africa and Mexico, the corresponding figures in 2016 were 0.8 and 0.2, respectively (ILO, 2020).

3 Data and Descriptive Statistics

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), 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 establishment identifier (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 during each visit.¹⁰ We classify violations into five categories: health and safety, informality, remuneration, working time, and social contributions.

During the period of analysis, the 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 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

¹⁰ Although establishments may be cited for multiple violations, the data do not link fines to specific infractions; only the aggregate penalty amount is reported.

“double-visit” inspection rule (see Section 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, the sample contains an *unbalanced panel* of 100,260 establishments. For the analysis of employment, hiring, separations, and wages, we rely on a *balanced panel* of 50,639 establishments that were inspected only once between 2007 and 2017 and had employees in every year of that period¹¹. 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 6.

3.2 Descriptive Statistics

Table 1 presents the characteristics of inspected establishments in both the unbalanced and balanced panels. For comparison, we also include the characteristics of establishments that were not inspected during the analysis period. Columns 1 and 3 report summary statistics for the period before inspection, while Column 2 shows statistics for the first period in which non-inspected establishments appear in the data.

The descriptive statistics reveal two main takeaways. First, inspected establishments are, on average, slightly larger and exhibit higher turnover than non-inspected ones. This provides initial evidence that inspected establishments have, on average, worse outcomes: inspectors appear to receive more complaints and choose to inspect establishments with more separations (including quits and dismissals) and higher hiring rates. High turnover may signal noncompliance with labor legislation, as workers might voluntarily leave poor working environments. Appendix B Figure B.1 presents survival rates for inspected establishments starting from the inspection year, alongside rates for other establishments active in that same year. The figure shows that inspected establishments are more likely to exit in any given year.

Second, inspected and not inspected establishments display similar age profiles and sectoral compositions. Nearly 90% of establishments are more than five years old, with services, commerce, and industry being the most represented sectors, in that order. The construction sector is underrepresented, accounting for only 3% of establishments—a proportion also observed among non-inspected establishments. This likely reflects our sample restrictions, which bind more strongly for construction given their temporary nature.

¹¹ The balanced panel includes only establishments active in RAIS in all eleven years of analysis (2007 to 2017).

Table 1 provides additional details on inspections. Inspection characteristics show that most inspected firms (71%) did not receive any notifications of violations, while 29% were notified and 20% 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), suggesting that financial penalties are generally modest.

The differences in characteristics between inspected and non-inspected establishments motivate the empirical strategy of this paper, which focuses exclusively on establishments that were inspected during the analysis period. Column 3 of Table 1 presents descriptive statistics for this sample. These establishments exhibit lower turnover and lower average wages than inspected establishments in the unbalanced panel, but still display higher turnover than non-inspected ones.

Table 1: Descriptive Statistics: Establishments

	Unbalanced Panel		Balanced Panel
	Inspected Once	Non-Inspected	Inspected Once
	(1)	(2)	(3)
Establishment Characteristics:			
Number of employees	33.44 (54.83)	29.32 (66.22)	31.83 (50.96)
Number of hires	9.17 (26.29)	5.91 (17.76)	7.17 (18.95)
Number of separations	8.25 (22.92)	5.64 (16.38)	6.40 (17.18)
Ln(average wage)	7.63 (0.62)	7.65 (0.92)	6.98 (0.59)
Age:			
Up to 3 years	0.07 (0.25)	0.06 (0.24)	0.04 (0.19)
3-5 years	0.08 (0.27)	0.06 (0.24)	0.05 (0.22)
More than 5 years	0.86 (0.35)	0.88 (0.33)	0.91 (0.28)
Sector:			
Industry	0.25 (0.43)	0.23 (0.42)	0.24 (0.43)
Commerce	0.31	0.26	0.31

	(0.46)	(0.44)	(0.46)
Services	0.41 (0.49)	0.48 (0.50)	0.43 (0.50)
Construction	0.03 (0.18)	0.03 (0.16)	0.02 (0.14)
Inspection Characteristics: Inspected without notification			
	0.71 (0.45)	-	0.75 (0.44)
Inspected with notification	0.29 (0.45)	-	0.25 (0.44)
# Notifications	1.05 (3.25)	-	0.97 (3.25)
Inspected with fines	0.20 (0.40)	-	0.18 (0.39)
Fine Amount (in ₡)	1,794 (13,757)	-	1,677 (14,968)
Fine Amount per notification (in ₡)	1,831 (6,346)	-	1,949 (7,230)
Number of Establishments	100,260	188,636	50,639

Note: The table reports averages and standard deviations (in parentheses) of key variables over the establishment distribution. 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. Data: RAIS and inspection data (2007-2017).

While the event-study framework does not require establishments to have identical baseline characteristics—only parallel counterfactual outcome trends—substantial baseline differences raise concerns about selection and indicate that non-inspected establishments are not a valid comparison group. In the next section, we address this issue by introducing a staggered difference-in-differences approach that provides a more suitable comparison group for inspected establishments in the balanced panel.

4 Effects of Labor Inspections on Establishments' Outcomes

This section outlines the empirical strategy and summarizes the main results from the establishment-level analysis. We start by presenting the staggered difference-in-differences (DiD) method. Next, we examine the effects of inspections on establishments that remain

active, focusing on employment, hiring, separations, and wages. Finally, we assess the robustness of our estimates through additional tests and alternative specifications.

4.1 Staggered Difference-in-Differences Approach

In our setting, establishments are inspected at different points in time. Following Callaway and 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. The sample is restricted to establishments inspected only once and continuously holding active contracts throughout the 11-year analysis window (2007–2017). In this setup, establishments not yet inspected in a given year serve as the control group for those already inspected.

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

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

where $i \in \{1, 2, \dots, N\}$ indexes establishments, $t \in \{2007/h1, 2007/h2, 2008/h1, \dots, 2017/h2\}$ indexes semesters, and $g \in \{2008/h1, 2008/h2, \dots, 2017/h2\}$ 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 preserving a comparable pool of establishments. Its 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, noting that many establishments are eligible for inspection and that the timing of inspections is determined by workers and enforcement agencies, rather than by the establishments themselves. As a result, inspections generally come as a surprise. 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 , $\hat{ATT}(g, t)$, 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.

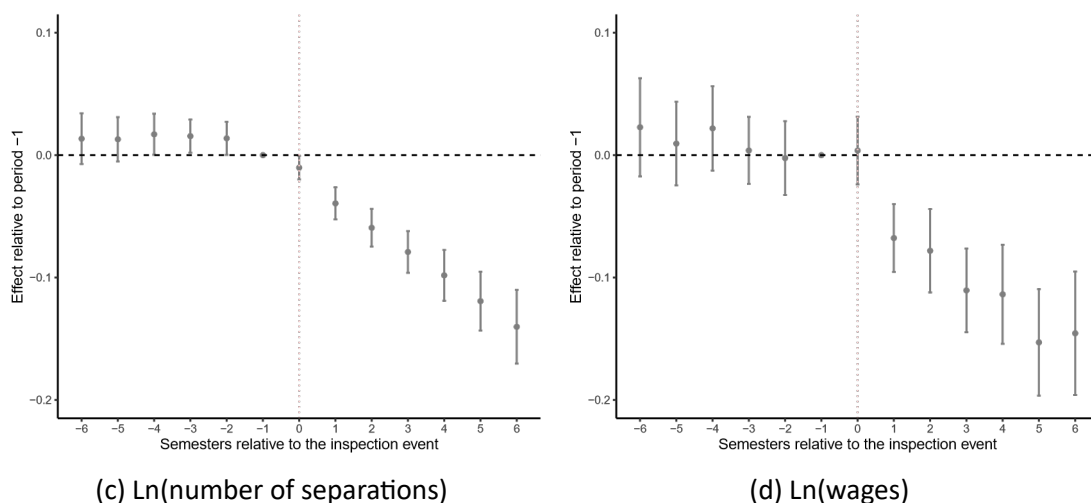
4.2 Employment, Hiring, Separation and Wages

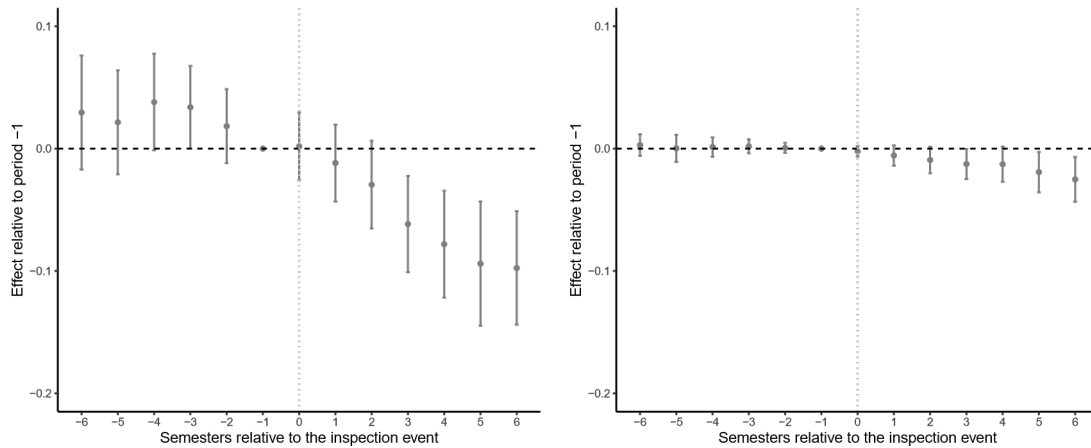
In this section, we present the effects of inspections on establishments' employment levels, number of hires, number of separations, and average wages. The estimates are based on a balanced panel of establishments that were active in all 11 years of the sample, had between 10 and 500 employees, and experienced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

Figure 1 reports the cohort-aggregated average treatment effects on the treated for each period relative to the inspection date. In each graph, the time interval between periods is one semester. The first three panels display a similar pattern: following an inspection, establishments experience a sharp and statistically significant decline in employment (Panel A), hiring (Panel B), and separations (Panel C). Panel D shows that average wages also decrease, but at a more gradual pace. Moreover, pre-inspection trends are similar for treated and control establishments, supporting the validity of the empirical analysis and the use of not-yet-treated establishments as the control group.

Table 2 summarizes these results and shows the average effect of the inspection in the six semesters following an inspection. Column 1 shows that establishments have a 7.8% reduction in employment relative to the control group. This decline stems from a large drop in hiring rather than an increase in separations and may reflect a contraction in business activity following labor inspections. In fact, the number of separations also falls. This pattern suggests that establishments may adopt a “freezing” strategy, substantially limiting operational decisions, especially those regarding workforce adjustments. Such behavior may arise from a shift in establishments' perception of a higher likelihood of future inspections, prompting them to reduce both hiring and separations to manage anticipated compliance costs.

Figure 1: 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 matched with inspection data. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The control group consists of establishments not yet treated. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

We further analyze the effects on hiring and provide evidence of increased formalization. Panel B of Figure 1 shows that the average number of hired workers does not immediately decline following an inspection. Instead, there is no discernible effect during the inspection period itself, with a reduction emerging in the subsequent period. Appendix B Figure B.2 examines this pattern in greater detail, revealing that firms increase hiring of workers who lacked an active formal labor market link in the year preceding the inspection. Notably, these workers may already have been employed by the inspected firms but without a formal contract. In such cases, formalization was likely prompted—or mandated—by the inspection process. We return to these results when analyzing the effects by type of notification.

Table 2: The Average Effects of Inspection on Establishments' Outcomes

	(1)	(2)	(3)	(4)
	Ln(N. employees)	Ln(N. hirings)	Ln(N. separations)	Ln(wages)
Post × Inspected	-0.0780*** (0.0054)	-0.0952*** (0.0113)	-0.0531*** (0.0116)	-0.0124*** (0.0040)
Pre-treatment Mean	30.11	6.28	5.83	1,333.85
Nº of Establishments	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: Ln(number of employees), Ln(number of hirings), Ln(number of separations), and Ln(wages). The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The data are semiannual. The control group consists of establishments not yet inspected. The number of establishments and the pre-treatment mean of each dependent variable (measured at t=1) are reported in the last two rows. Standard errors are clustered at the establishment level.

We investigate heterogeneous effects by establishments' characteristics, such as size, sector of activity, and age. Figure 2 illustrates the average post-treatment effect, and Appendix B Tables B.2, B.3, and B.4 present the point estimates. To ensure comparability, each estimate is obtained from a regression restricted to establishments within the same characteristic

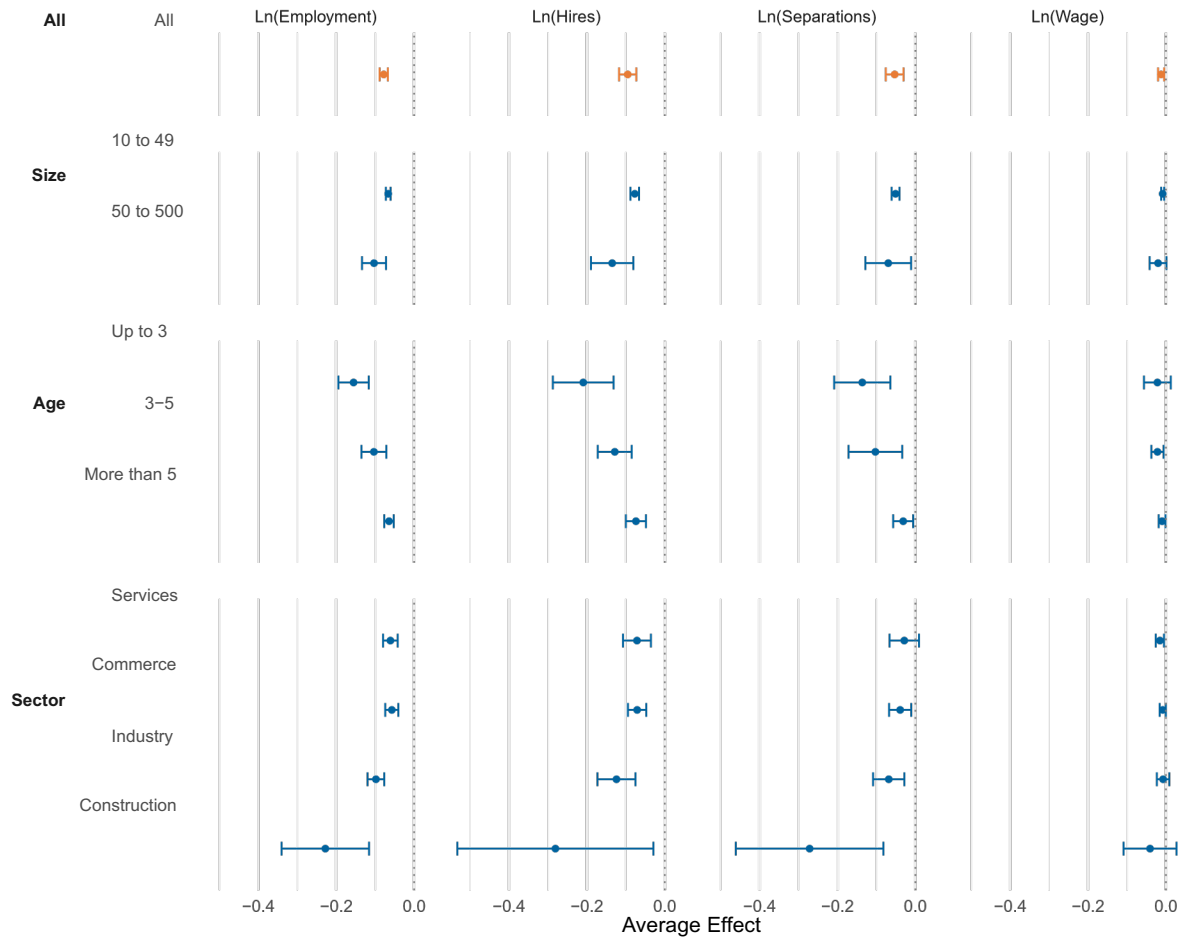
group. For example, small inspected establishments are compared with small not-yet-inspected establishments.

The first notable finding from Figure 2 is that employment and hiring effects are negative across all groups. The effects on separations and wages are also mostly negative, although for some groups they are statistically indistinguishable from zero.

Secondly, while there seems to be some heterogeneity in point estimates, we cannot reject equality across groups in most cases. For example, medium-sized establishments (with between 50 and 500 employees) have a larger reduction in employment and in hirings, with a difference statistically different at the 10% significance level (but not at 5%). Notable differences occur for young versus old establishments, with the latter having more muted responses to inspections, likely reflecting greater business stability, and between the services and commerce sectors versus the construction sector, with the latter exhibiting stronger responses. The effects in the construction sector are more likely to be driven by stop-work orders related to safety regulations¹². Nevertheless, construction firms account for a small share of our sample, which helps explain the large confidence intervals.

Figure 2: Heterogeneous Effects of Inspections on Establishments' Outcomes

¹² Due to the often temporary nature of construction projects, establishments in this sector frequently struggle to maintain adequate safety equipment, ensure proper working conditions, and raise awareness about the use of safety measures.



Note: The figure reports the aggregate effects of inspection on different outcomes: $\ln(\text{number of employees})$, $\ln(\text{number of hirings})$, $\ln(\text{number of separations})$, and $\ln(\text{wages})$. 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 5 years. The sector categories are: industry, services, construction, and commerce. The figure plots point estimates and the 95% confidence interval. Standard errors are clustered at the establishment level. The corresponding regression tables are reported in Appendix B (Tables B.2, B.3 and B.4).

The findings in this section are consistent with a growing body of literature examining how establishments respond to labor inspections. Prior research shows that inspections, by increasing compliance costs and restricting access to informal labor, can negatively affect employment and other establishment-level outcomes.

