Labor Inspections, Firm Adjustments and Employee Costs

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Abstract

Labor inspection plays a crucial role in addressing the high prevalence of the informal sector and the low compliance with labor regulations in developing countries. However, while inspections can lead to increase in labor costs, there is limited evidence regarding their effects on worker outcomes. In this study, we combine employer-employee matched data with establishment-level data on labor inspections in Brazil to estimate the impact of these inspections on worker earnings trajectories in the years following an inspection. Our findings indicate persistent, negative wage effects lasting up to four years after inspections. Workers in inspected establishments are also more likely to leave their jobs following an inspection. We find that these negative wage effects are driven both by firm stayers, consistent with within-firm adjustments, and firm-leavers, suggesting reallocation costs. The type of violations is important with severance payments violations having a more significant negative impact than formalization violations. We examine potential channels to explain this difference and find that the firm's cost pass-through, the amenities associated with the regulation, and the wage spillover effects resulting from the firm's pay policies may play a role.

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1 Introduction

Two major challenges in labor markets of developing countries are the large informal sector and low compliance with labor regulations. Globally, around 61% of the workforce is informal, with the figure reaching 93% in developing countries. Even when firms hire workers formally, compliance with labor regulations is often lacking due to complex rules and inadequate enforcement. For example, in Argentina, only 53% of formal firms comply with social security regulations (Ronconi, 2010). To address these issues, governments employ labor inspections, sending officials to monitor compliance, issue fines and conduct follow-ups to prevent recidivism. In their 2019 Centenary Declaration, the ILO highlights the importance of labor inspection in "promoting working conditions and enforcing legal provisions". Despite being an important toolkit in the governments' arsenal, recent studies show that they may have unintended negative effects on firms such as reducing employment levels and revenue growth. However, while the literature has mostly looked at firm outcomes, there is still scarce evidence on the effects on workers. Thus, understanding how labor inspections affects workers' outcomes is crucial to evaluate their overall effectiveness, especially if they inadvertently harm the very individuals they were meant to protect.

In this study, we aim to fill this gap by shedding light on how labor inspections affect workers' labor market outcomes in Brazil. First, we supplement the literature by providing context of how firms respond to inspections by investigating the effects on firm outcomes. Then, with a better understanding of firms responses, we turn to the main question of the study in which we investigate the effects of inspections on employee outcomes. In addition, we explore the mechanisms that drive our findings, particularly whether the effects are driven by firm adjustments or employee reallocation costs associated with job displacement.

To answer the questions posed above, we utilize two administrative datasets obtained from the Brazilian government. First, we use inspection-level dataset from the Ministry of Labor, which provides a list of formal establishments that were inspected by the ministry along with detailed information described in the inspection reports[1]. Crucially, we have the time of the release of the inspection report, which we use to identify the year of inspection, and information on the type of infractions found in each inspection. We then merge with a second administrative dataset, the employer-employee matched dataset of Brazil, RAIS, using the firm identifiers provided in both datasets. The RAIS dataset covers the universe of all formal contracts assembled yearly by the Ministry of Labor, and contains detailed worker and contract level information such as wages, date of admission and separation, contract type, hours worked and worker demographics.

[1]For clarity, while we have data at the establishment level for both datasets, we will use the terms "establishment" and "firms" interchangably throughout this paper

It is important to note that our analysis focuses exclusively on formal establishments, which is important in its own right. First, most inspections worldwide are targeted at formal establishments since they are easier to locate. Second, while a large portion of informal employment is located at the informal sector, formal establishments also hire a significant share of informal workers. For instance, Ulyssea (2018) shows using survey dataset that around 40% of informal employees in Brazil work in small formal firms, while 52% of informal employment is found in firms with at least 11 employees.

For identification, we employ a matched difference-in-differences strategy in which we match inspected firms with control firms, and compare the trajectory of outcomes for both groups. As control firms, we use those inspected at a future date. For the worker-level analyses, we obtain a list of workers who were employed at these firms the year before inspection and follow their labor market outcomes regardless of where they work after the inspection. Our analysis reveals that being caught with a labor violation is associated with large costs to the firm. We find that 4 years after inspection, inspected firms reduce their employment size by around 15% and average wages by 1%, relative to their control group.

Given the large decline in employment implying large costs on firms due to inspections, a natural question that arises is how inspections affect workers' outcomes. Using our worker-level analyses, we document negative, persistent costs on workers. Our findings show that workers employed in inspected firms in the year before inspection earn less wages after inspection, relative to workers who were working in the control firm the year before inspection and that these negative effects are persistent 4 years after inspection. Furthermore, workers of inspected firms are more likely to leave the firm and work less than 12 months of full time work in the years after inspection.

Leveraging information on types of violation, we examine whether the effects on wages vary by the type of regulation violated. Our findings indicate that the type of infraction matters, with inspections that found informal employees hired having non-significant effects on wages. In contrast, inspections that found violations in *Fundo de Garantia do Tempo de Serviço* (FGTS) severance payments induce significant, negative effects on wages.

Next, we investigate whether there are any distributional consequences on wages across different subgroups of workers by examining heterogeneous impacts by worker characteristics. Surprisingly, college educated workers are more negatively impacted than their lower educated counterparts. Moreover, workers who are at the top tercile of baseline wage distribution are also negatively impacted, while workers at the bottom tercile actually experience wage increase. To explain this finding, we consider one important labor market institution in Brazil, the minimum wage. If firms are constrained by minimum wage, wages can't drop even more. Indeed, we find that the negative effects are mainly driven by workers earning

significantly higher than the minimum wage. Furthermore, we also find the negative effects of inspections are largely concentrated in firms with small share of minimum wage workers, suggesting the importance of the minimum wage as a constraint when setting wages.

The estimated post-inspections wage results are consistent with two potential channels. First, the decline in wages may stem from firms adjusting their wage structure. This adjustment may be due to either costs pass-through or compensating differentials linked to provision of amenity resulting from increased compliance. Second, the observed wage decrease may also reflect reallocation costs incurred when workers transition to new jobs after being displaced from inspected firms.

To examine whether the reduction in wages is due to within-firm changes in compensation, we focus on employees who remain in the firms. The regression results from this sample show that wages decline for stayers, suggesting that within-firm adjustment in compensation is one channel that can explain the observed wage changes. A perfectly competitive labor market framework is inconsistent with the wage decline since firm-specific shocks, such as inspections, should not affect wages that is set competitively in the market. To help rationalize this finding, we build an imperfect competition, wage-posting model in which firms have labor market power akin to Card et al. (2018) incorporating amenity provision and inspection. This model successfully predicts lower wages for firm stayers after inspections.

Focusing on two types of infraction - informality and severance payment - we find that the wage drop is mainly driven by inspections that caught severance payment violation. On the other hand, the effect on wages for firms that are caught with informality infraction is significant and positive. We show that both of these types of violations are crucial; inspections that do not uncover formalization infractions show no positive wage effects, while those that do not catch severance payment violations exhibit no wage declines.