Almeida and Carneiro (2009) find that increased enforcement reduces employment (-0.47%) and hiring (-0.38%) in Brazil, using municipality-level data on inspections and labor market outcomes. Using establishment-level data, we capture the direct effects of inspections, rather than spillover effects on establishments unlikely to be directly affected by inspections, which may explain why our estimated effects are larger. Prado et al. (2023) and Brotherhood et al. (2024) use similar inspection and labor market data for Brazil, focusing on inspections specifically targeting informal labor, and find an initial increase in employment followed by consecutive declines. Likewise, la Parra and Bujanda (2024), analyzing random inspections in Mexico, find immediate declines in employment and hiring following inspections. Unlike our

results, however, they attribute the decline in employment not only to reduced hiring but also to increased separations during the inspection year.

4.3 Robustness

We conduct a series of robustness checks to confirm that our results are not driven by modeling choices or sample restrictions. We test the robustness of our main results using three alternative specifications. First, we redefine our four main outcomes using the inverse hyperbolic sine transformation. Appendix B Figure B.3 shows that the results remain largely unchanged. Second, we restrict the analysis to include establishments active in every $[-6,6]$ window around an inspection date—that is, those inspected between 2010 and 2014 for which we have data from 2007 to 2017. This approach reduces the sample size substantially, from 51,806 establishments in the balanced panel to 25,103 establishments, yet the estimated effects are very similar to our baseline results (Appendix B Figure B.4). Finally, we exclude establishments inspected between 2003 and 2007 to ensure that the 2007–2017 inspection is the first within a sufficiently long time window. The results, shown in Appendix B Figure B.5, remain consistent with our main findings.

5 Potential Mechanisms: Punishment and Deterrence

In the previous section, we showed that inspections lead establishments to reduce both employment and turnover. What drives this response to inspections? In this section, we explore two possible mechanisms: punishment and deterrence.

The first mechanism, punishment, captures how establishments react when found in violation of labor regulations. Inspected establishments that fail to meet their legal obligations may face penalties such as stop-work orders or fines, and may also be required to settle overdue contributions or payments. In such cases, inspections raise payroll-related costs. In response, establishments may downsize or reduce turnover, with the magnitude of the reaction likely depending on the type of infraction and the severity of the penalty¹³.

The second mechanism, deterrence, is a standard feature of models of regulatory compliance. In these frameworks, profit-maximizing firms choose between complying with regulations and evading labor laws, with the decision hinging on the perceived likelihood of inspection (Ashenfelter and Smith, 1979). Compliance entails meeting legal obligations, whereas evasion reduces labor costs and increases flexibility—but carries the risk of penalties

¹³ Both hiring and firing involve employment adjustment costs, so reductions along these margins may reflect efforts to lower variable labor costs.

if detected. In this context, more frequent inspections—or a higher perceived chance of being inspected—increase the expected cost of non-compliance (Viollaz, 2018).

In our setting, deterrence arises when firms update their beliefs about inspection risk after being inspected. Inspections may alter establishments' expectations regarding future oversight, thereby encouraging greater compliance. Notably, deterrence can induce behavioral changes even when no formal violations are identified.

We next examine these mechanisms by analyzing how establishments respond to key features of the inspections they experience—such as whether they were notified or fined, the amount of the fine, and the type of notification.

5.1 Responses Across Inspection Outcomes: Notified vs. Fined

We begin by analyzing establishments' responses by inspection outcome: not notified, notified, and fined. Comparing these groups allows us to assess whether the observed responses are driven primarily by penalties or by the deterrence effect of inspections. For each analysis, the sample is restricted to establishments within the same outcome category, with not-yet-inspected establishments serving as controls for each year.

Table 3 shows that establishments inspected but not notified (Panel A) experience effects almost as large as those observed among notified (Panel B) and fined (Panel C) establishments across key outcomes. Event-study estimates are reported in Appendix B Figures O.3-O.6. All inspected establishments reduce employment and hiring, including those not notified, suggesting that inspections alone can discourage hiring and limit expansion. Another possible—though unverified—explanation is corruption, which could impose costs as high as, or even higher than, official penalties. Separations respond more strongly when penalties are applied, although the difference is not statistically significant. Statistically significant wage declines are observed only among notified or fined establishments (Column 4), indicating that wage adjustments are more closely tied to penalties than to deterrence alone.

Overall, the results indicate that both deterrence and penalties shape establishments' responses to inspections. The sizable effects among inspected but non-penalized establishments highlight the strong role of deterrence, particularly in employment and hiring decisions. Meanwhile, the amplified effects among notified and fined establishments—especially for wages—underscore the additional burden of penalties. These findings suggest that inspections affect establishment behavior not only through direct enforcement but also by shaping expectations about future oversight.

Table 3: Establishments' Responses by Inspection Outcome

Outcome:	$\ln(N^o \text{ Employees})$	$\ln(N^o \text{ hirings})$	$\ln(N^o \text{ separations})$	$\ln(\text{wages})$
	(1)	(2)	(3)	(4)

Panel A: Establishments inspected without notification (N=38,162)

Post × Inspected	-0.0721*** (0.0086)	-0.0900*** (0.0176)	-0.0387** (0.0150)	-0.0057 (0.0047)
Pre-treatment Mean	30.93	7.17	6.37	1,311.09

Panel B: Establishments inspected with notification (N=12,477)

Post × Inspected	-0.0972*** (0.0159)	-0.1041*** (0.0249)	-0.1171*** (0.0342)	-0.0306*** (0.0093)
Pre-treatment Mean	34.46	7.18	6.48	1,310.81

Panel C: Establishments inspected with fines (N=8,583)

Post × Inspected	-0.0834*** (0.0157)	-0.0694** (0.0309)	-0.0796** (0.0331)	-0.0289** (0.0104)
Pre-treatment Mean	34.21	7.17	6.37	1,290.35

Note: *** significant at the 1% level; ** at the 5% level; * at the 10% level. This table reports the aggregate effects of inspections on the following outcomes: ln(number of employees), ln(number of hirings), ln(number of separations), and ln(wages). The estimation sample consists of a semester panel of establishments with 10 to 500 employees that were inspected only once between 2007 and 2017. Each column corresponds to Equation (1), estimated separately for establishments that were inspected without notification, with notification, or with fines. The number of inspected establishments is reported. Means of dependent variables are computed at $t = -1$. Standard errors are clustered at the establishment level. Event-study estimates are shown in Figures O.3–O.6 in Appendix B.

5.2 Responses to Inspections: Severity and Type of Notification

We next examine more granular dimensions of inspections: the number and type of notifications issued, as well as the amount of fines applied.

Number of notifications and fine amount

Table 4 reports establishments' responses by the number of notifications received and the total amount of fines imposed. The sample is smaller than in the previous analysis, as establishments inspected without notification are excluded. Appendix B Figures O.1, O.7–O.10, and O.15–O.18 present the corresponding event-study estimates.

We find little evidence that the intensity of detected infractions amplifies the response to inspections. Across employment, hiring, separations, and wages, the effects are statistically indistinguishable between establishments receiving a single notification and those receiving multiple notifications (Panels A and B). Panels C and D compare establishments with fines above and below the median total amount. While small fines are associated with modest reductions in separations and no effect on hiring, larger fines show the opposite pattern. These differences, however, are estimated imprecisely, with wide confidence intervals that preclude definitive conclusions. No statistically significant differences emerge for employment or wages.

Taken together, these results reinforce the conclusion from the previous section: deterrence, rather than punishment, appears to be the primary mechanism driving establishments' responses to labor inspections.

Table 4: Establishments' Responses by Severity of the Penalty

Outcome:	Ln(N^o Employees) (1)	Ln(N^o hirings) (2)	Ln(N^o separations) (3)	Ln(wages) (4)
<i>Panel A: Establishments that received one notification (N=5,422)</i>				
Post * Inspected	-0.1221*** (0.0255)	-0.1105*** (0.0353)	-0.1598*** (0.0511)	-0.0349** (0.0120)
Pre-treatment Mean	33.50	7.19	6.85	1,348.79
<i>Panel B: Establishments that received more than one notification (N=7,055)</i>				
Post * Inspected	-0.0843*** (0.0201)	-0.1039** (0.0405)	-0.0872** (0.0415)	-0.0276** (0.0150)
Pre-treatment Mean	35.18	7.17	6.21	1,282.77
<i>Panel C: Amount of Fine - Less than R\$ 3,000 (N=3,717)</i>				
Post × Inspected	-0.0977*** (0.0261)	-0.0107 (0.0461)	-0.1654** (0.0690)	-0.0308* (0.0170)
Pre-treatment Mean	28.71	5.61	5.48	1,287.46
<i>Panel D: Amount of Fine - More than R\$ 3,000 (N=4,866)</i>				
Post × Inspected	-0.0763*** (0.0179)	-0.1168** (0.0422)	-0.0096 (0.0355)	-0.0264* (0.0144)
Pre-treatment Mean	38.41	8.37	7.04	1,292.56

Note: ***: significant at 1% level; **: at 5% level; *: at 10% level. This table reports the aggregate effects of inspection on different outcomes: Ln(number of employees), Ln(number of hirings), Ln(number of separations), and Ln(wages). 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; establishments that received more than one notification; establishments fined less than 3,000 Brazilian reais; and ; establishments fined more than 3,000 Brazilian reais. The number of establishments is reported. Means of dependent variables are computed from $\mathcal{L} = -1$. Standard errors are clustered at the establishment level. Event study estimates are presented in Figures O.7-O.10, and O.15-O.18 in Appendix B.

Type of infraction

Table 5 reports the responses of notified establishments by type of infraction. We classify infractions into five categories: (i) worker health and safety, (ii) presence of workers without formal contracts, (iii) failure to pay worker remuneration, (iv) violations of working time regulations, such as excessive hours, and (v) failure to deposit contributions into the employee

severance fund (*FGTS*). These categories together account for nearly 50% of all recorded infractions. Appendix B Table B.1 provides a more detailed description of the specific infractions within each category.

For this analysis, we restrict the sample to establishments notified for a single type of infraction. This ensures a clear classification of establishments but reduces the sample size, resulting in less precise estimates with wide confidence intervals. Event study estimates are reported in Appendix B Figures O.11-O.14.

The main finding is that point estimates of establishment responses to health and safety violations are large and negative for both employment and hiring, whereas infractions related to informality are associated with positive employment effects and no discernible impact on hiring. These estimates, however, are imprecise. A plausible explanation is that health and safety violations often lead to stop-work orders, reducing the workforce—an intended outcome for inspectors seeking to remove workers from hazardous environments. In contrast, violations related to informality may increase employment as firms are compelled to formally register previously uncontracted workers.

Table 5: Establishments' Responses by Type of Infraction

Outcome:	Ln(N^o Employees)	Ln(N^o hirings)	Ln(N^o separations)	Ln(wages)
	(1)	(2)	(3)	(4)
<i>Panel A: Health and Safety (N=363)</i>				
Post × Inspected	-0.1451* (0.0842)	-0.2258* (0.1156)	-0.6109 (0.4876)	-0.0122 (0.0235)
Pre-treatment Mean	38.05	9.10	7.84	1,237.59
<i>Panel B: Informal Worker (N=834)</i>				
Post × Inspected	0.0394 (0.0649)	-0.0061 (0.0910)	-0.0440 (0.1291)	-0.0079 (0.0166)
Pre-treatment Mean	23.35	5.88	5.05	983.93
<i>Panel C: Remuneration (N=933)</i>				
Post × Inspected	-0.0671 (0.074)	-0.0039 (0.1326)	-0.3161 (0.2411)	-0.0735 (0.0521)
Pre-treatment Mean	30.99	6.69	6.32	1,273.29
<i>Panel D: Working Time (N=1,046)</i>				
Post × Inspected	-0.0528*	-0.0751	-0.0123	-0.0067

	(0.0296)	(0.0637)	(0.0653)	(0.0131)
Pre-treatment Mean	28.03	7.01	6.08	1,307.28
<i>Panel E: Contributions (N=335)</i>				
Post × Inspected	-0.1380	-0.1112	-0.2288	-0.0238
	(0.1280)	(0.2655)	(0.1869)	(0.0381)
Pre-treatment Mean	28.87	4.08	6.46	1,041.58

Note: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. This table reports the aggregate effects of inspection on different outcomes: $\ln(\text{number of employees})$, $\ln(\text{number of hirings})$, $\ln(\text{number of separations})$, and $\ln(\text{wages})$. 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 O.11-O.14 in Appendix B.

6 Effects of Labor Inspections on Workers' Outcomes

In the previous sections, we showed that many establishments reduce their size, as well as their hiring and separation levels, following an inspection. In this section, we investigate whether these effects have implications for incumbent workers' subsequent trajectories in the labor market.

6.1 Matching Differences-in-Differences Approach

We implement a matching difference-in-differences approach to investigate worker-level outcomes. The treated group comprises individuals who were working at inspected establishments at the time of inspection. We restrict the worker-level analysis to individuals employed at establishments inspected in 2011, which provides a balanced six-year pre- and post-treatment window. In the Appendix B, we present results for individuals employed at establishments inspected in each year within the 2010-2013 period. These results are very similar to those presented here and lead to the same conclusions discussed in the main text.

We first describe the matching algorithm used to identify a comparable control group and then discuss the implementation of the event study.

6.1.1 Matching algorithm

To select individuals for the control group, we apply an exact matching procedure. First, we restrict the pool of control individuals to workers who were never employed at establishments inspected between 2007 and 2017. Each treated worker is then matched to a control worker with identical values on 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). When multiple eligible controls are available, one is selected at random.

Using this procedure, we successfully match 37,250 treated workers to control units. Table B.5 in Appendix B reports descriptive statistics for treated and control groups in 2010, the year before the inspection, showing that the two groups are well balanced in both demographic and labor market characteristics.

6.1.2 Event-Study

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

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

where subscripts i and t denote worker and semester, respectively; $1[t = 2011 + k]$ is an indicator variable equal to one for each semester, except the semester of the inspection; $Treated_t$ is an indicator variable equal to one for individuals employed at inspected establishments during the event semester and zero for individuals in the control group, as defined in the previous section; α_i represents worker fixed effects; α_t represents semester fixed effects; and y_{it} is the labor market outcome of interest, such as employment or wages. Standard errors are clustered at the worker level.

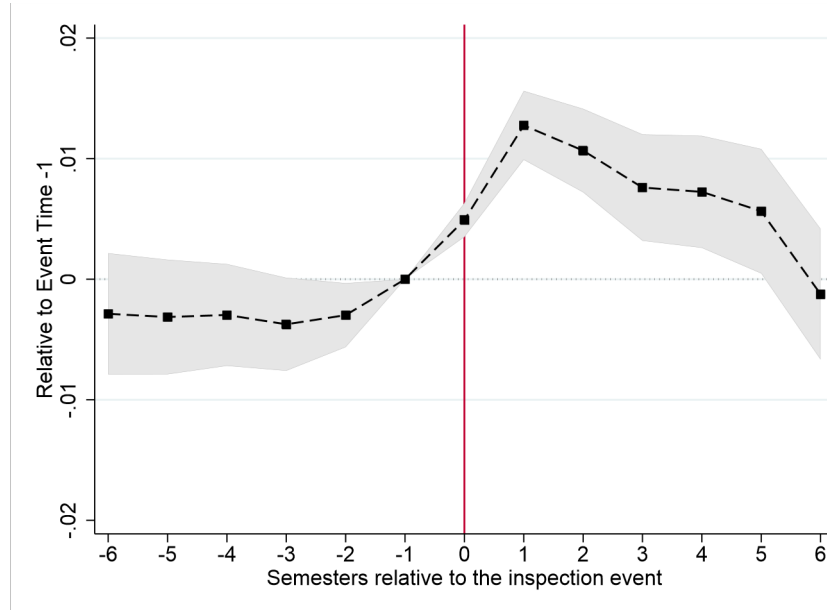
Under the identifying assumptions, the coefficients β_k for $k > 0$, capture the average treatment effect of labor inspections on the treated workers for each period following the inspection. The key identifying assumption is that, in the absence of an inspection, outcomes for workers in treated and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

6.2 Effects of Labor Inspections on Employment

Figure 3 presents the estimated dynamic effects of labor inspections on workers' employment status, using the specification in Equation 2. In the pre-inspection period ($k < 0$), employment differences between treated and control groups show no significant trend, with coefficients fluctuating around zero. This supports the parallel trends assumption and, in turn, the validity of our identification strategy.

Following the inspection, treated workers experience a modest but statistically significant short-term increase in employment probability relative to the control group. One semester after the inspection, the probability of employment rises by about 1%, but this effect gradually dissipates, converging to zero within six semesters¹⁴. This outcome may reflect the formal employment of workers in their new jobs, as the analysis follows the worker rather than the inspected establishment.

Figure 3: The Effects of Inspection on Incumbent Workers' 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 equal to 1 if the worker was employed in period t , and 0 otherwise. The control group consists of workers who, during the analysis period, were not employed at firms that were inspected. The omitted category is the semester before the event. 95% confidence interval are based on standard errors clustered at the worker level.