While the positive effects for informality violation is initially surprising, we provide suggestive evidence indicating that spillover effects to workers around minimum wage due to hiring of informal workers and reorganization of the firm which induces reallocation of tasks could explain the increase. Turning to the decline of wages for severance payment violators, we show evidence that both increased amenities and costs pass-through is consistent with the observed within-firm decline in wages.

Meanwhile, to test whether job displacement and reallocation costs can also account for the observed wage decline in our main results, we focus on the sample of workers who left the inspected firms. Our findings show that individuals who leave the inspected firms involuntarily also suffer wage losses, while individuals who leave voluntarily experience a wage increase. Notably, involuntary leavers who switch occupations are more adversely affected than those who remain in the same occupation. Meanwhile, switching sectors does not appear

to have a significant differential impact, as both groups—those who switch sectors and those who do not—experience wage declines following the inspection.

This paper adds broadly to the literature on the effects of labor regulations in developing countries. Early studies have documented the negative impacts of stringent labor regulations on labor market outcomes (Botero et al., 2004; Besley and Burgess, 2004; Freeman, 2010). However, recent research has increasingly emphasized the importance of effective or *de facto* regulations, taking into account the enforcement of these regulations (Kanbur and Ronconi, 2018; Ronconi, 2015; Basu, Chau, and Kanbur, 2010)[2]. This is especially important in developing countries, where enforcement and state capacity is often lacking. Our study contributes to the literature by examining the impact of strengthening *de facto* regulations through inspections, holding constant the *de jure* regulations or letter of the law.

Recently, the literature on the impacts of labor inspections have been growing, but has mostly focused on labor market level and firm-level outcomes. At the municipality level, Almeida and Carneiro (2012) finds that formal employment at the municipality level rises with more enforcement while Ponczek and Ulyssea (2022) shows that labor inspections reduce the ability of low-skill workers to cope with trade shocks. More related to our work, Brotherhood et al. (2019) and Prado, Santos, and Van Doornik (2024) examine firm-level outcomes, finding that inspections result in a decline in firm size. Furthermore, using loan-level data, Prado, Santos, and Van Doornik (2024) also shows that inspected firms face financial distress and difficulties in getting new loans. A key distinction between our paper and theirs is that they focus exclusively on inspections with formalization infractions, while we study other kinds of violations, particularly the FGTS severance payment violations.

Furthermore, relative to the literature's focus on municipality and firm-level outcomes, our main contribution lies in our examination of the effects on worker-level outcomes in the short and long run. The worker-level analysis provides valuable insights for policymakers seeking a comprehensive evaluation of inspections by highlighting the potential trade-offs associated with inspections. Specifically, our study indicates that inspections can increase worker turnover, reducing the coverage of workers that can benefit from the inspections, and reduce wages for those who stay.

One advantage of worker-level analyses is that we focus on employees who were present before the inspection, eliminating the composition bias often found in firm-level studies that arises from changes in the types of workers at a firm before and after inspections. Our setting also allows us to examine the effects for both workers who remain in the firms as well those who are displaced from the inspected firms. This is an advantage over firm level analyses

[2]Kanbur and Ronconi (2018) shows that controlling for enforcement levels, the negative correlation between labor regulations and labor market outcomes does not hold anymore.

which only allow the effects for workers who are present in the inspected firms.

To our knowledge, the only other paper examining worker-level outcomes is Parra and Fernández Bujanda (2024), which finds short-term wage increases for incumbents in Mexico that fade after six months and lower starting wages for new hires post-inspection. Our paper complements this study by furnishing the long term effects of incumbent workers, regardless of where they work, allowing for a thorough analyses of the mechanisms involved by investigating within-firm adjustments and reallocation costs. Furthermore, we also provide new evidence of the distributional consequences of inspection, particularly the interaction between inspections and another labor market institution, the minimum wage.

Second, we contribute to the literature on informality in developing countries[3]. Most studies focus on firms' decisions to operate in the informal or formal sector, that is, whether to comply with regulations and tax(Rauch, 1991; Meghir, Narita, and Robin, 2015). One key exception is Ulyssea (2018), which differentiated the extensive margin of informality - decision of firms to operate informally or formally, and the intensive margin of informality - decision of *formal* firms to hire workers informally. Our analyses relate to the intensive margin of informality since all the establishments in our sample are formal, and we contribute to this literature by examining the effects of higher informality enforcement on worker outcomes.

Third, we contribute to the literature on firms' responses to firm-specific shocks, and the implications on workers. The type of firm-specific shocks studied varies from increased enforcement (Boudreau, 2024, Szerman, 2024), negative trade shocks (Garin and Silvério, 2024), cash windfalls (Garin and Silvério, 2024), and demand shocks (Kline et al., 2019). Responses differ by shock type, with demand shocks found to affect employment and wages, while increase in costs due to enforcement having mixed evidence on wages. We provide new evidence that firm-specific shocks on costs can impact both employment and wages. Furthermore, a general consensus in this literature is the inability of perfect competition models of the labor market to explain firm-specific shocks affecting wages, and necessitates a model of imperfect competition. Our study aligns with this consensus by presenting a conceptual framework in which an imperfect competition model with inspection and amenity can rationalize wage declines after inspections.

The rest of this paper proceeds as follows. Second 2 provides the institutional background of the Brazilian labor market and labor inspections. Section 3 describes the data utilized in this project. Then, we describe the empirical strategy employed in this study in section 4. We start with the establishment-level analyses in section 5. Then we turn to the main crux of our paper, the worker-level analyses in section 6. Lastly, we conclude with section 7.

[3]See Ulyssea (2020) for an overview of the state informal sector in developing economies, along with the causes, consequences of informality and the different toolkits to tackle informality

2 Institutional Context

The Brazilian law establishes that every employee must hold a labor contract, which in Brazil is defined by having a booklet (*Carteira de Trabalho*) that registers workers' entire employment history in the formal sector. Upon hiring someone, employers must sign the booklet including general information about the job[4], and report the hiring to the government. A registered worker, conventionally called a formal worker, is entitled to several benefits, such as paid vacations, 120 days of maternity leave, and 50% overtime premium for hours worked in excess of 44 hours a week. Additionally, employers need to comply with the minimum wage laws and to make monthly payroll contributions of 20% to Social Security and 8% towards workers' seniority accounts (*FGTS*).

The *FGTS* is a fund created to provide a financial safeguard for employees who go through an unemployment shock. In practice, it works as a severance payment individual account. Workers roughly accumulate a one-month salary for each year worked and usually cannot have access to their full balance unless they are fired without cause[5]. From the perspective of employers, the cost of firing a worker increases with the contract duration. Upon firing someone, employers must give the laid-off worker a 40% severance pay "fine" of the amount of the balance accumulated over the worker's tenure on that specific job. A worker fired without cause must also receive a one-month advance notice and is eligible for unemployment insurance depending on her tenure[6].

Given that the rigidity of the labor code creates incentives for non-compliance, the government promotes labor inspections as the main instrument to ensure that firm compliance with the regulations. The Ministry of Labor is in charge of organizing the inspection plans, and enforcement is decentralized at the state (*Superintendência Regional do Trabalho e do Emprego - SRTE*) and local levels (*Gerências Regionais do Trabalho e do Emprego - GRTE* and *Agências Regionais - AR*). The SRTE is located in every state capital, and the number of GRTE or AR depends on the size and economic relevance of the state. The catchment area of each of these units covers a group of municipalities, and inspectors travel by car from their base city to the cities where the firms are located (Almeida and Carneiro, 2012).

One of the main focuses of the inspections is collecting the FGTS contributions, but once inspectors visit the firms, they check compliance with other aspects of the labor code, such as worker registration, minimum wage regulations, severance payment, overtime limits,

[4]Date of admission, wage, occupation, and the employer register number.

[5]The only other conditions to have access to the FGTS balance are purchases of residential property, serious health shocks, natural disasters, staying three years without a formal job, or retirement.

[6]In the analyzed period, workers were eligible for 3, 4 or 5 monthly paid benefits if they had respectively 6 to 11, 12 to 23, or more than 24 months of accumulated tenure including a one-month advance notice period. It is worth noting that the UI in Brazil is not experience rating.

and others. The Ministry of Labor, in collaboration with the SRTE, selects the list of firms to be inspected and establishes some targets for the overall FGTS collection. Firms can be selected in two different ways: random selection or anonymous tips (Cardoso and Lage, 2005). The government does not report the share in each of these groups, but considering that the system operates at capacity and the probability of being inspected is low for a high share of the firms, it is likely that some prioritization mechanism should be in place even if the government were to follow only the anonymous tips. To illustrate, in 2011, the number of inspectors for 1000 firms was 0.98 (3541200 firms and 3502 inspectors), and the unconditional probability of being inspected was 5.02%.

The low probability of inspections hides the fact that enforcement is size-dependent and increases with firm size. This pattern is not unique to the Brazilian context. It has been widely used to model informality and is also observed in the tax context for many developing countries (Bachas, Jaef, and Jensen, 2019; Ulyssea, 2018; Meghir, Narita, and Robin, 2015). Figure 1 presents the probability of inspection size-gradient separately by firms that were inspected before and firms that were not. We document two important findings. First, there is a high variation in the inspection probability. Focusing on firms never inspected before, the yearly probability of being inspected varies from 1.3% to 50.3% for firms up to 5 and more than 1000 employees, respectively. Second, even conditional on the size, firms that were inspected always present a higher probability of being inspected than firms never inspected before, which indicates some additional deterrence mechanisms.

One limitation of labor inspections is that they mainly focus on non-compliance with the labor law in formal firms. Targeting informal firms is harder since, by definition, there are no records of unregistered firms. We use the PNAD household survey from 2011 to 2013 to estimate the share of workers directly affected by the labor inspections. Figure A1 presents some descriptive statistics of the firms' and workers' informality by firm size. First, panel A1a shows that the share of formal firms is increasing in the firm size and approaches one very fast, with more than 95% of the firms with more than 5 employees having a registry. The share of formal workers is also increasing in the firm size as presented in panel A1b, but with a lower gradient. For example, firms with 11 or more employees still present around 9% of informal workers. Importantly, even though the share of informal workers decreases with the firm size since the worker's distribution is tilted towards larger firms, firms with more than 5 employees employ more than 50% of all informal workers. This implies that a high share of informal workers work in formal firms, as also concluded by (Ulyssea, 2018). Furthermore, even if workers are registered, they may not receive the benefits they are eligible for. For example, around R$42 billion of FGTS severance payments are owed by registered firms, affecting 6.2 million workers.

3 Data

Labor Inspections Data. The main dataset that we use is the labor inspection dataset obtained from the Labor Inspection Office (*Secretaria de Inspecao do Trabalho*) of the Ministry of Labor. This dataset comprises of a summary of all labor inspections of formal establishments conducted by the Ministry of Labor since 1995. For each inspection, we have information on firm identifiers, and the month-year in which the inspection reports were released, which we use as to identify the year of inspection. Furthermore, the data also contains the type of labor infractions found such as working hours, FGTS, formalization, hours of rest, salary, child labor, etc. Table A1 presents the counts of inspection by infraction type[7]. In our analyses, we examine inspections which find at least one type of violation as well as focus on formalization and FGTS violations separately.

Matched Employer-Employee Data, RAIS. The second dataset we use is an administrative dataset covering the universe of all formal workers and firms of Brazil, the *Relação Anual de Informações Sociais* (RAIS), from 2007 to 2019. RAIS, assembled yearly by the Ministry of Labor, is the main source of formal labor market data in Brazil. It is a high-quality census of the Brazilian formal labor market that comprises detailed contractual information on 76.1 million contracts of registered workers and 3.9 million in registered firms. Each observation represents an employment contract between a firm and a worker and contains information on monthly wage, age, gender, race, education level, sector, occupation, the month of admission and separation, establishment size, and location. Importantly, the firm identifier in RAIS is the same as the firm identifier in the labor inspections data, allowing us to link the two datasets together.

As this dataset only covers formal workers working in formal establishments, one main limitation of this dataset is that we do not observe those individuals who are unemployed or working at informal firms. Furthermore, important for this project, we also do not observe unregistered workers who are working at formal firms.

Other Data Sources. To complement our analyses, we also use other miscellaneous datasets such as the minimum wages data from the Institute for Applied Economic Research (IPEA), as well as the household survey data, the PNAD. The PNAD, which is collected by *Instituto Brasileiro de Geografia e Estastiscta* (IBGE), is a detailed survey that contains labor market information at the individual level. We turn to the PNAD to compute the average wage of the informal sector since the RAIS dataset does not contain information on the informal sector.

[7]Note an inspection may catch more than one type of violations. Therefore, the percentages in this table will add up to more than 1

4 Empirical Strategy

This section describes the matched difference-in-differences research design utilized in this project to estimate the impact of labor inspections on establishment-level and worker-level outcomes. We implement a matched, difference-in-differences design to address the main econometric issue of the selection of establishments into inspections. As previously explained, inspections are not random with inspectors reacting to anonymous tips. If anonymous tips are mostly sent about larger establishments or incompliant establishments, then inspected establishments will be different to non inspected establishments. We try to deal with this problem in two ways. First, as control group, we only used establishments that are also inspected, but at a later date. Second, we use a matching procedure to ensure that the set of treated and controls in our analyses are comparable and serve as appropriate counterfactual groups. This matching strategy has been used in recent papers utilizing difference-in-differences research designs (Goldschmidt and Schmieder, 2017; Smith et al., 2019; Arnold, 2021).

This section goes as follows. First, we describe the matching procedure employed to construct the matched sample. Then, we describe the regression specifications that we implement using the sample constructed.