The relative employment growth in the first period after the inspection may directly reflect establishments' decisions to reduce separations, as documented in Section 4. Inspections prompt establishments to adjust their labor practices, lowering both hiring and separations in response to compliance pressures. As a result, workers employed at these establishments may experience greater short-run job stability, contributing to the observed increase in employment probability. Over time, as establishments adapt to regulatory constraints, this effect dissipates 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 4, establishments experience substantial employment reductions following an inspection. However, these reductions do not

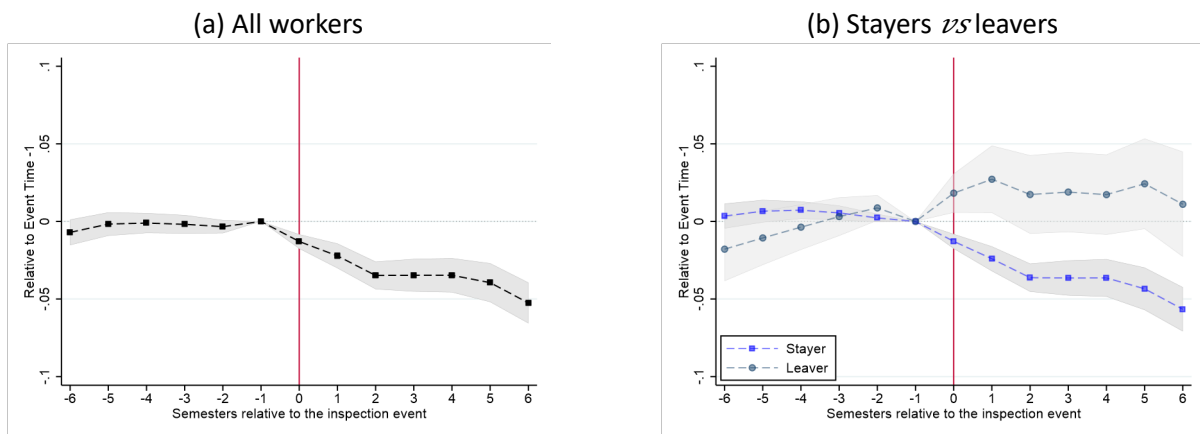
¹⁴ Appendix B Figure B.6 presents results for individuals employed at establishments inspected in each of the years 2010 through 2013. The findings are similar across years, with a maximum employment increase of 2%.

appear to harm 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 align with evidence from labor inspections in Mexico, which similarly show no adverse employment effects for incumbent workers (la Parra and Bujanda, 2024), easing concerns that regulatory enforcement could cause widespread job losses.

6.3 Effects of Labor Inspections on Earnings

Figure 4 presents the effects of labor inspections on workers' wages, estimated using the specification in Equation 2. Panel A shows a statistically significant decline in wages following the inspection, with a persistent downward trend throughout the analysis period. Six semesters after the inspection, treated workers earn about 5% less than their counterparts in the control group. This effect is slightly smaller for other inspection years (see Appendix B Figure O.19). Since real wages rise 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 wages of workers who remained employed in the formal sector, either by staying at the same establishment or by moving to a different employer.

Figure 4: The Effects of Inspection on Incumbent Workers' Earnings



Note: This figure reports point estimates of the effects of inspection on earnings variables using the worker-level sample from RAIS data. Panel A presents estimates for all workers. Panel B reports separate estimates for stayers—workers who remained in the same job they were in at $t = -1$ —and leavers—workers who changed employers. The control group consists of workers who, during the analysis period, were not employed at establishments that were inspected. The omitted category is the semester before the event. 95% confidence intervals are based on standard errors clustered at the worker level.

Panel B of Figure 4 presents the estimated effects of inspections on wages separately for stayers—workers who remain with the same employer—and leavers—workers who move to a different employer after the inspection. The control group for stayers consists of workers who also remained with their employer from 2011 onward, while 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 shown in Appendix B Figure B.9. 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, consequently, 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 subsequent periods. Appendix B Figure B.7 shows similar patterns for other inspection years, with wage gains for leavers reaching up to 5%.

We examine why leavers experience wage gains and find that they typically move to establishments with higher wage premiums. We classify establishments as high or low wage premium based on their estimated fixed effects, obtained using the AKM approach (following Abowd et al., 1999). Specifically, we estimate a log-wage equation with establishment and worker fixed effects, using data covering the universe of formal workers and establishments in Brazil from 2007 to 2017¹⁵.

Figure 5 presents the results from the matching difference-in-differences analysis, where the outcome is the establishment fixed effect of the workers' current job in each year. We report results for individuals employed at establishments inspected in each of the years 2010 through 2013. Across all four figures, a similar pattern emerges: leavers, on average, move to establishments with higher wage premiums. The figures condition on employment at the inspected establishment at $t = 0$. For $t < 0$, some workers were employed in other establishments, which on average offered higher wage premiums, though not as high as those observed post-inspection. Figure 5 also reports the establishment fixed effects of stayers, which change only slightly, reflecting compositional shifts.

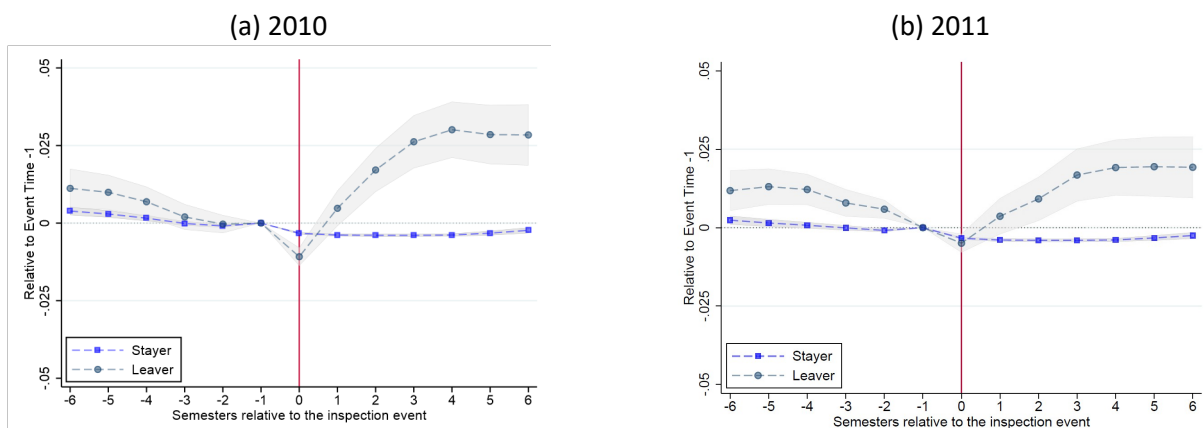
These results align with the predictions of search models such as Meghir et al. (2015) and suggest a potential positive effect of inspections. In such models, random search and labor market frictions can lead to unfavorable matches between workers and low-productivity firms. In our setting, inspected firms tend to have lower wage premiums, potentially reflecting lower

¹⁵ The sample includes 91,213,054 workers and 6,811,529 establishments, totaling 715,354,986 observations. Appendix A provides further details on the AKM estimation.

productivity¹⁶. Workers may therefore be poorly matched to these firms. Inspections can disrupt these matches, pushing some workers toward better employers. This can occur either because inspections increase incentives for on-the-job search—leading to improved matches—or because involuntary separations trigger reemployment in jobs that, on average, offer better conditions than their previous ones¹⁷. These mechanisms point to a potential positive role of inspections: because inspections disproportionately occur in establishments with lower fixed effects—likely a byproduct of the complaint-driven process—they can facilitate workers’ transitions to better opportunities.

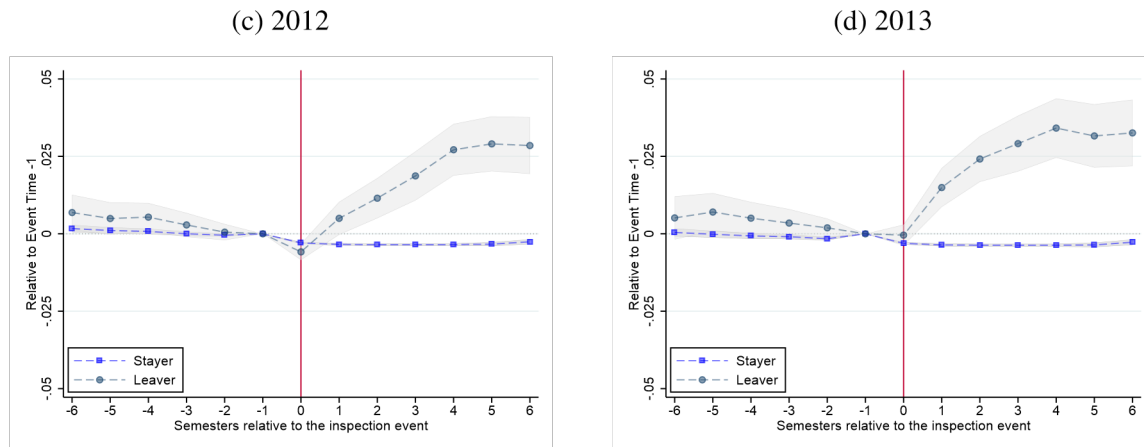
In summary, despite the significant and statistically robust negative effects at the establishment level, the evidence does not support the hypothesis that these impacts translate into higher non-formal employment or lower wages for workers. Instead, the results point to a small short-term increase in formal employment, slower wage growth for workers who remain in inspected establishments, and wage gains for those who change jobs—driven mainly by their transition to higher-wage-premium establishments.

Figure 5: Establishment Fixed Effects of Workers from Inspected Establishments for 2010–2013 (Stayers vs. Leavers)



¹⁶ Inspected establishments have, on average, establishment fixed effects that are 5.3% lower than those of the average establishment in Brazil. Lower wage premiums could also capture factors unrelated to productivity, such as compensating differentials for amenities.

¹⁷ This effect may reflect mean reversion, as displaced workers are re-matched to the unconditional distribution of establishments, which on average offer higher wage premiums than the inspected firms.



Note: This figure reports estimates of the establishment fixed effect in which workers from inspected establishments are employed in each year. Establishment fixed effects are obtained from a log-wage equation following Abowd et al. (1999), commonly referred to as the AKM approach. More details on the estimation of these parameters are provided in the Appendix A. The figure uses the worker-level sample from RAIS data and plots separate estimates for stayers —workers who remained in the same job they were in at $t = -1$ — and leavers —workers who changed employers. The control group consists of workers who, during the analysis period, were not employed at establishments that were inspected. The omitted category is the semester before the event. 95% confidence interval are based on standard errors clustered at the worker level.

7 Conclusion

This paper examines a widely discussed trade-off in the enforcement of labor regulations. Although enforcement through inspections is intended to ensure compliance with labor legislation, achieving such compliance can impose significant costs. Consequently, the economic outcomes are ambiguous, potentially resulting in job losses and wage reductions, or alternatively, in job creation and wage gains.

Using rich administrative data from Brazil, we document how regulatory enforcement shapes both establishments' and workers' trajectories. At the establishment level, inspections significantly reduce employment, driven primarily by declines in hiring rather than increases in separations. Part of this reduction appears intentional, driven by violations related to worker health and safety. Another part occurs even in establishments found to have no violations, possibly reflecting additional costs imposed on business owners by the inspection process.

At the worker level, inspections do not reduce the employability of affected workers; however, they are associated with lower wage growth for those who remain employed in inspected establishments. In contrast, workers who move to new jobs experience wage gains, largely explained by transitions to establishments with higher wage premiums. We interpret these patterns as evidence of labor market frictions, consistent with the framework of Meghir et al. (2015). In this context, inspections may help reallocate workers toward more productive firms, potentially increasing aggregate productivity and offsetting some of the perceived costs of enforcement.

Developing economies often have a large share of low-productivity firms, for which compliance with labor legislation can be costly (Ulyssea, 2018). While stricter enforcement

may burden such firms, the absence of persistent negative employment effects for workers suggests that labor market transitions are relatively smooth, allowing displaced workers to reallocate

without long-term adverse consequences. By documenting these trade-offs in labor inspections, our findings offer valuable insights for designing regulatory policies that strengthen worker protections while minimizing unintended economic distortions.

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A Appendix: AKM

Section 6 analyzes worker mobility using establishment fixed effects, estimated from a log-wage equation with worker and establishment fixed effects, commonly known as the AKM model. This section briefly outlines the estimation procedure for establishment fixed effects.

We use RAIS data from 2007 to 2017, restricting the sample to workers employed at least 20 hours per week and earning positive wages. We exclude workers with more than 100 observations in the dataset between 2003 and 2023. The resulting largest connected set comprises 91,213,054 workers and 6,811,529 establishments, totaling 715,354,986

observations. This sample covers 99.6% of total observations and 92.6% of all establishments active during the period.

The goal is to characterize establishments as high- or low-wage premium. We estimate the following reduced-form equation:

$$\ln(w_{it}) = \theta_i + \psi_{j,i,t} + X_{it}\beta + \lambda_t + \varepsilon_{it} \quad (3)$$

where w_{it} denotes average hourly wages, θ_i are worker fixed effects, $\psi_{j,i,t}$ are establishment fixed effects (with j indicating the establishment where worker i is employed in year t), X is a polynomial of age¹⁸, and λ_t are year fixed effects.

From this estimation, we use the estimates of $\psi_{j,i,t}$ to characterize the establishments to which workers from inspected firms transition. Caution is warranted when interpreting these estimates: a higher establishment fixed effect indicates that such establishments pay higher wages relative to what workers earn elsewhere. These “higher wages” may reflect higher productivity, compensating differentials for fewer valued amenities, monetary or otherwise, among other factors.

For reference, though unrelated to the objectives of this study, the mean hourly wage in Brazilian Reais in 2003 is 1.91, with a variance of 0.53. This variance decomposes into worker effects (57.3%), establishment effects (26.7%), time-varying observable characteristics ($X\beta$) (3.7%), and residual variation (12.1%). For additional examples of AKM estimation using Brazilian RAIS data, see Gerard et al. (2021) and Engbom and Moser (2022).

B Appendix: Tables and Figures

Table B.1: Types of violations verified during inspections

Infraction Group	Infraction	Description
Health and safety	Occupational Health and Safety	A set of mandatory occupational health and safety standards issued by the Brazilian Ministry of Labor. These standards—known as Normas Regulamentadoras (NRs)—cover a wide range of workplace conditions and risks, such as the use of personal protective equipment (PPE), machine safety, work at height, confined spaces, ergonomics, and hazardous substances
	Employment Registration and CTPS	Infractions related to the lack of labor registration or failure to properly record information in the Work and Social Security Card (CTPS)
Informal worker	Individual employment contract	Irregularities in formal employment agreements, such as lack of documentation or missing legal clauses
	Wages and Compensation	Late or missing wage payments, including bonuses, commissions, and legal wage supplements
Remuneration	Minimum wage	Payment of wages below the national minimum wage
	Christmas bonus/13th salary	Non-payment or delayed payment of the mandatory year-end bonus (13th salary)
	Transportation voucher	Failure to provide or improper deduction of the transportation benefit
	Unemployment insurance	Failure to issue or delay in issuing unemployment insurance forms.

¹⁸ Following (Card et al., 2013), we use the transformation $(age - 40)/40$ and include its square and cubic terms.

Working time	Working hours	Violations involving excessive working hours, lack of time tracking, non-provision of rest breaks, or unpaid overtime
	Work schedule Display	Absence or incorrect display of the employee work schedule in the workplace
	Vacation/Paid leave	Irregularities such as non-payment, untimely granting, or improper division of paid vacation time
	Rest period	Violations related to daily or weekly rest periods, and intra-work breaks
	Night work	Irregularities in pay or working conditions for night shifts
Contributions	Employee Severance Fund (FGTS) FGTS's Social Contribution	Failure to deposit mandatory contributions to the Severance Pay Fund (FGTS) Non-payment of the additional 10% FGTS contribution
Child and Adolescent Labor	Child Labor Protection	Employment of minors in prohibited activities, irregular hours, or without registration
Other	Contract Modification	Unauthorized or unilateral changes in the employment contract that harm the worker
	Contract suspension/interruption	Cases in which the employment contract is suspended or interrupted under conditions not permitted by law.
	Notice of termination/Prior notice	Failure to provide or correctly calculate prior notice of termination
	RAIS and CAGED	Failure to submit, or submission with errors, of mandatory employment reports: the Annual Social Information Report (RAIS) and the General Register of Employed and Unemployed Persons (CAGED).
	Related to the labor inspection	An infraction related to the labor inspection occurs when an employer denies access to the workplace or to the requested documents, or refuses to provide information to the labor inspector.
	Unions	Irregularities in collection or non-transfer of union dues, including lack of compliance with union laws
	Professional drivers	Non-compliance with labor legislation specific to professional drivers (Law No. 13.103/2015), including violations related to working hours, rest and breaks, unpaid waiting time, and failures to provide mandatory insurance and asset protection.
	Apprenticeship	Noncompliance with legal requirements for hiring and training apprentices, such as not meeting the minimum quota, lacking formal contracts or training enrollment, or assigning tasks unrelated to professional development
	People with Disabilities (PwD)	Noncompliance with hiring quotas for workers with disabilities
	Outsourcing	Irregular outsourcing practices, such as failure to supervise service providers or using intermediaries to avoid direct employment
	Women's Labor	Failure to comply with protective rules for women, such as maternity rights, work restrictions, and stability during pregnancy
	Domestic work	Violations of the Domestic Workers' Law (Complementary Law No. 150/2015), such as non-payment of FGTS or overtime
	Rural work	Violations of rural labor laws, especially Law No. 5,889/1973, regarding contracts, conditions, and housing

Note: This table presents the classification of labor violations according to the Brazilian Ministry of Labor and Employment (MTE), based on categories from the Radar SIT database (<https://sit.trabalho.gov.br/radar/>).

Table B.2: Heterogeneity: Average Effects of Inspection on Establishments' Outcomes by Size

	(1) Ln(number of employees)	(2) Ln(number of hirings)	(3) Ln(number of separation)	(4) Ln(wages)
<i>Panel A: Establishment with size between 10 to 49</i>				
Post × Inspected	-0.0666*** (0.0031)	-0.0770*** (0.0057)	-0.0508*** (0.0051)	- 0.0084*** (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
<i>Panel B: Establishment with size between 50 to 500</i>				
Post × Inspected	-0.1034*** (0.0157)	-0.1350*** (0.0278)	-0.0696*** (0.0299)	-0.0203** (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(number of employees), Ln(number of hirings), Ln(number of separations), and Ln(wages). The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The data are semiannual. The control group consists of establishments not yet inspected. 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 and the pre-treatment mean of each dependent variable (measured at t=-1) are reported in the last two rows. Standard errors are clustered at the establishment level.

Table B.3: Heterogeneity: Average Effects of Inspection on Establishments' Outcomes by Age

	(1)	(2)	(3)	(4)
	Ln(number of employees)	Ln(number of hirings)	Ln(number of separation)	Ln(wages)
<i>Panel A: Establishment with ages up to 3 years</i>				
Post × Inspected	-0.1556*** (0.0199)	-0.2093*** (0.0398)	-0.1363*** (0.0368)	-0.0219 (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
<i>Panel B: Establishment aged between 3 and 5 years</i>				
Post × Inspected	-0.1034*** (0.0163)	-0.1284*** (0.0222)	-0.1026** (0.0351)	-0.0218** (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
<i>Panel C: Establishment older than 5 years</i>				
Post × Inspected	-0.0646*** (0.0062)	-0.0740*** (0.0132)	-0.0312** (0.0131)	-0.0099** (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(number of employees), Ln(number of hirings), Ln(number of separations), and Ln(wages). The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The data are semiannual. The control group consists of establishments not yet inspected. All columns refer to Equation (1) restricted to one of the following age categories: less than 3 years, 3-5 years, and more than 5 years. The number of establishments and the pre-treatment mean of each dependent variable (measured at t=-1) are reported in the last two rows. Standard errors are clustered at the establishment level.

Table B.4: Heterogeneity: Average Effects of Inspection on Establishments' Outcomes by Sectors

	(1)	(2)	(3)	(4)
	Ln(number of employees)	Ln(number of hirings)	Ln(number of separation)	Ln(wages)
<i>Panel A: Industry</i>				
Post × Inspected	-0.0982*** (0.0110)	-0.1240*** (0.0249)	-0.0683*** (0.0205)	-0.0073 (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*** (0.0086)	-0.0709*** (0.0120)	-0.0390** (0.0145)	-0.0082** (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*** (0.0096)	-0.0713*** (0.0183)	-0.0284 (0.0193)	-0.0155** (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*** (0.0573)	-0.2808** (0.1284)	-0.2715** (0.0967)	-0.0408 (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(number of employees), ln(number of hirings), ln(number of separations), and ln(wages). The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The data are semiannual. The control group consists of establishments not yet inspected. All columns refer to Equation (1) restricted to one of the following sectors: industry, services, construction, and commerce. The number of establishments and the pre-treatment mean of each dependent variable (measured at t-1) are reported in the last two rows. Standard errors are clustered at the establishment level.

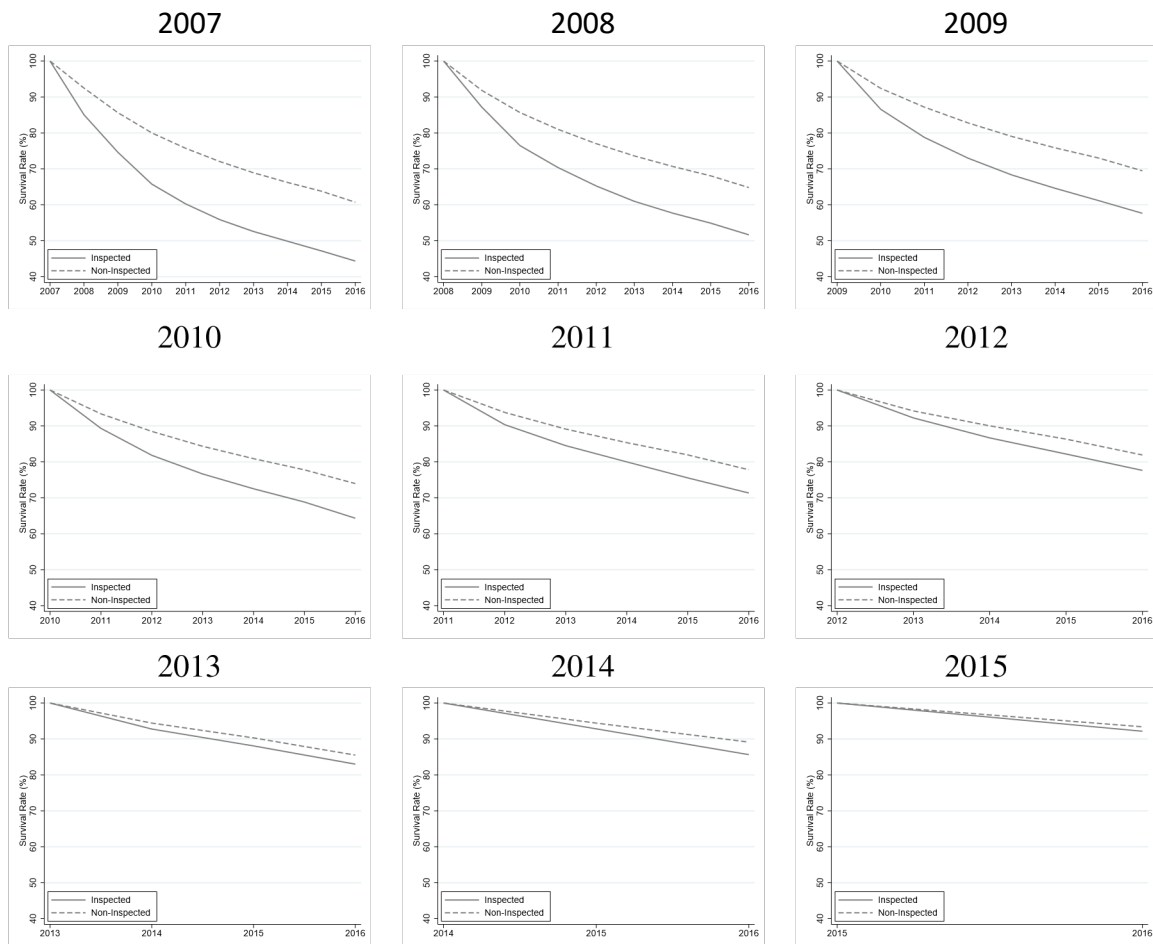
Table B.5: Descriptive Statistics - Worker

	Treated	Control
Earnings	3,065 (4,338)	3,199 (4,324)
Ln Earnings	7.54 (1.16)	7.56 (1.21)
Age	40.22 (11.62)	40.45 (11.60)
Male	0.595 (0.491)	0.518 (0.50)
Education		
Less than Primary	0.181 (0.385)	0.206 (0.404)
Primary	0.199 (0.399)	0.174 (0.379)
High School	0.405 (0.491)	0.402 (0.49)

College	0.189 (0.392)	0.193 (0.394)
Occupation		
Manager	0.054 (0.226)	0.047 (0.212)
Professional	0.198 (0.398)	0.201 (0.400)
White Collar Lower Level	0.205 (0.404)	0.202 (0.401)
Blue Collar	0.544 (0.498)	0.551 (0.0497)
Number of Observations	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 B.1: Survival Rates of Establishments, 2007–2015

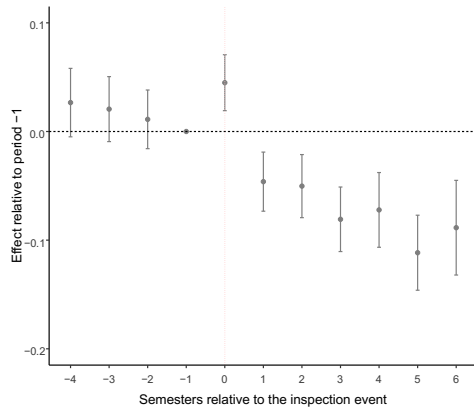


Note: The figure shows survival rates for establishments that were inspected compared to those that were never inspected. Each panel corresponds to establishments inspected in the given year and conditions on survival in that same year for both groups. An exit is defined as the last year the establishment is observed with formal contracts in RAIS.

Figure B.2: The Effects of Inspection on Establishments' Number of Hired Workers

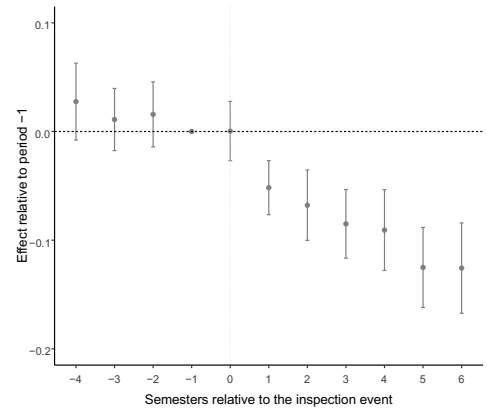
(a) Ln(number of hires)

Out of formal labor market in $t=-1$



(b) Ln(number of hires)

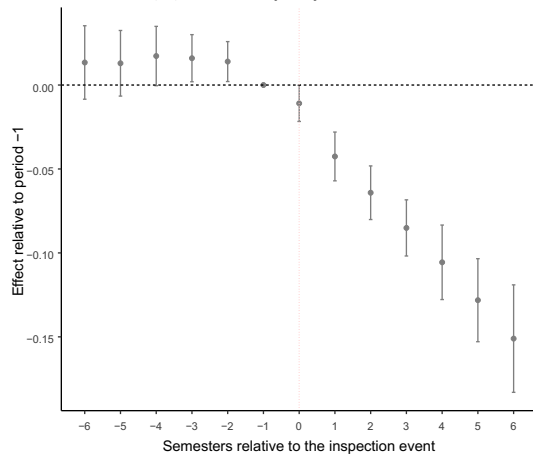
Active in the formal labor market in $t=-1$



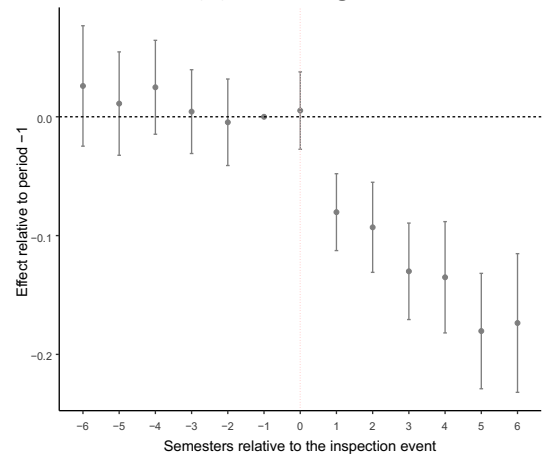
Note: This figure reports point estimates of the effects of inspection on Ln(number of hiring) using the establishment-level sample from RAIS matched with inspection data. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The control group consists of establishments not yet treated. 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 B.3: Robustness: The Effects of Inspection on Establishments' Outcomes - IHS transformation

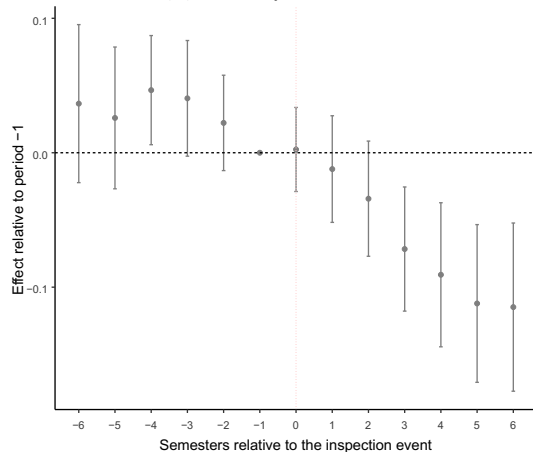
(a) IHS employment



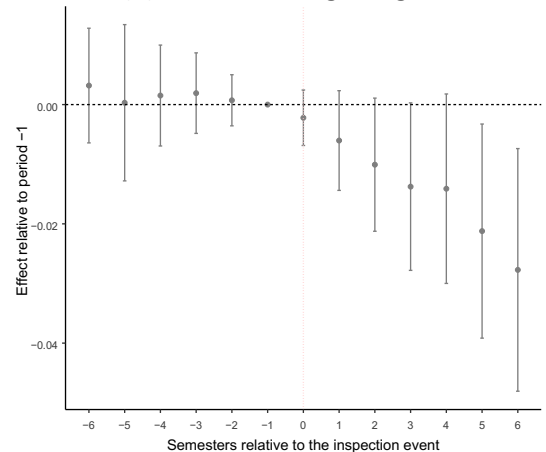
(b) IHS hiring



(c) IHS separation



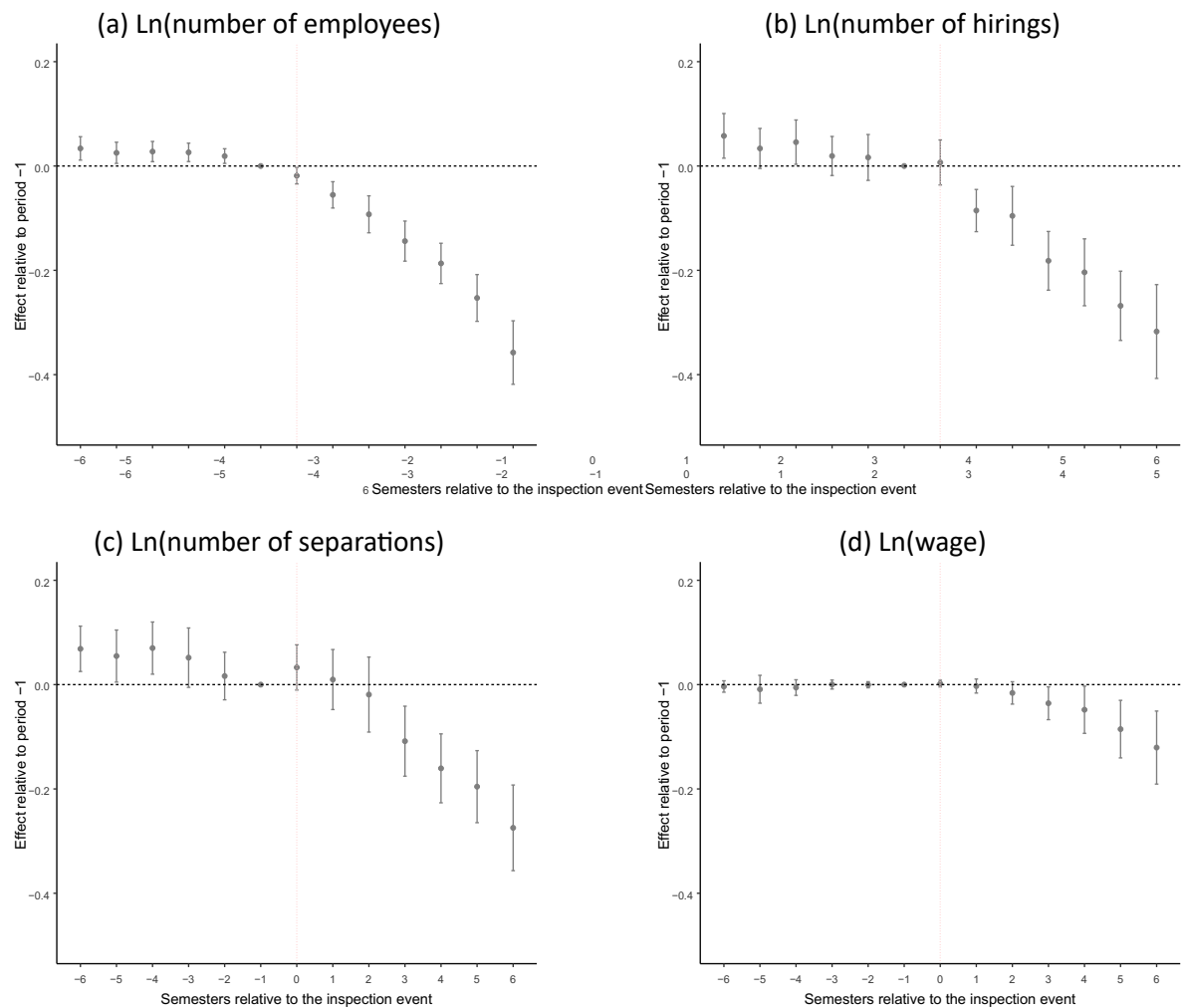
(d) IHS real average wage



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS matched with inspection data. In this robustness, we apply the IHS transformation to the variables. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and

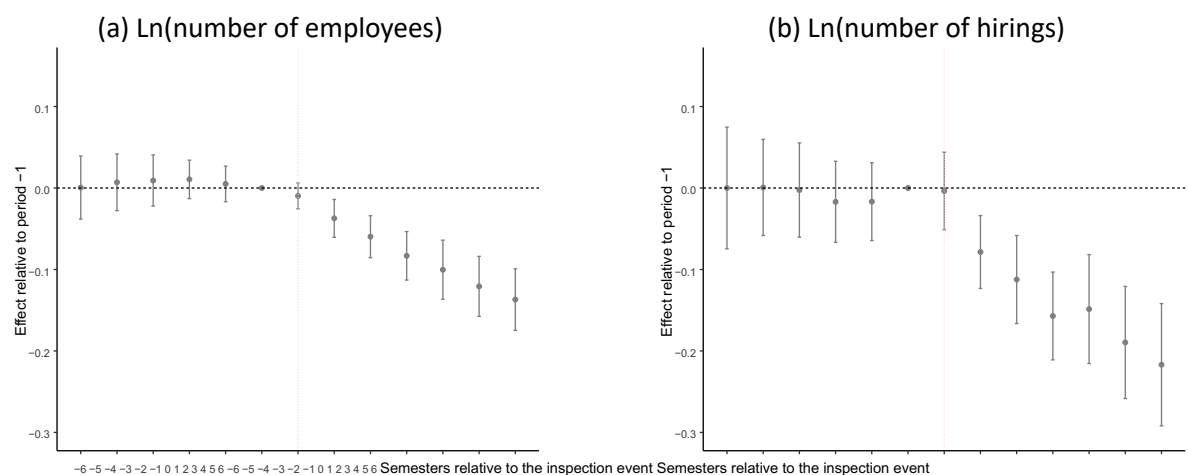
2017. The control group consists of establishments not yet treated. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