4.1 Matched Sample

To construct the matched sample of inspected and control establishment, we start with the establishment-level inspection reports data. The data encompasses all inspection reports for ever-inspected establishment, therefore we may find multiple duplicates of establishment across and within years. We deal with this in the following way: First, for establishments that have multiple inspection reports in a year, we collapse the data at the establishment-year level. For each type of violation we construct binary variables indicating whether at least one violation was found in a given year. For example, if all reports in a year for a given establishment indicate that the FGTS status is no violation, we code the new yearly variable to be none. If at least one of the reports in a year indicate irregularity for FGTS, we code the new variable to be irregular. Second, since we are interested in effects of first-time inspections and to avoid duplicated observations, for each establishment we only keep the earliest year of inspection report. With the above sample restrictions, we obtain a establishment level data of inspected establishments with information on earliest year of inspection, and indicators denoting whether a violation is found in a year for all infraction types.

We then merge the list of inspected establishments constructed above with a panel of establishment-year level data constructed from the RAIS dataset and perform the matching

procedure using this sample. We focus on inspected firms with more than 5 employees in the year before inspection. Note that all of the establishments in this sample were inspected. As treated establishments, we use establishments first inspected in 2011 to 2013 while as potential control establishments, we use establishments first inspected in 2018 or 2019. Since we follow establishments up to 4 years after inspections, the potential control establishments were never inspected during our period of interest. Specifically, for each establishment first inspected in 2011 to 2013, we select a potential control establishment that satisfies the following requirements: i) were first inspected in 2018 or 2019, ii) located in same state and operates in same sector, iii) have more than 5 employees in the year before inspection of treated establishment and iv) is in the same bins/intervals for employment growth, size, and total wage in the year prior to year of inspection of the treated establishment[8] [9]. For establishments with multiple matches, following Lagaras (Forthcoming) we choose which control establishment to keep by taking the highest propensity scores based on quadratic in employment, quadratic in total wage, share of workers above high school and micro-region. We then restrict to a balanced sample of establishments found in RAIS three years before to four years after the first inspection. In our analyses, we focus on firms with inspections finding at least one type of violation.

To construct the worker-level regression sample, we obtain all workers that were employed in the treated or control establishment of our main establishment-level sample in the year before inspection, $t-1$. Note that year of inspection here is the year of inspection of the treated establishment to which the control establishment is assigned to. From now on, when we say to the year of inspection, we refer to this definition. We then follow these workers over time, regardless whether they stay in the treated or control firm. We make several restrictions to the worker level data. First, we include only workers employed at the end of year before inspection, $t-1$, with full time contracts that are CLT, and with tenure of at least 1 year. For workers who were working in multiple full-time jobs at the end of the year, we keep workers whose main job is at the inspected/control establishment where main job is defined as the job which pays the highest wage[10] [11]. Second, we restrict to workers who

[8]Note that when matching on covariates, we use the covariates during the year before inspection of the *treated* establishments. For example, when matching a firm inspected in 2011, we obtain the baseline characteristics in 2010 of this firm and the potential control firms and match on these

[9]We construct 15 bins using the percentiles of the treated establishments values for employment size and total wage. Specifically, we calculate the deciles and for the highest decile only (above 90th percentile), we create additional quintiles culminating in 15 bins total. The reason for creating the additonal quintiles for the highest decile is the total wage and size of establishments in our data are right skewed with long tails therefore creating these additional bins allow us to get better matches for establishments in the right tail.

[10]CLT is the legislation that regulates the standard employee contracts in Brazil, which were described in the institutional background section. The exceptions to CLT are: public servant contracts and internship contracts.

[11]Recall RAIS is organized at the contract level, so we can observe all the contracts as well as establishments that a particular work at in the end of the year.

were found in RAIS i.e. formally employed, in all three years before the year of inspection therefore the set of workers in our sample is those that are strongly attached to the formal labor market. Lastly, we keep only workers aged 22 to 65 in the year prior to the inspection.

Following studies on displacement using the RAIS data, the analysis on this paper focuses mainly on workers who are continuously present in RAIS. This means that these workers are working formally in all the eight-year period of interest. Therefore, the estimates found in the main results of this paper may be an underestimate of the true effects of inspections on earnings if workers leave the formal sector to unemployment or informal employment. This is one limitation of this paper, as well as all other studies that use this dataset. To deal with this limitation, as robustness, in 6.1, we made assumptions to the earnings of individuals who dropped out of RAIS in the years after inspection and replace the earnings with zeros, wage in the informal sector or minimum wage. We estimate the impact of inspections on workers' wages using these assumptions.

A crucial advantage of the RAIS dataset is the level of granularity of the data that is presented at the job/contract level which allows us to observe the wages of all contracts worked in a year. Our main outcome variable exploits this data structure. Our main outcome variable is (log) average wages by taking the weighted average of the wages across all jobs worked in a year using number of months worked in the job in a year as weights. Some individuals may work in more than one establishment in a year. However, in our data, there are only a small number of workers working at multiple firms in a year[12]. Hence, for most of the workers, the average wage across all jobs in a year is the average wage at the main job.

4.2 Summary Statistics

Tabel 1 displays the summary statistics for inspected and control establishments. In our sample, we have around 12,200 inspected firms along with their associated control firms. Comparing the (log) size, total wages, mean wages and share of demographics between inspected and control establishments, we can see that there is no statistically significant difference between inspected and control establishments. Both inspected and control establishments employ on average around 18 workers. Our sample also have a comparable sector composition between inspected and control establishments with the firms in our sample dominated by the retail sector. One variable that the matching does not perform as well is share of male workers with a statistically significant difference between inspected and control groups. To deal with this lack of balance, we are going to include this variable in our regressions as a control variable.

[12]Around 10% of workers worked in multiple firms after inspection while this number is much lower, only around 1%, for workers who stay in the baseline firm after inspection

Turning to the worker-level regression sample, table 2 shows the summary statistics for workers working in inspected and control establishments in the year prior to inspection. Workers in our sample are slightly overpopulated with white, male workers with education above high school. Around 60% of workers in our sample are male, 40% of sample possess education below high school, and 70% are white. Furthermore, the average tenure in our sample is around 4.5 years. Comparing the differences in these variables between the inspected and control workers, there does seem to be some statistically significant differences between inspected and control workers. However, reassuringly the magnitude of most of the differences are quite small. To control for these differences, we include these variables in our regression specifications.

4.3 Empirical Specification

To estimate the effects of inspection on establishment-level outcomes, we use the matched sample at the establishment-level constructed above and implement a staggered difference-in-differences design. Our research design compares the trajectory of firm-level outcomes of inspected firms relative to the control firms before and after year of inspection. In particular, we estimate the following dynamic regression specification:

$$Y_{ft} = \sum_{r=-3, r\neq -1}^{4} \beta_r Inspected_f \mathbb{1}(t = t^* + r) + \delta X_f + \alpha_f + \alpha_t + \epsilon_{ft} \tag{1}$$

where Y_{ft} is an outcome variable of establishment f at year t, $Inspected_f$ is a dummy for an inspected establishment, $\mathbb{1}(t = t^* + r)$ are dummies indicating that the observation is r years away from the inspection year, t^*. For each control firm, we assign the year of inspection to be the inspection year of the treated firm it is matched to. Firm-level outcomes of interest are log of total employees, log of average wages, share of hires and share of termination. We include as controls establishment-level variables, X_f, share of male workers and employment growth by interacting these variables with year fixed effects. As part of X_f, we also include sector-by-year fixed effects as well as microregion-by-year fixed effects. α_f are firm-fixed effects to control for time-invariant firm unobservables while α_t are year fixed effects to control for common shocks affecting all establishments that may be changing over time. Coefficients of interests are β_r, which can be interpreted as the average difference in outcome between inspected and non-inspected establishment at relative time r, relative to the omitted time period, -1. Note that the time period zero is the period when establishments are inspected, so establishments are partially treated in this period. Standard errors are clustered at the establishment level.

To estimate the effects of inspection on labor market outcomes at the individual level, we compare the trajectory of worker-level outcome of workers who were previously working at inspected establishment with workers who were not working at inspected establishments, before and after year of inspection. We implement a similar regression specification as 1 but at the worker-level as follows:

$$Y_{ift} = \sum_{r=-3, r\neq -1}^{4} \beta_r Inspected_{if} \mathbb{1}(t = t^* + r) + \delta X_f + \gamma X_i + \alpha_i + \alpha_t + \epsilon_{ift} \tag{2}$$

where Y_{ift} is an outcome variable at time t of a worker i who was working at baseline establishment f at time t^*, $Inspected_{if}$ is dummy indicating that the observation is r years away from the inspection year, t^*. $\mathbb{1}(t = t^* + r)$ are dummies indicating an inspection occurred r years relative to the year of inspection t^*. As with the firm-level regression, X_f includes baseline firm level variables interacted with year fixed effects. X_i includes controls at the individual level such as age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, baseline wage and tenure. These were the variables that were found not to be balanced in the balance check, eventhough the difference is not large. α_i are worker fixed effects, while α_t are firm fixed effects. As outcomes, we consider the worker average wages (across all jobs in a year), separation probability, and probability of working less than 12 months in a year. The coefficients of interests are β_r, which indicates the difference in average worker level outcomes between inspected and non-inspected firms at relative period r, compared to the omitted time period, the year before inspection. Standard errors are clustered at the worker level.

The primary identifying assumption is the parallel-trends assumptions, which states that in the absence of inspection, the outcome trajectory of inspected and non-inspected establishment and workers would have evolve similarly or follows a parallel trend. Both specifications above are useful to assess the "parallel-trends" assumption of a difference-in-differences strategy and illustrate the effects of inspection over time. Furthermore, I also implement the following difference-in-differences specification to get the aggregate effect of inspections across all time periods post inspection:

$$Y_{ift} = \beta Inspected_{if} \times Post_t + \delta X_f + \gamma X_i + \alpha_i + \alpha_t + \epsilon_{ift} \tag{3}$$

where $Post_t$ is an indicator equaling 1 for all the years after an inspection.

Recently, there has been a burgeoning literature on the problems of using two-way fixed effects specifications with regards to staggered difference-in-differences designs in the presence of heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille, 2020;Goodman-

Bacon, 2021;Callaway and Sant'Anna, 2021). The main problem that arises is that a standard two-way fixed effects specifications may use already-treated groups as control groups. This may lead to negative weights when computing the average treatment effects, therefore the sign of the average treatment effect may be the opposite of cohort-specific, two-period difference-in-differences estimate. Our paper is not susceptible to these problems since our control group are never treated during the period of analyses so we do not have any comparison where we compare inspected firms with previously-inspected firms. This research design is similar to Cengiz et al. (2019) and have also been used recently in a matched difference-in-differences design (Lagaras, Forthcoming; Arnold, 2021). Note that as in Cengiz et al. (2019), we also interact the firm and time fixed effects with the group or inspection cohort dummies.

5 Establishment-Level Analysis

This section presents results on the effects of inspections on firm-level outcomes. The main outcome variables that we use are (log) number of employees and (log) average wages. These analyses serve as useful starting point in understanding how inspections affect establishments and provide context of what's happening at the establishment level before switching to the worker-level analyses. To understand what drives the effects on establishment size, we also look at share of hires and share of termination.

5.1 Results

First, we present results with (log) number of employees and (log) average wages as outcomes. Figure 2 displays the event study plots of running equation 1, with panel (a) showing results for number of employees and panel (b) showing results for average wages. We find no statistically significant difference in the trajectory of outcomes for both log employee and log average wage before inspection, supporting the parallel trends assumption and evidence that the matching procedure worked well, conditional on the controls included. We find that both total number of employees and average wages decline after inspection, with the effects persistent and growing over time. We find size decline gradually and 4 years after inspection, number of employees of inspected establishments decline by 15% relative to the control establishments. Average wage also falls gradually and 4 years after inspection, average wage of workers in inspected establishments declined by 1% relative to the control establishments.

Next, we investigate whether the decline in size is driven by increase in separation or decrease in hires. Panel (c) of Figure 2 displays event study coefficients with share of separation as outcome and panel (d) has share of hires as the outcome variables. These

shares are computed as a share of number of employees at baseline. Panel (c) suggests that establishments experience an increase in termination in the years immediately after inspection (year 0 and 1), relative to the control establishments, when terminations increase by around 4.5% on average. However, the share of terminations drop in the following periods and finally by year 4 after inspection, share of termination actually drops by less than 5%.

Likewise, hires also exhibit the same patterns as illustrated in panel (d). The increase in hire is more immediate and only in the year during inspection, when hires increase by around 6%, relative to the control establishments. In the following years, share of hires tend to decline, culminating in share of hires declining by around 8% 4 years after inspection.

Compared to recent papers on the impact of inspections in Brazil, we find similar decline in size post-inspection. However, the magnitude found in our paper is larger compared to the literature. One potential reason for this is because previous studies focus on inspections with at least formalization infractions, while our study includes all inspections that identify at least one infraction. Indeed when we limit to formalization infraction, we also find similar magnitude as the literature as reported in figure 3 panel (a). The smaller magnitude arises because establishments that are caught with formalization infraction have to hire formally those workers that they previously employ informally.

Indeed, when we examine the effects of inspection on separation and hires separately by whether the inspection finds formalization infraction or not in panel (b) of figure 3, we find a spike in hiring during the year of inspection only for establishments with formalization violations. On the other hand, establishments caught with violations other than formalization do not respond by increasing the hires. Meanwhile, panel (c) of figure 3 illustrates that separations increase for inspections with both formalization and without formalization infractions but the magnitudes are larger for firms with formalization infractions.