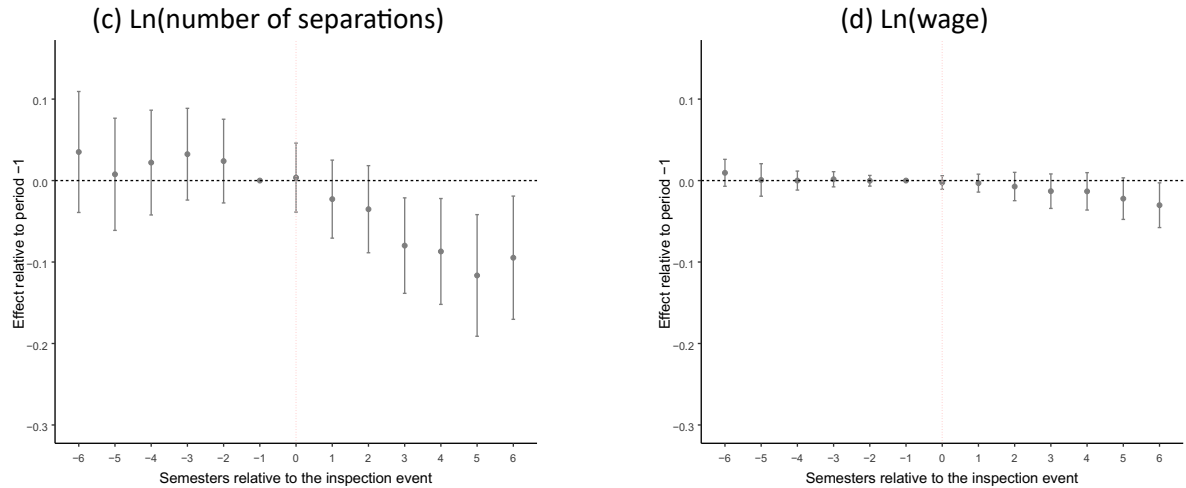
Figure B.4: Robustness: The Effects of Inspection on Establishments' Outcomes - stayers



Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS matched with inspection data. The sample includes establishments with 10 to 500 employees, inspected only once between 2007 and 2017. Observations are restricted to a semiannual panel balanced within a $[-6, +6]$ window around the inspection. The control group consists of establishments not yet treated. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

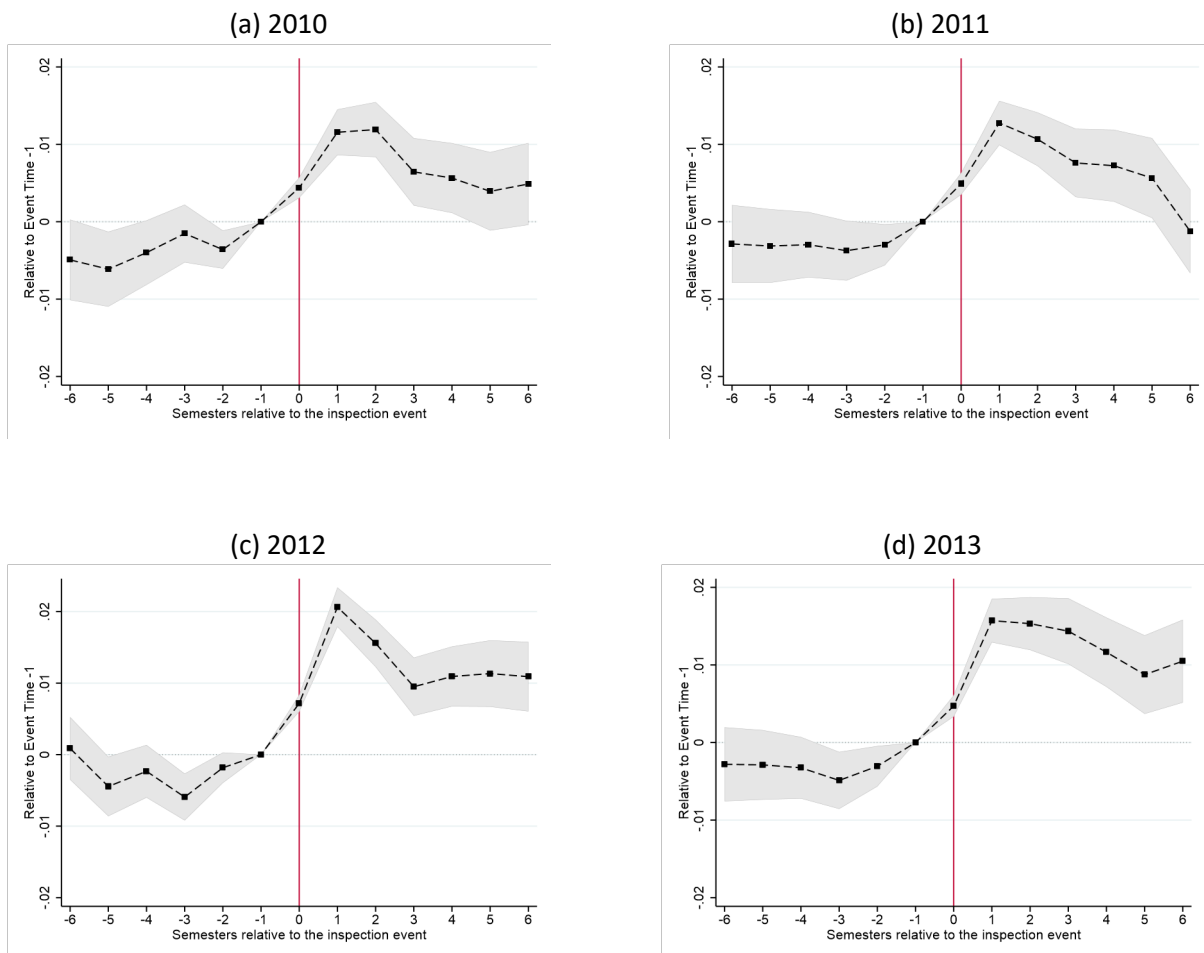
Figure B.5: Robustness: The Effects of Inspection on Establishments' Outcomes - Drop Treated Pre-2007





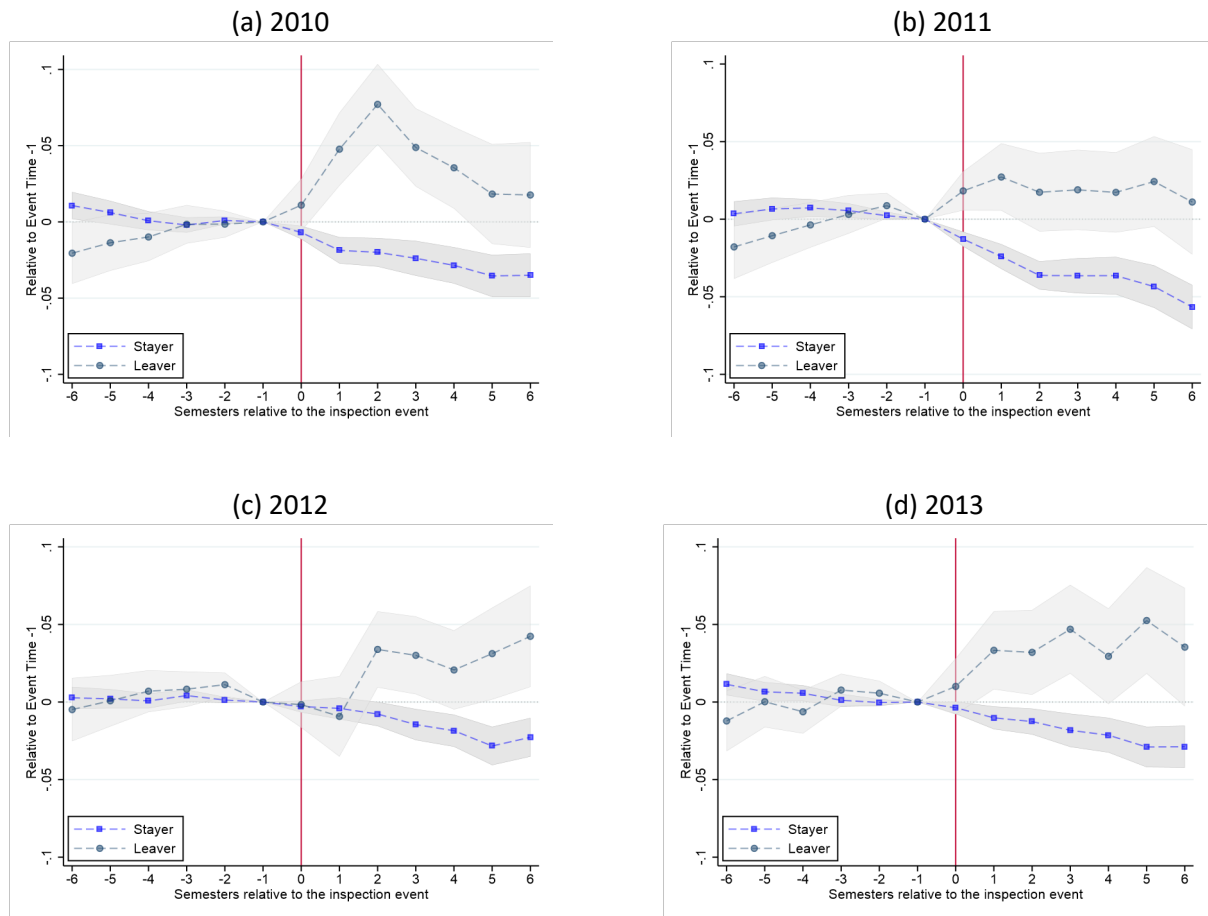
Note: This figure reports point estimates of the effects of inspection on different outcomes using the establishment-level sample from RAIS matched with inspection data. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The sample is restricted to establishments that were not inspected between 2003 and 2007, in addition to the construction restrictions of the main sample. The control group consists of establishments not yet treated. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure B.6: The Effects of Inspections on Workers' Formal Employment for 2010–2013



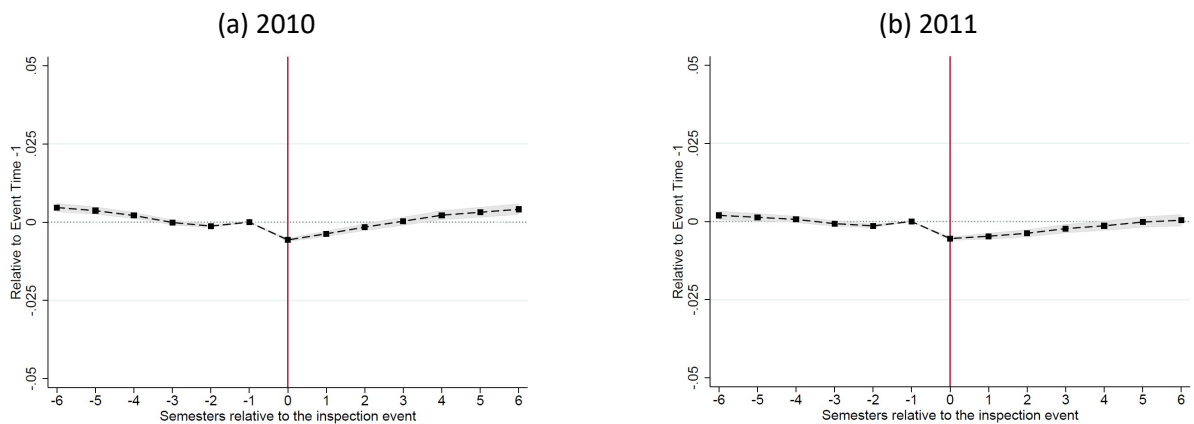
Note: This figure reports point estimates of the effects of inspection on employment variable using the worker-level sample from RAIS data. Employment variable is a dummy equals 1 if the worker was employed in i , and 0 otherwise. The control group consists of workers who, during the analysis period, were not employed at firms that were inspected. Panel A includes workers treated in 2010, Panel B those treated in 2011, Panel C in 2012, and Panel D in 2013. Each panel includes only the corresponding treated cohort. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the worker level.

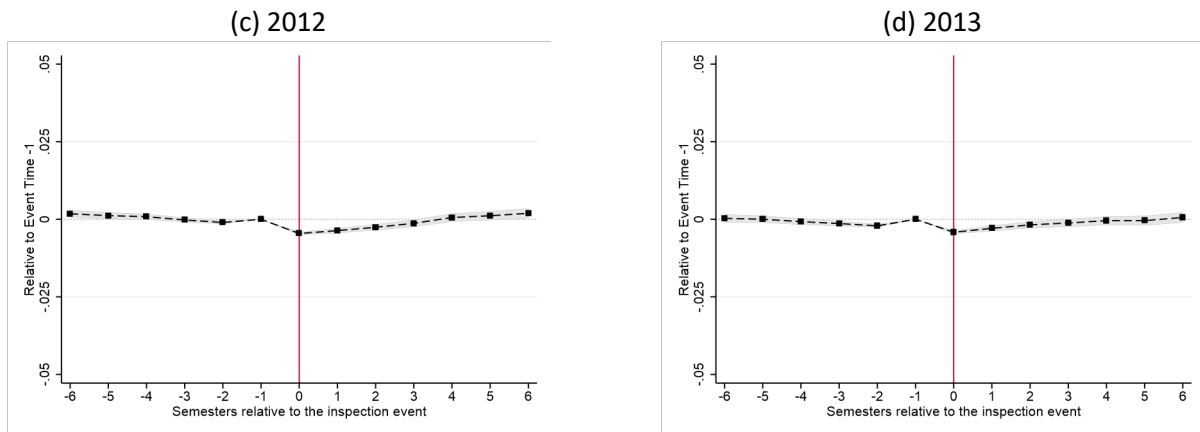
Figure B.7: The Effects of Inspections on Workers' Earnings for 2010–2013 (Stayers vs. Leavers)



Note: This figure reports point estimates of the effects of inspection on earnings variables using the worker-level sample from RAIS data. 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 control group consists of workers who, during the analysis period, were not employed at firms that were inspected. Panel A includes workers treated in 2010, Panel B those treated in 2011, Panel C in 2012, and Panel D in 2013. Each panel includes only the corresponding treated cohort. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the worker level.

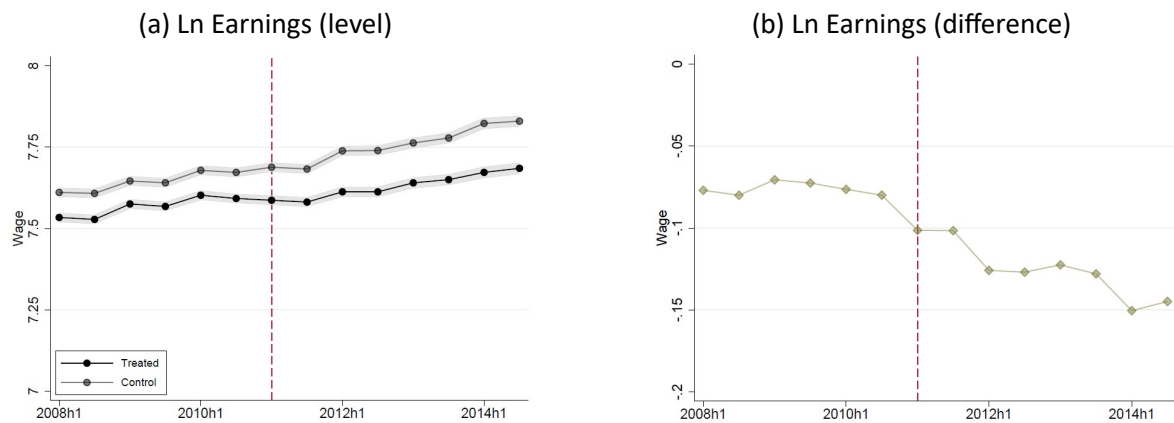
FigureB.8: EstablishmentFixedEffectsofWorkersfromInspectedEstablishmentsfor2010–2013





Note: This figure reports estimates of the establishment fixed effect in which workers from inspected establishments are working in each year. Establishment fixed effects are obtained from a log-wage equation as in Abowd et al. (1999), commonly referred to as AKM. More details on the estimation of these parameters are provided in the Appendix. The figure uses the worker-level sample from RALS data. The control group consists of workers who, during the analysis period, were not employed at firms that were inspected. Panel A includes workers treated in 2010, Panel B those treated in 2011, Panel C in 2012, and Panel D in 2013. Each panel includes only the corresponding treated cohort. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the worker level.

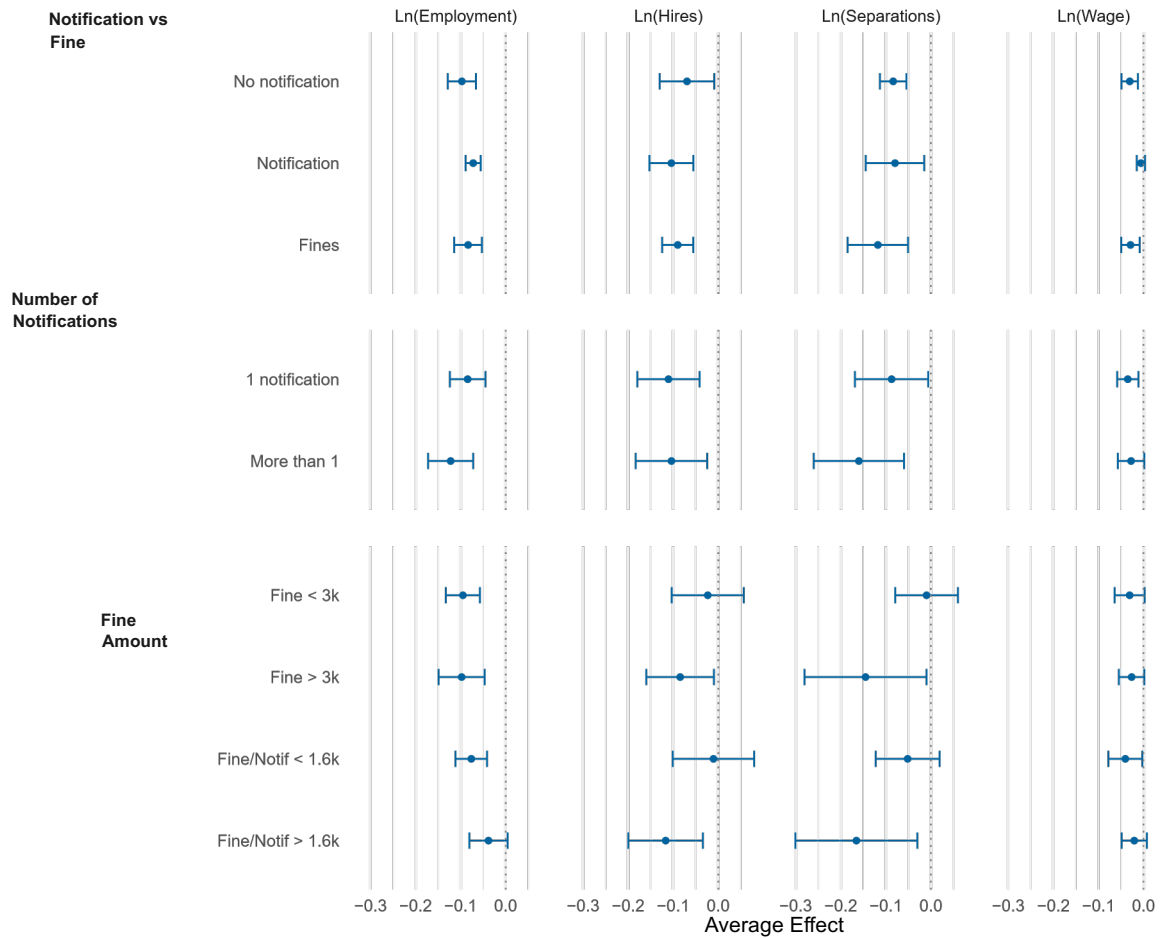
Figure B.9: Evolution Ln Earnings - Stayers



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.

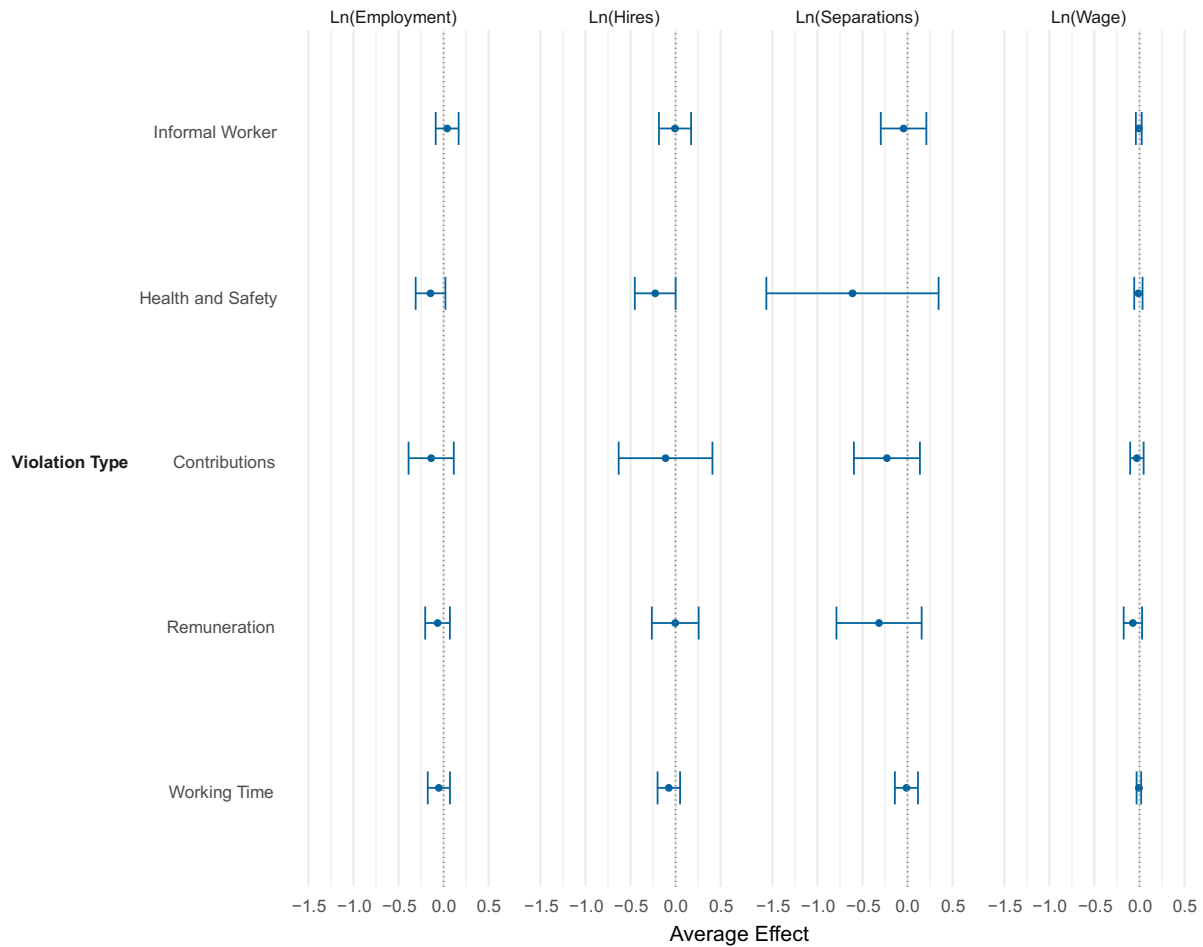
C Online Appendix

Figure O.1: Heterogeneous Effects of Inspections on Establishments' Outcomes - Inspection Outcome and Severity of the Penalty



Note: The figure reports the aggregate effects of inspection on different outcomes: $\ln(\text{number of employees})$, $\ln(\text{number of hirings})$, $\ln(\text{number of separations})$, and $\ln(\text{wages})$. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The control group consists of establishments not yet treated. '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 $\text{US\$}3,000$, total fine amount more than $\text{US\$}3,000$, fine amount for notification less than $\text{US\$}1,600$, and fine amount for notification more than $\text{US\$}1,600$. The figure plots point estimates and the 95% confidence interval. Standard errors are clustered at the establishment level.

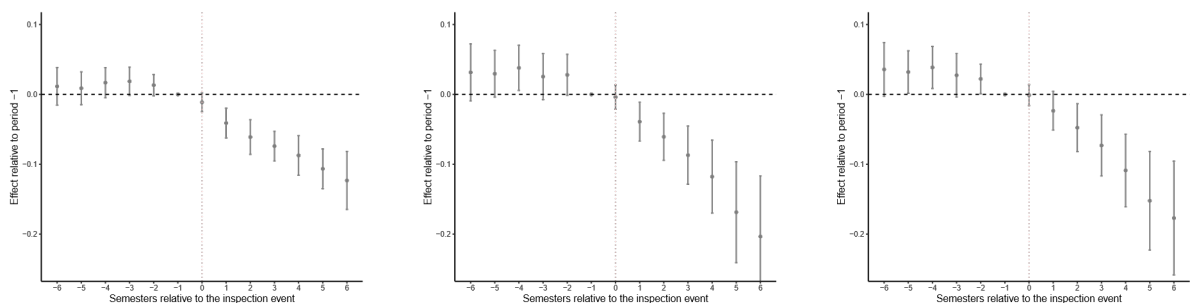
Figure O.2: Heterogeneous Effects of Inspections on Establishments' Outcomes - Type of Infraction



Note: The figure reports the aggregate effects of inspection on different outcomes: $\ln(\text{number of employees})$, $\ln(\text{number of hirings})$, $\ln(\text{number of separations})$, and $\ln(\text{wages})$. The estimation sample consists of a semester panel of establishments from 10 to 500 inspected only once between 2007 and 2017. The control group consists of establishments not yet treated. 'Violation Type' is divided into: notified of irregularities in workers' health and safety, notified of 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 O.3: Potential Mechanisms-Inspection Outcome: The Effect of Inspection on $\ln(\text{number of employees})$

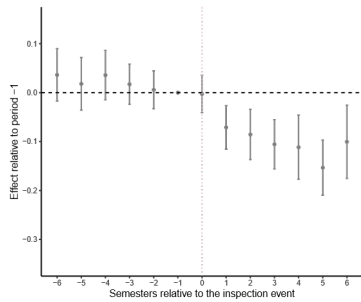
(a) Establishments inspected without notification (b) Establishments inspected with notification (c) Establishments inspected with fines



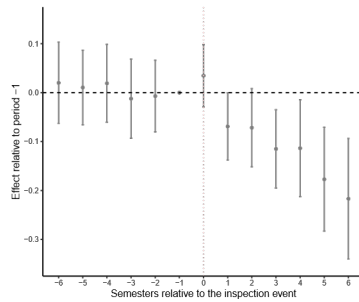
Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of employees})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to inspections without notification; Panel B to inspections with notification; and Panel C to inspections resulting in fines. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure O.4: Potential Mechanisms-Inspection Outcome: The Effect of Inspection on $\ln(\text{number of employees})$

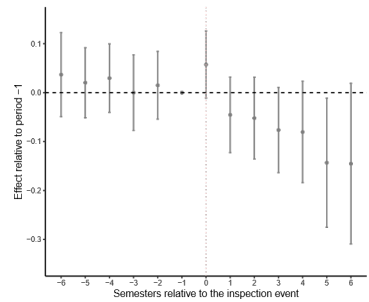
(a) Establishments inspected without notification



(b) Establishments inspected with notification



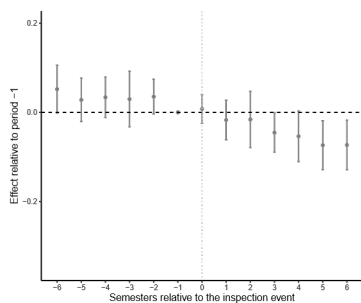
(c) Establishments inspected with fines



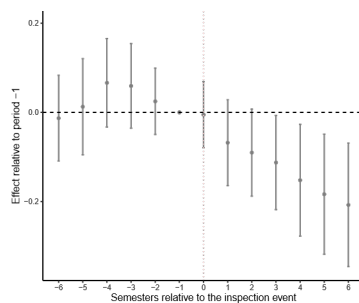
Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of hirings})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to inspections without notification; Panel B to inspections with notification; and Panel C to inspections resulting in fines. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure O.5: Potential Mechanisms-Inspection Outcome: The Effects of Inspection on $\ln(\text{number of separations})$

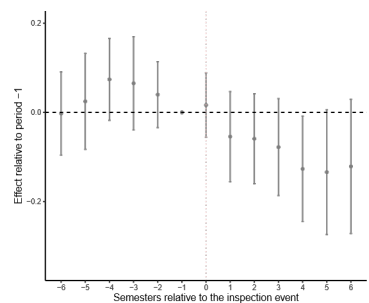
(a) Establishments inspected without notification



(b) Establishments inspected with notification

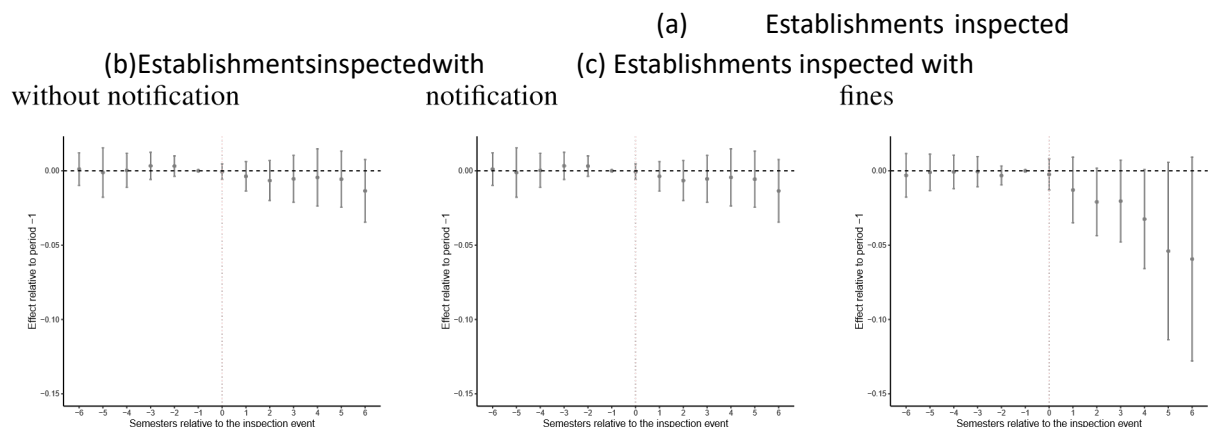


(c) Establishments inspected with fines



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of separations})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to inspections without notification; Panel B to inspections with notification; and Panel C to inspections resulting in fines. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

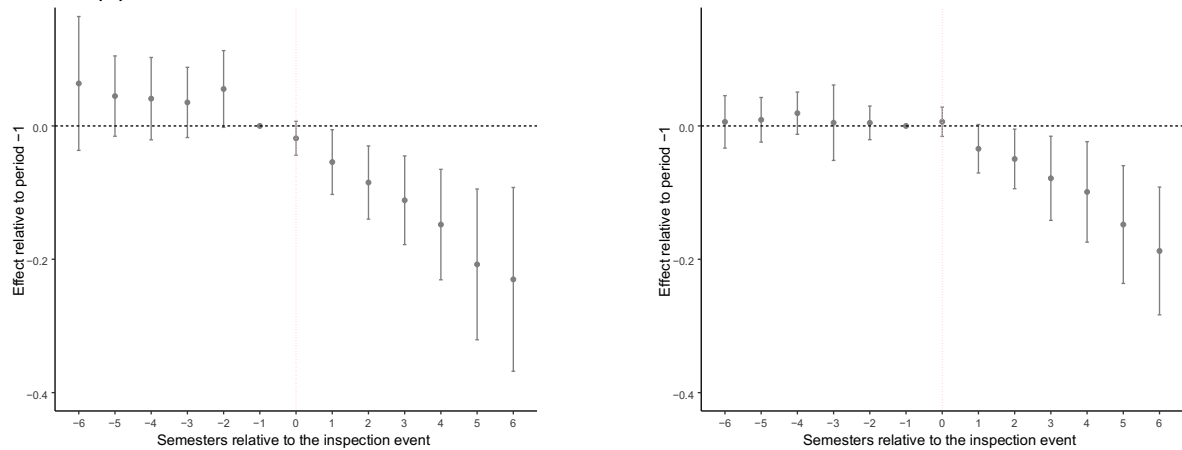
Figure O.6: Potential Mechanisms - Inspection Outcome: The Effects of Inspection on $\ln(\text{wages})$



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{wages})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to inspections without notification; Panel B to inspections with notification; and Panel C to inspections resulting in fines. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure O.7: Potential Mechanisms - Severity of the Penalty: The Effects of Inspection on $\ln(\text{number of employees})$

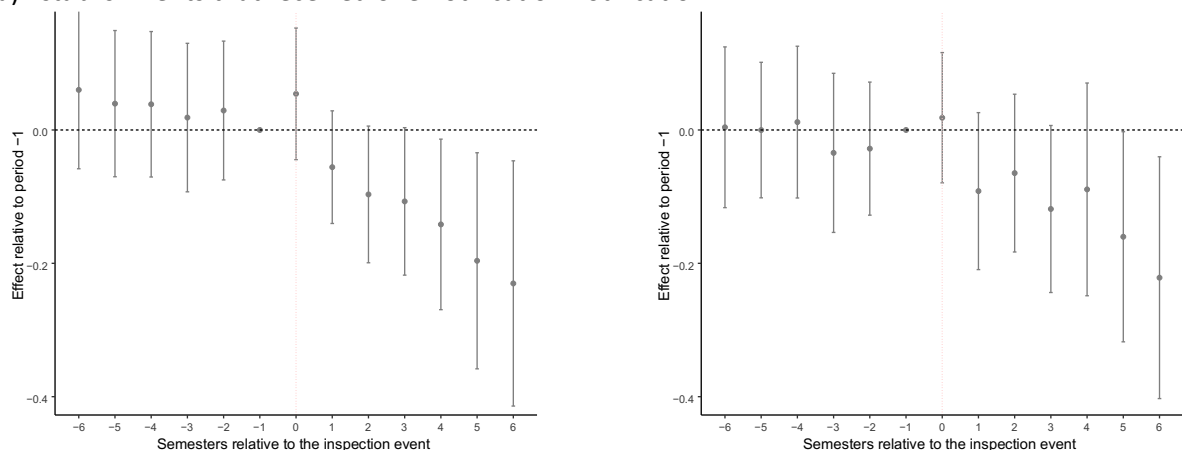
(b) Establishments that received more than one notification
(a) Establishments that received one notification



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of employees})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received one notification; Panel B refers to establishments that received more than one notification. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure O.8: Potential Mechanisms - Severity of the Penalty: The Effects of Inspection on $\ln(\text{number of hirings})$