As previously explained, firms caught hiring unregistered workers are required to formalize these employees. Using the exact month of the inspection reports release and the date of admission of workers available in RAIS, we can identify workers hired exactly during inspection, who are likely to be workers previously informal and are now formalized due to the inspections. Table A3 compares the baseline characteristics of these plausible "informal" workers with workers hired 6 months before the inspections. The "informal" workers are more likely to be younger, less educated, part-time work and earn lower wages compared to the formal incumbents. Furthermore, we can also assess whether the spike in separations after inspections is driven by these "informal" workers. Figure A2 plots the share of "informal" workers among those who separate by time 0 and time 1, by treatment status. The share of "informal" workers among those separated by time 0 and time 1 is higher for inspected firms compared to the control firms, suggesting that inspected newly hired "informal" workers tend

to separate after they get hired.

5.2 Discussion

As noted earlier, these inspections are not random and there may be unobservables that are correlated with inspections. We try to deal with this by using a combination of matching procedure to obtain a set of firms that are as similar as possible and a difference-in-differences design. While a matching estimator essentially assumes a selection on observables assumption which may not be enough for clean causal statements, we argue we have a reasonable enough setting and findings to suggest that the estimates can be seen as causal.

First, our matching procedure works well in providing a balanced set of firms in terms of their observable characteristics and as reflected in the lack of pre-trends. Second, in our regression specifications, we employ a multitude of fixed effects including firm fixed effects, sector-year fixed effects, and micro-region-year fixed effects ruling out time-variant sector or region shocks as drivers of our estimates. Any possible confounder would have to be firm-specific, changing over time (such as negative demand shocks that cause firms to downsize) and coinciding with the inspection year. Lastly, the exact and particular patterns of the outcomes obtained limit the possible omitted variables that could cause the same pattern. In particular, two striking patterns in support of a causal interpretation of inspections are the immediate spikes of hires and separations at time zero, and hires only increasing at time zero for firms with formalization infraction. It is unlikely that other omitted variables can induce an immediate spike in hires only for firms with formalization violations.

Since we use future-inspected firms as control group, the identification essentially boils down to the timing of inspections being random and uncorrelated with outcomes, conditional on fixed effects in equation 1 and the matching variables. As shown in figure 1, the probability of inspection for smaller firms (less than 20 employees) being inspected is quite small, less than 10%. Therefore, the assumption of random timing would be more appropriate for this group of firms. We repeat the benchmark specification on this subset of firms and find similar results to the main sample, as presented in figure A3.

One potential concern of using future inspected firms as the control group is that they may behave differently in the years leading up to their inspection, alerting the attention of the inspection office. For example, if they grow much faster in years leading up to inspection, they may not serve as appropriate counterfactual group for the treated group in later years. To address this concern, we present event study plots for log employee by inspection cohort in figure A4. Note that the 2011 cohort are only followed up to 2015, so the control group shouldn't have picked up growth yet since the control groups only start getting inspected

in 2018-2019. On the other hand, we follow the 2013 cohort up to 2017. For this cohort, their control group's growth may have already picked. Therefore, if this concern is valid, we should expect different estimates for these two cohorts. However, figure A4 shows that the coefficients across all three cohorts are very similar, alleviating the concern.

Another potential concern relates to firm exit due to inspection and sample selection. Figure A5 presents results from cross section regression with an indicator if firm i has zero employment at time t as the outcome and indicator being inspected as the main independent variable. To obtain the control group, we again use a matching procedure based on observables the year before inspection and the pool of control group are never inspected firms. Panel (a) of figure A5 presents results on exit rates of firms inspected with at least one infraction and the increasing trend across time suggests that inspections lead to an increase in exit rates. The negative coefficients, though surprising, is mechanical due to the treated group being forced to survive up to the month of inspection while this restriction is not present for the control group[13]. Panel (b) examines the exit rates of firms inspected during the 1st quarter of the calendar year, to remove this mechanical effect. As we can see, the negative coefficient is now much smaller closer to zero and is not statistically significant.

Recall that we restrict our sample to a balanced panel of firms that operate in all years of interest, resulting in a positively selected group of stable firms. In particular, the treated firms are more positively selected than the control group since they were still able to survive even after being inspected. Therefore, estimates derived from this sample are likely to underestimate the true impact of inspections. If instead we do not restrict to a balanced panel and allow treated group to exit, the control group is more positively selected resulting in an overestimate of the impact of inspection[14]. This allows us to use the results of regressions from both the balanced and unbalanced sample to bound the true estimate. Table A2 presents the results from both samples, with panel (a) showing results from balanced sample while panel (b) showing results from an unbalanced sample. We find a lower bound from the balanced panel of around 7% and an upper bound of around 15%. As such, the estimates we found so far in this paper are conservative estimates of the true effects of inspection.

6 Worker-Level Analysis

The findings of the previous section suggest that inspections exert costs to establishments. In light of these costs, a natural question that arises is how this increase in costs

[13]For example, a firm inspected in November of 2011 by definition has to survive until at least November 2011 while the corresponding control firm does not have this restriction.

[14]Note that we use future inspected firms as the control group, therefore by construction, they can't exit before being inspected

affect workers. In this section, we turn to how inspections affect labor market outcomes at the worker level, in particular wages, separation and total months of formal employment. First, we show that workers are adversely affected by inspections in terms of wages and months of employment in section 6.1. Then, we distinguish the negative effects of inspection on workers for those workers who stay (stayers) in the establishments and those who leave (leavers). This differentiation is key in understanding the mechanisms in which workers are affected by inspection. Looking at the effect on stayers allows us to examine within-firm adjustments in the compensation structure due to potentially amenity provision or cost pass-through. Meanwhile, analyzing the effects on leavers could shed light potential reallocation costs when workers are displaced from inspected establishments. Section 6.2 presents results for stayers while 6.3 shows results for workers who leave inspected establishments.

6.1 Baseline Results

First, we start with the overall impacts of inspections on workers' labor market outcomes. Figure 4 plots the coefficients of β_r from running equation 2 for log average wages in panel (a), probability of separation in panel (b) and probability of working less than 12 months of full time work in a year in panel (c). From panels a and c, we can see that there is no statistically significant difference in the trajectory of wages and months of formal employment in the pre-period, supporting the parallel trends assumption. Turning to the coefficients in the post period, panel (a) shows that there is a significant decline in wages for inspected workers relative to the control workers. Aggregating the point estimates in the post periods suggests a decline of wages by around 0.5%. We can also see that the effect on wages is gradually increasing over time, with wages dropping by more than 1% 4 years after inspection. This decline in wages due to inspections can be a result of workers being displaced and facing reallocation costs, or inspected firms changing their wage structure in response to the inspections and reducing wages for workers who stay. Which of these mechanisms are in play will be explored further in section 6.2 and 6.3.