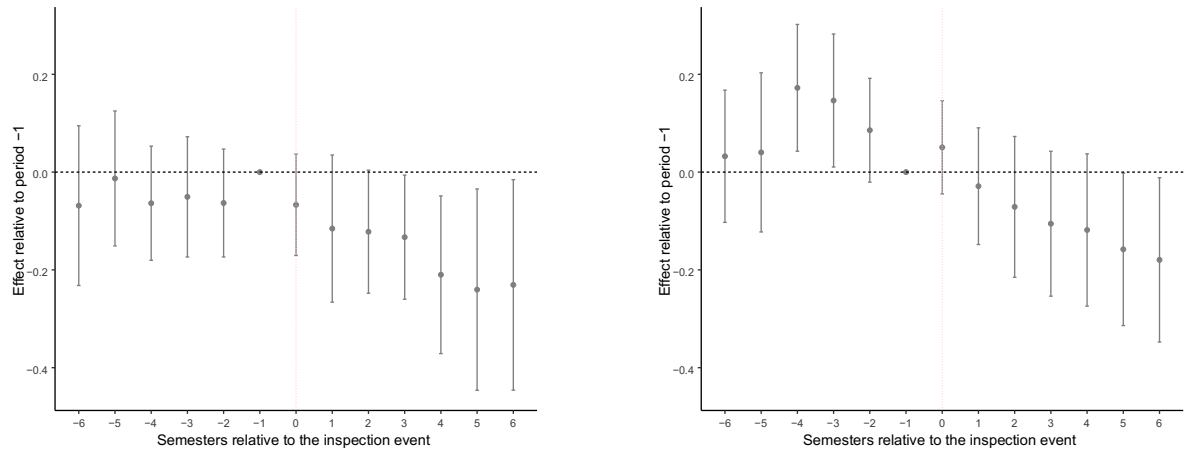
(b) Establishments that received more than one notification
(a) Establishments that received one notification



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of hirings})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received one notification; Panel B refers to establishments that received more than one notification. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

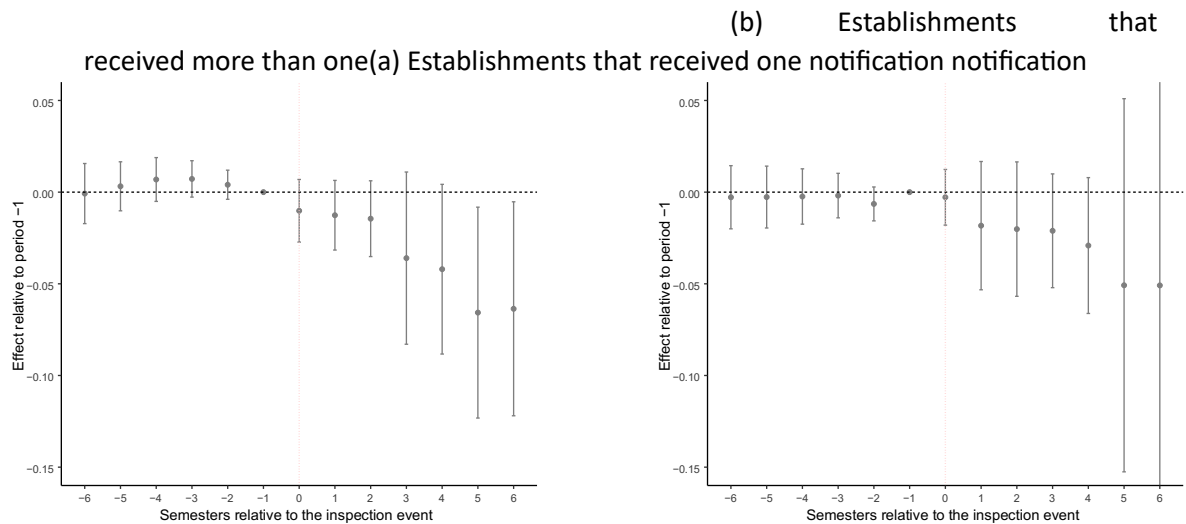
Figure O.9: Potential Mechanisms - Severity of the Penalty: The Effects of Inspection on $\ln(\text{number of separation})$

(b) Establishments that received more than one notification
(a) Establishments that received one notification



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of separations})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received one notification; Panel B refers to establishments that received more than one notification. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

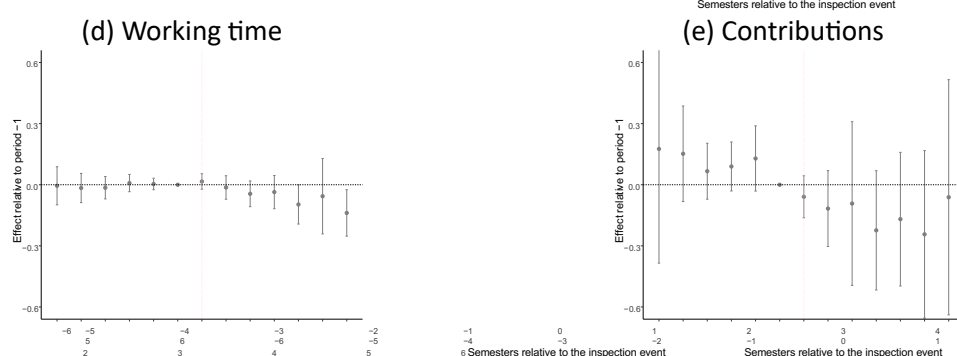
Figure O.10: Potential Mechanisms - Severity of the Penalty: The Effects of Inspection on $\ln(\text{wages})$



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{wages})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received one notification; Panel B refers to establishments that received more than one notification. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

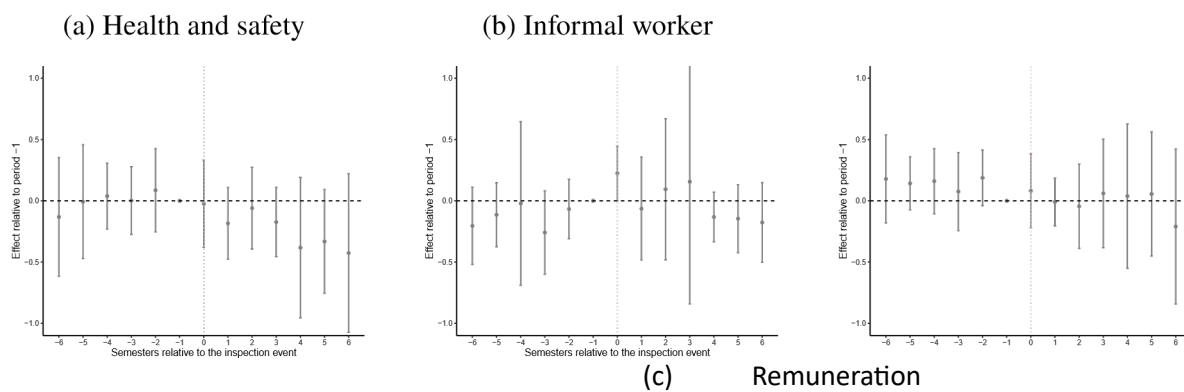
Figure O.11: Potential Mechanisms - Type of Infraction: The Effects of Inspection on Ln(number of employees)

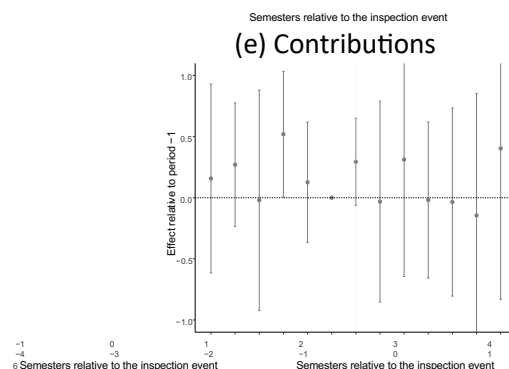
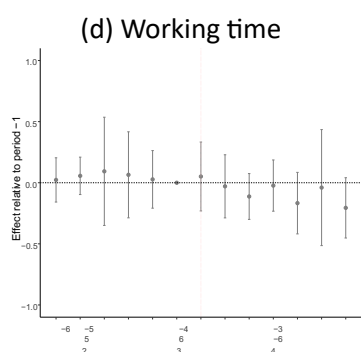




Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of employees})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments notified exclusively for violations related to occupational health and safety; Panel B to those notified exclusively for violations related to informality; Panel C to those notified exclusively for violations related to remuneration; Panel D to those notified exclusively for violations related to working hours; and Panel E to those notified exclusively for violations related to irregularities in social contributions. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

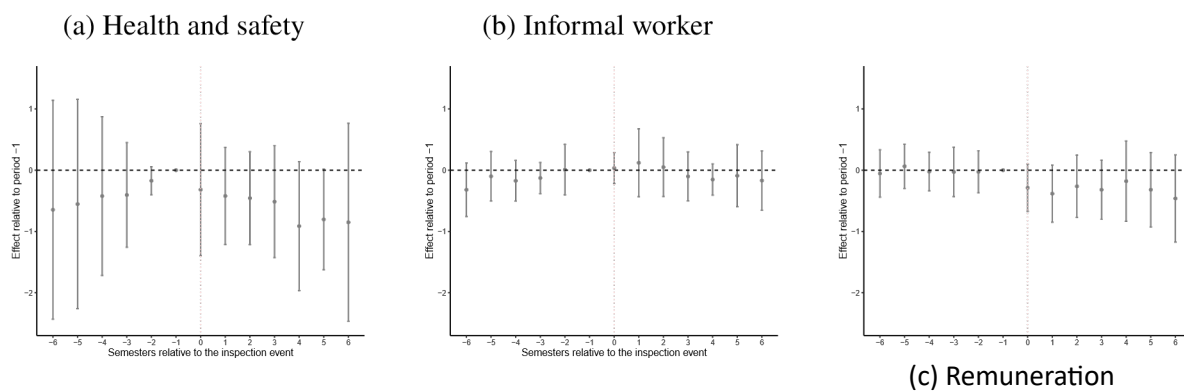
Figure O.12: Potential Mechanisms - Type of Infraction: The Effects of Inspection on Ln(number of hirings)

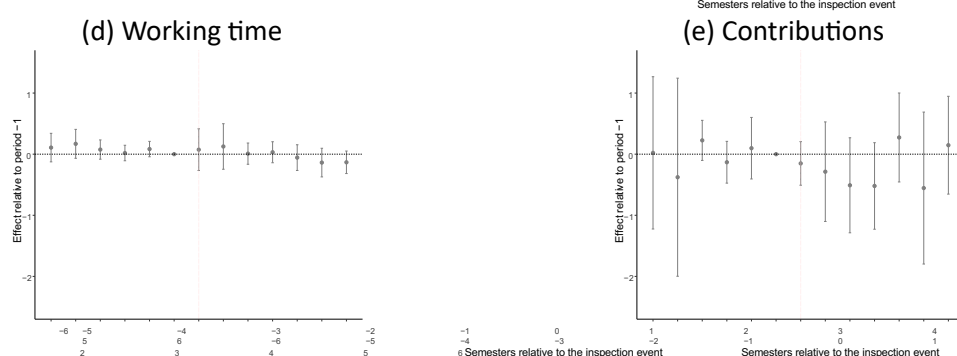




Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of hirings})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments notified exclusively for violations related to occupational health and safety; Panel B to those notified exclusively for violations related to informality; Panel C to those notified exclusively for violations related to remuneration; Panel D to those notified exclusively for violations related to working hours; and Panel E to those notified exclusively for violations related to irregularities in social contributions. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

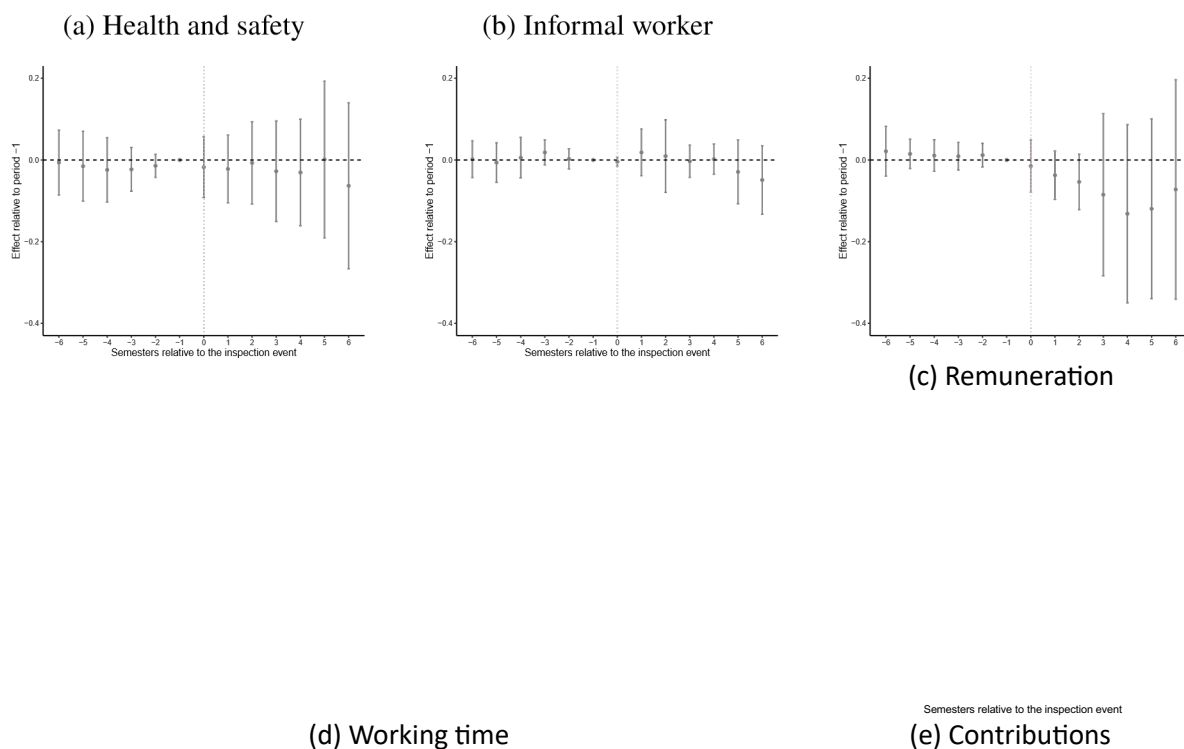
Figure O.13: Potential Mechanisms - Type of Infraction: The Effects of Inspection on Ln(number of separation)

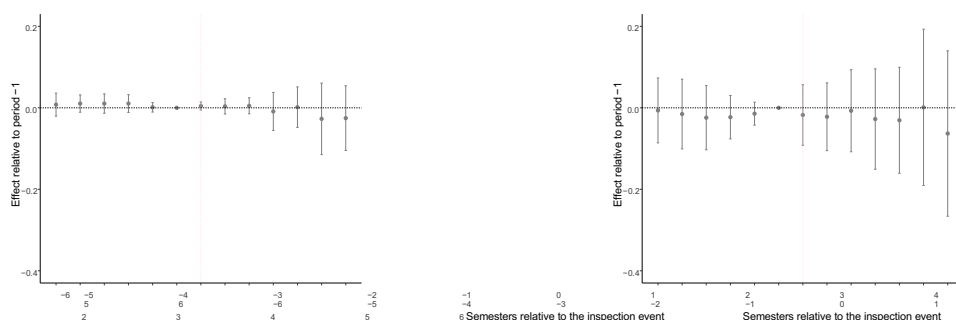




Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of separations})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments notified exclusively for violations related to occupational health and safety; Panel B to those notified exclusively for violations related to informality; Panel C to those notified exclusively for violations related to remuneration; Panel D to those notified exclusively for violations related to working hours; and Panel E to those notified exclusively for violations related to irregularities in social contributions. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

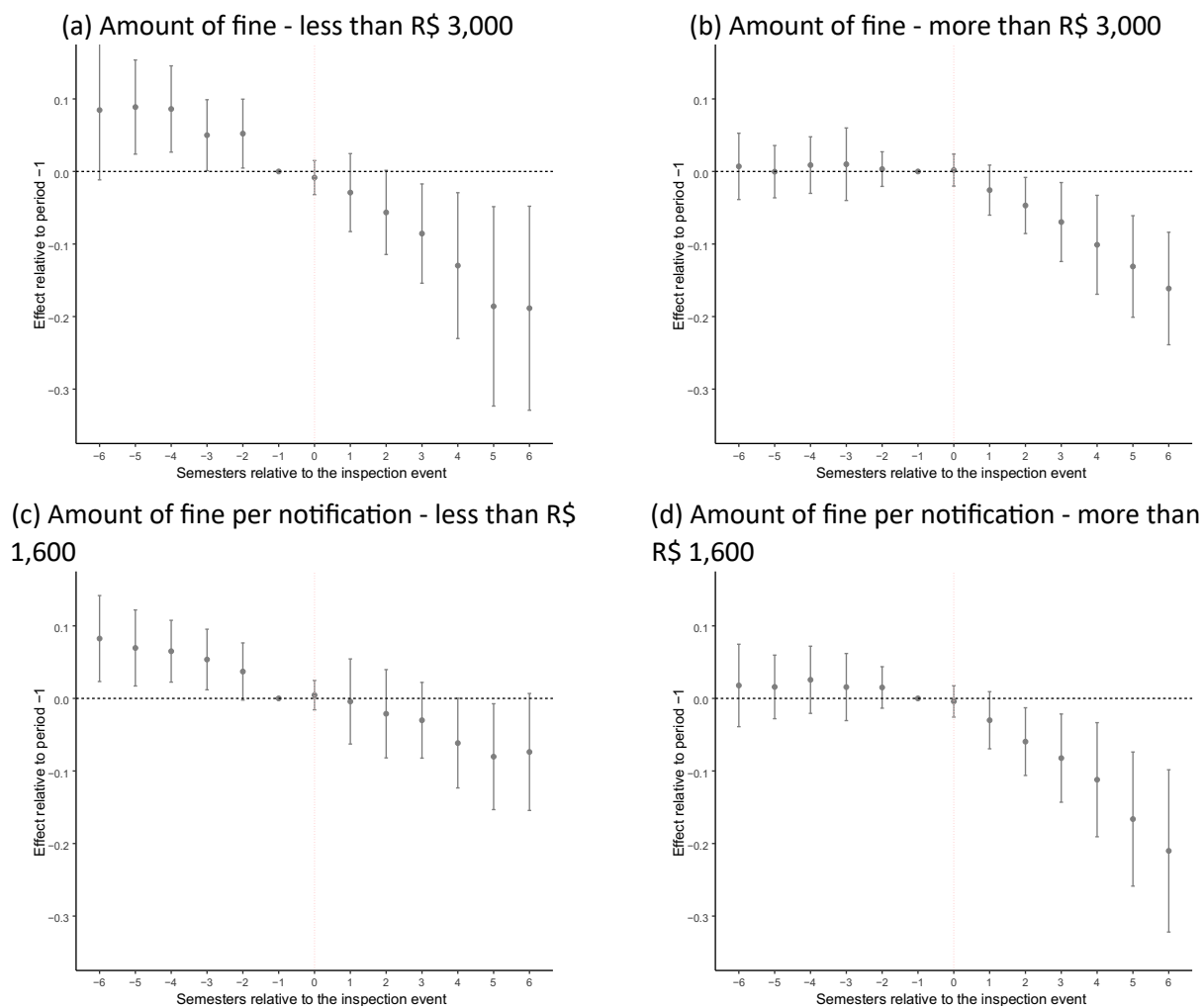
Figure O.14: Potential Mechanisms - Type of Infraction: The Effects of Inspection on $\ln(\text{wages})$