Panel (b) of figure 4 illustrates that there is a higher degree of separation for workers working in inspected firms. On average, inspections increase probability of separation by 1.7pp for workers working in inspected firms relative to the control workers. This amounts to 8% of the mean of separation for the control group, which is around 21%. Lastly, panel (c) of figure 4 indicates that inspections lead to an increase in the probability of working less than 12 months of full time work. The effect is immediate, with the coefficient at time 0 (year of inspection) suggesting a 1.25pp increase in likelihood of working less than a full year of full time work. Again, the effect is also persistent over time until 4 years after inspection. Comparing with the mean of the control group which is around 25%, the effect

of the inspection amounts to 4% of the control group mean. Overall, panel (b) and (c) of figure 4 suggests that inspections are associated with job displacement and reallocation costs. Connecting the wages effects and these displacement effects, it is likely that part of the decline in wages after inspection is driven by these costs.

As previously discussed in section 4.1, one limitation of the RAIS data is we only observe individuals who stay in formal employment. If workers who exit formal employment experience larger wage losses, the wage effects obtained so far is an underestimate of the true effect. To gauge how large is this underestimate, we make several assumptions to the wages of individuals who dropped out of RAIS after the inspection: i) workers drop out to unemployment, ii) workers drop out to informal employment and earn average informal sector wages, and iii) workers who drop out earn the minimum wage. Panel A of Table 3 shows the aggregate difference-in-differences estimates of running equation 3 after making these assumptions. Column (1) presents results from the baseline sample, column (2) presents estimates where we replace wages of RAIS drop-outs with zeroes, column (3) replaces wages of drop-outs with average informal sector wages in the state, and column (4) replaces wages of drop-outs with the minimum wage. When we assume workers drop out to unemployment, the magnitude of the negative effect of inspections is larger, amounting to a 4% drop in wages compared to 0.46% in the baseline estimates. When we replace wages of drop-outs with the minimum wage, the magnitude of the negative effect of inspections is around 0.9% while assuming the average wage of the informal sector results in a drop in wages of around 0.7%.

We also ran an analysis where we use as outcome an indicator of dropping out formal employment in the years after inspection in at least one year. Panel B of table 3 reports the cross-section regression results indicating that inspections increase the probability of dropping out formal employment by 1.2pp. Overall, this suggests that focusing only on workers who stay in RAIS in the years following inspection would provide a lower bound on the adverse effects of inspections on workers

6.1.1 Heterogeneity

First, we present results on heterogeneity by type of regulation that were violated, focusing on formalization and FGTS violations. We focus on formalization infractions because this has been an area of interest in the literature. Meanwhile, the choice to focus on FGTS is due to FGTS violations being one of the main targets of inspectors and FGTS violations captures a different intensive margin of non-compliance, not paying payroll/severance contributions. Panel (a) of figure 5 reports the results for firms that violate *at least* formalization

regulation while panel (b) displays results for firms that violate *at least* FGTS regulations[15]. Note that the set of establishments in both of these samples are not mutually exclusive and an establishment with both formalization and FGTS violations would be included in both regressions. We see different effects for formalization infraction and FGTS infraction. Individuals that worked in a firm that violated FGTS regulations experience a significant and persistent drop in wages, with wages in the 4th year after inspection being more than 1% lower. On the other hand, the effects on for workers in formalization violators firms do not experience a significant drop in wages, except in the 4th year after inspection.

Next, we turn to heterogeneity by worker characteristics in table 4, shedding light on any distributional consequences of inspections. First, columns (1) and (2), which split the sample by tercile of baseline wage distribution, show that the negative effects are mainly driven by workers in the top tercile of baseline wage distribution, while the effect on workers in the bottom tercile is positive and statistically significant. Furthermore, when we look at effect on wages by education level in columns (3) - (4), we see that the magnitude is more negative for college educated than for workers with high school or below education. These findings may be surprising given that lower educated workers and workers in the lower end of wage distribution are usually more vulnerable to negative shocks.

To explain these findings, we turn to a key labor market institution, the minimum wage. Motivated by recent research which finds significant impact of minimum wages in Brazil (Haanwinckel and Soares, 2021; Derenoncourt et al., 2021), we explore how minimum wage transmits the effect of inspections to workers. First, we look at how close the wages of workers at year before inspection to the minimum wage in columns (5) and (6) of table 4. Column (5) displays results for workers with wages below 1.5 times the minimum wage, while column (6) presents the results for workers with wages above 1.5 times the minimum wage. The negative effects are driven by workers earning far above the minimum wage, while the effects for workers around the minimum wage is positive. A likely explanation is that the minimum wage creates a lower bound which prevents further wage reductions for workers earning around the minimum wage. This finding is consistent with the previous findings regarding education and wage distribution if the lower educated and low earning workers earn around the minimum wage.

Next, we explore another dimension of how minimum wage transmit the effects of inspection to workers by examining firm' wage structure in terms of the minimum wage. For each firm in our sample, we compute the share of workers who earn below 1.5 times the minimum wage in the year before inspection. Then, we split the sample according to the median of this

[15]Note that FGTS violations are related to formal workers. If a firm is found to have informal workers, but pay all of its FGTS payments to its formal workers, then the inspectors would not categorize as the firm having an FGTS violations even though in practice, it doesn't pay FGTS to the informal workers.

shares[16]. Column (7) presents results for firms below the median, showing that the negative effects are driven by this subsample of firms. Meanwhile, column (8) presents results for firms above the median and shows no statistically significant effect of inspection on wages. This may be because establishments with plenty of workers around the minimum wage are more binded by the minimum wage, therefore they have no room to adjust the wages downwards in response to the inspection. We also run other heterogeneity by gender, age, and tenure presented in appendix table A4, showing that the negative effects are mainly concentrated in higher tenure, female and older workers.

6.2 Effect on Stayers

The previous subsection documents the overall negative effects of inspection on workers outcomes. Two possible mechanism behind these effects are firms adjusting their wage structure, and reallocation costs when workers face displacement after inspection. This section explores the first explanation by focusing on workers who remain at the baseline establishment post-inspection. Following Arnold (2021), we restrict the analysis to stayers in both both the treated and control groups. This ensures the treatment group does not mechanically contain workers with more stable job histories and prevents the estimates to be mechanically driven by workers in the control group changing jobs if we were to also include workers in the control group who left the baseline establishment.

Note that whether an individual stays in the establishment could be influenced by the inspection. Therefore, we are susceptible to sample selection issues by conditioning on a future outcome. For example, if those who leave the establishment are workers who would face larger wage drops had they stayed, then the estimates we obtain in this section would be upward biased. Therefore, the estimates produced in this section should be interpreted with this caveat in mind. Nonetheless, the analyses of stayers are still informative in providing a deeper understanding of the impacts of inspections and whether within-establishment changes in the wage structure could be one potential mechanisms driving the effects.

6.2.1 Conceptual Framework

To fix ideas on how wages of stayers are affected by inspections related to compliance with amenity regulations, we extend the imperfect competition model of the labor market from Card et al. (2018) by incorporating amenity and inspection probability. We build a

[16]Firms below the median share of workers <1.5 minimum wage had low number of workers around the minimum wage at the year before inspection while establishments above the median had high number of workers around the minimum wage

static, wage-posting model where firms have wage setting power and firms maximize profit by choosing two decisions: optimal wages and whether to provide the minimum level of amenity required by the government. Workers obtain utility from receiving wages and amenity.