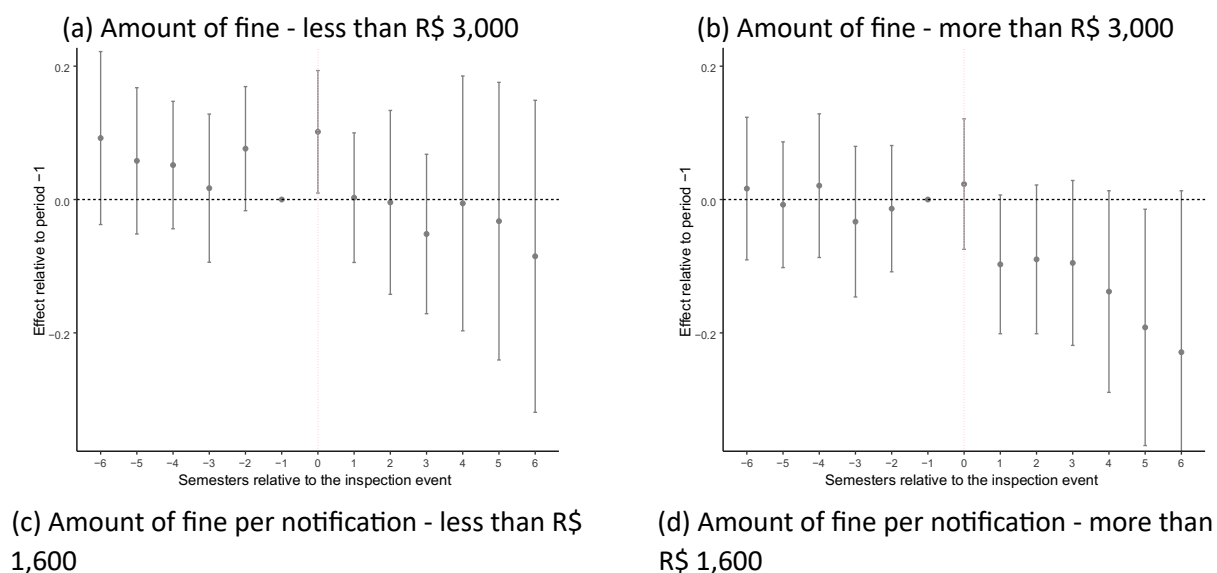
Note: This figure reports point estimates of the effects of inspection on $\ln(wages)$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments notified exclusively for violations related to occupational health and safety; Panel B to those notified exclusively for violations related to informality; Panel C to those notified exclusively for violations related to remuneration; Panel D to those notified exclusively for violations related to working hours; and Panel E to those notified exclusively for violations related to irregularities in social contributions. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

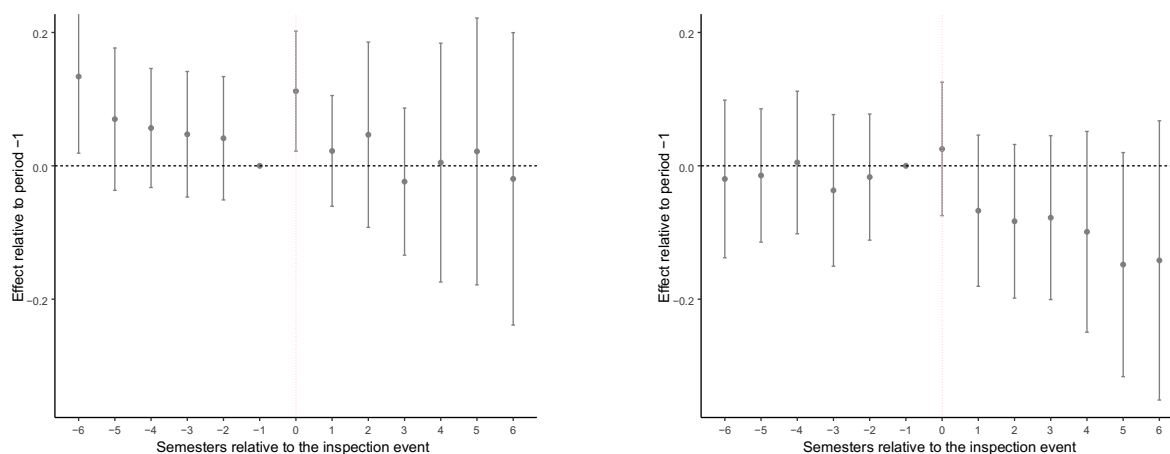
Figure O.15: Potential Mechanisms - Severity of the Fine: The Effects of Inspection on Ln(number of employees)



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of employees})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received fines of up to R\$3,000; Panel B refers to establishments that received fines above R\$3,000; Panel C refers to establishments with an average fine per infraction of up to R\$1,600; and Panel D refers to establishments with an average fine per infraction above R\$1,600. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure O.16: Potential Mechanisms - Severity of the Fine: The Effects of Inspection on $\ln(\text{number of hirings})$



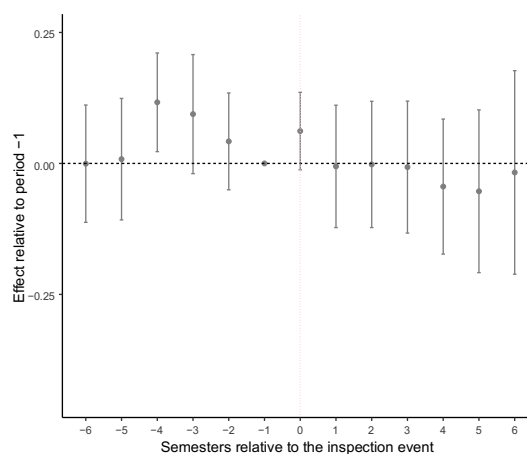
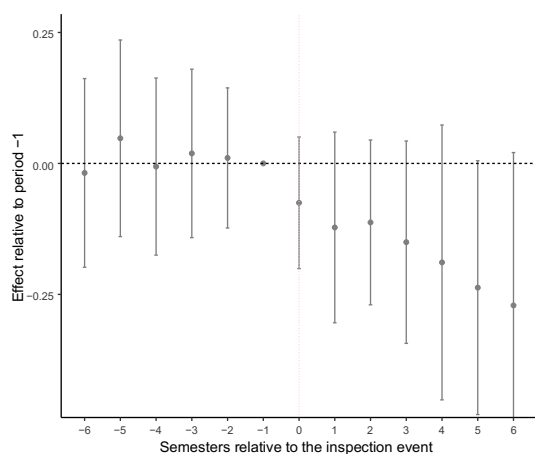


Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of hirings})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received fines of up to R\$3,000; Panel B refers to establishments that received fines above R\$3,000; Panel C refers to establishments with an average fine per infraction of up to R\$1,600; and Panel D refers to establishments with an average fine per infraction above R\$1,600. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

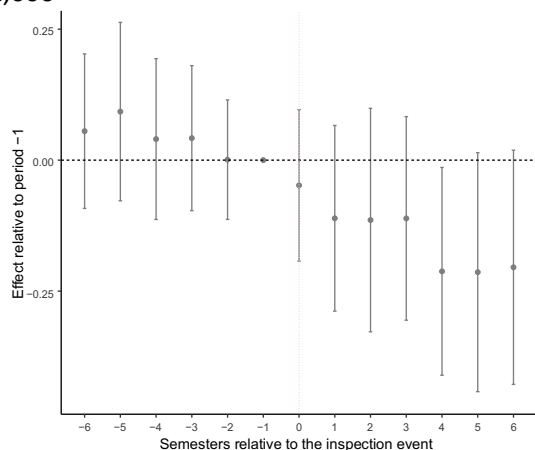
Figure O.17: Potential Mechanisms - Severity of the Fine: The Effects of Inspection on $\ln(\text{number of separation})$

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

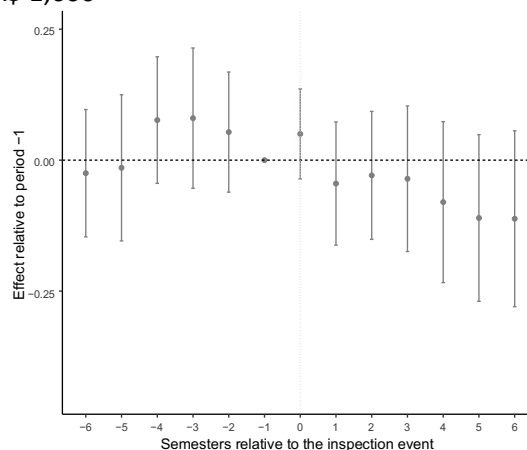
(b) Amount of fine - more than R\$ 3,000



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

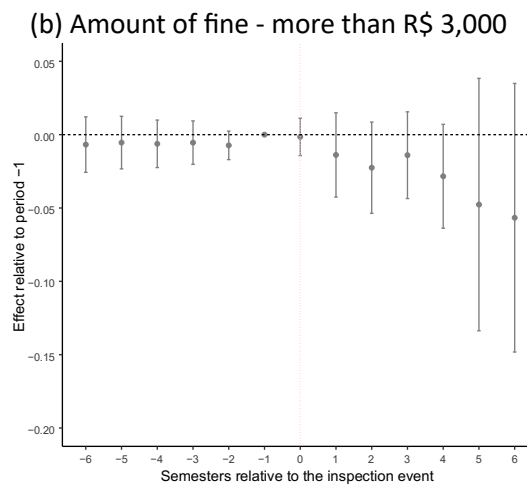
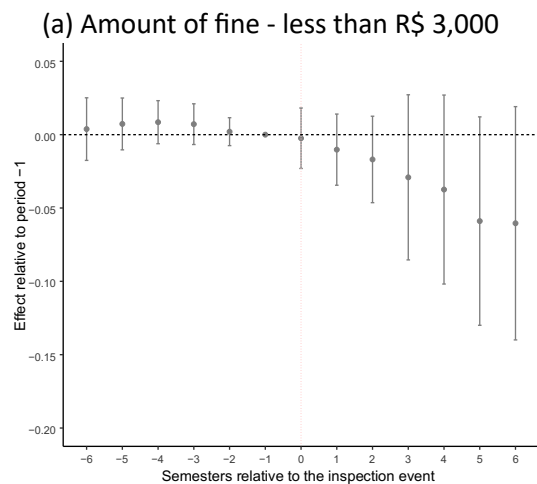


(d) Amount of fine per notification - more than R\$ 1,600



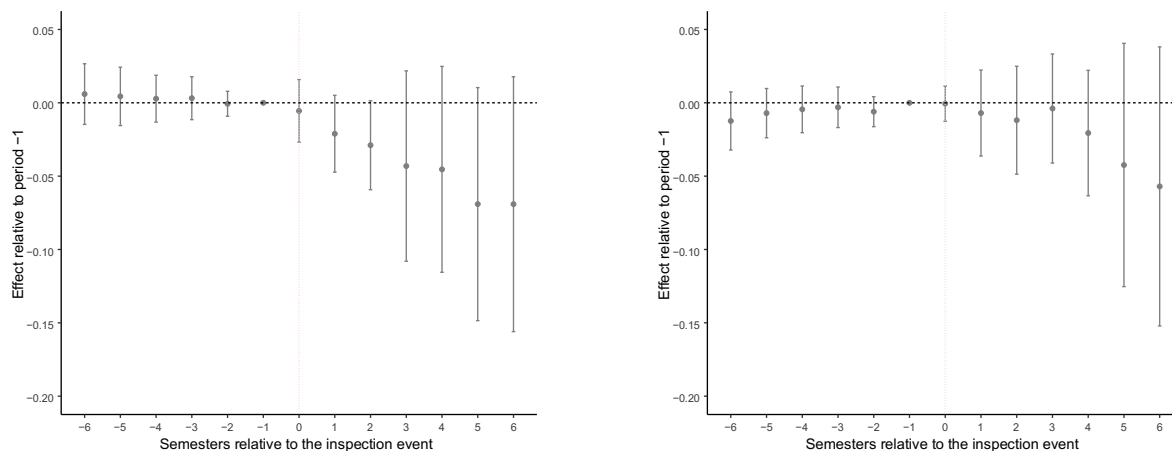
Note: This figure reports point estimates of the effects of inspection on $\ln(\text{number of separations})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received fines of up to R\$3,000; Panel B refers to establishments that received fines above R\$3,000; Panel C refers to establishments with an average fine per infraction of up to R\$1,600; and Panel D refers to establishments with an average fine per infraction above R\$1,600. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure O.18: Potential Mechanisms - Severity of the Fine: The Effects of Inspection on Ln(wages)



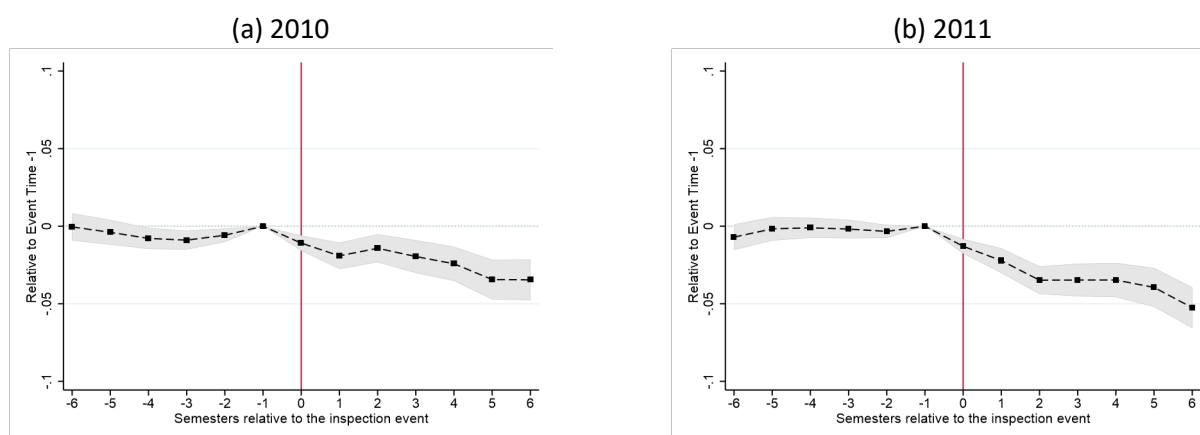
(c) Amount of fine per notification - less than R\$ 1,600

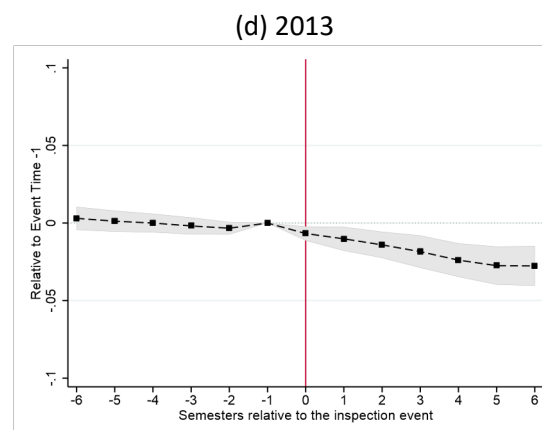
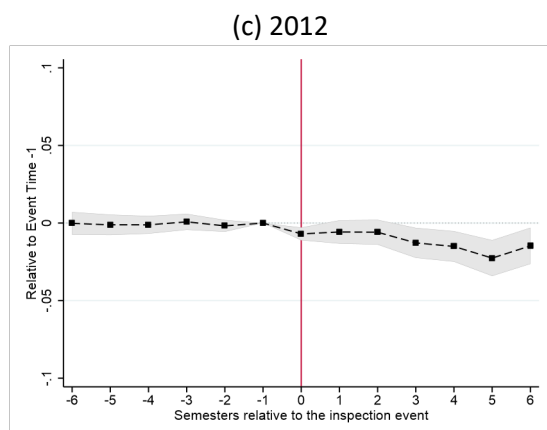
(d) Amount of fine per notification - more than R\$ 1,600



Note: This figure reports point estimates of the effects of inspection on $\ln(\text{wages})$ using the establishment-level sample from RAIS matched with inspection data. Panel A refers to establishments that received fines of up to R\$3,000; Panel B refers to establishments that received fines above R\$3,000; Panel C refers to establishments with an average fine per infraction of up to R\$1,600; and Panel D refers to establishments with an average fine per infraction above R\$1,600. The omitted category is the year before the event. 95% confidence interval based on standard errors clustered at the establishment level.

Figure O.19: The Effects of Inspections on Workers' Earnings for 2010–2013





Note: This figure reports point estimates of the effects of inspection on earnings variables using the worker-level sample from RAIS data. The control group consists of workers who, during the analysis period, were not employed at firms that were inspected. Panel A includes workers treated in 2010, Panel B those treated in 2011, Panel C in 2012, and Panel D in 2013. Each panel includes only the corresponding treated cohort. The omitted category is the semester before the event. 95% confidence interval based on standard errors clustered at the worker level.