Concretely, on the labor supply side, worker i working formally at firm j receive utility U_{ij} derived from wage W_j and amenity I_j. Specifically, the utility received is $U_{ij} = log(W_j) + \alpha I_j + \epsilon_{ij}$, where α is an additive utility if firm provides amenity, I_j equals to 1 if firm provides amenity and 0 otherwise, and ϵ_{ij} is distributed under type 1 Extreme value distribution. Therefore, assuming that the number of firms is large, probability of working for firm j can be approximated by $P_j \approx \kappa e^{log(W_j)+\alpha I_j}$. If there are N number of workers in the labor market, the firm-specific labor supply function is therefore $L_j = N\kappa e^{\beta log(W_j)+\alpha I_j}$

On the demand side, firms produce output $Y = A_j G(L)$, where A_j is firm-specific productivity shifter, and $G(L)$ is increasing, and concave in labor, L. Firms decides W_j and amenity provision taking into account the labor supply function. Cost of providing amenity per worker equals C. Furthermore, with some exogenous probability P, firms are inspected. If inspected and found to not provide the amenity, firms have to pay full cost of providing the amenity plus some fine, F. We assume that $C > P(C + F)$, that is the cost of providing amenity is greater than the expected cost of not providing amenity which ensures firms have incentive to not provide amenity.

To obtain the optimal decisions, we solve using a two-step maximization problem. In step 1, conditional on a choice of amenity provision, the firms choose optimal wages W_j. In step 2, we compare the profit obtained when providing amenity vs when no provision of amenity. In step 1, the firm maximization problem if it does not provide amenity is

$$\max_{W_j} \quad A_j G[L_j(W_j; I_j = 0)] - L_j(W_j; I_j = 0)[W_j + P(C + F)]$$

while the maximization problem if it provides amenity is

$$\max_{W_j} \quad A_j G[L_j(W_j; I_j = 1)] - L_j(W_j; I_j = 1)[W_j + C]$$

Denote W_j^1 as optimal wages that the firm chooses when providing amenity, while W_j^0 to be the optimal wages that the firm chooses when not providing amenity. When a firm gets inspected, it has to provide amenity i.e. the response of the firm when it gets inspected is to change the wages to W_j^1. Therefore, the prediction of the model that is appropriate for the empirical exercise of this section is the difference between W_j^0 and W_j^1.

Assuming that the production function is increasing and concave, we obtain the first prediction of the model that $W_j^0 > W_j^1$ (See appendix A1.1 for proof). This means that when

firms get inspected and are forced to provide amenity, incompliant firms will then reduce their wages. Furthermore, we also get the second prediction of the model: the difference between W_j^0 and W_j^1 is increasing in α (See appendix A1.1 for proof). In other words, the drop in wages when a firm gets inspected is larger if workers value the amenity more. Both of these predictions are going to be tested in the empirical exercises.

6.2.2 Results

We begin with the effects of inspection on wages of stayers, presented in figure 6. The post-period coefficients show that 4 years after inspection, wages for inspected workers who stayed in the baseline establishment gradually dropped compared to control workers who stayed in their baseline establishments. 4 years after inspection, wages drop by around 0.4% compared to pre-inspection levels. As previously explained, although this outcome variable takes into account all jobs worked in a given year, most of the workers who stayed in the baseline firm only worked in that baseline firm. Therefore, this analyses is sufficient to also get at how within-establishment wage changes after inspection. This change in wage structure contributes to the overall negative effect on wages we find earlier in section 6.1.

This effect on wages for stayers also mask heterogeneous effects by type of violations found as shown in figure 7. Panel (a) shows that across the 4 years after inspection, wages of workers who stay in baseline establishment caught with at least formalization violation increase by around 0.5% compared to their control workers. On the other hand, panel (b) shows that stayers at baseline establishment with FGTS violations experience a decline in wages of around 0.65%. This result for FGTS violations aligns with the prediction from the conceptual framework if workers value FGTS as a non-wage amenity. Panel (c) and (d) of figure 7 also show what happens to wages in firms with no formalization and no FGTS infraction respectively, confirming that these types of violations are key to the observed wage effects. The increase in average wages for stayers is not present in firms without formalization infraction while the decrease in average wages is also not present in firms without FGTS infractions. We obtain qualitatively similar results, albeit noisier, when we restrict the sample to only include mutually exclusive set of inspections shown in the appendix table A6.

As noted earlier, we are subject to sample selection issues when restricting the sample to stayers. To measure how large is the bias, we apply the trimming approach in Lee (2009) to obtain bounds on selection on wages of stayers, presented in appendix table A5. Columns (1) - (5) show that for inspections with at least formalization violations, the upper bound is always positive and statistically significant while the lower bound is not statistically significant and on average positive, except the last two years. On the other hand, for inspections with at least FGTS violations as indicated in columns (6) - (10), the upper bound is always not significant

and generally negative while the lower bound is negative and statistically significant.

To gauge the magnitude of the effects obtained, we compare the findings on stayers in this paper with findings related to firm-stayers in other contexts. Szerman (2024) finds that labor inspections related to disabled workers in Brazil led to a decline in wages of around 5.7% for incumbent disabled workers and limited effects on incumbent non-disabled workers. Meanwhile, Lagaras (Forthcoming) shows that in Brazil, workers who stay in target firms of a merger and acquisition deal do not experience significant decline in wages.

6.2.3 Mechanisms

This section explores potential mechanisms behind the wage decrease for inspected firms with FGTS violations and the wage increase for inspected firms with formalization violations. We examine two potential explanations for the wage decline: i) increase in amenity and ii) pass-through of costs. Labor inspections increase firms' compliance with labor regulations potentially increasing benefits to formal work. If workers value these benefits and see them as substitute to wages, they may be willing to accept lower wages. On the other hand, inspections could also exert costs to firms, as suggested by section 5.1. If firms pass this increase in costs to workers, this could result in decline in workers wages.

First, we examine whether compensating differentials due to increase in amenity explain the wage reductions in firms violating FGTS regulations. The previous conceptual framework predicts that the greater the utility individuals derived from the amenity, the larger the wage drop. The FGTS is a severance payment account that provides "insurance" amenity for workers upon job displacement. In particular, this amenity should be valued more by individuals working in firms with high layoff risk, since they can withdraw the FGTS balance to smooth their consumption upon the highly likely firing. Therefore, workers may be willing to accept lower wages as a substitute for the amenity. This is similar to the findings by Van Doornik et al. (2022), which shows that unemployment insurance reduces the wage premium that high-turnover firms pay since workers can access the UI if they got fired.

With this in mind, our empirical analysis to test whether increase in amenity can explain the drop in wages for stayers is to compare the wage changes between high turnover and low turnover establishments, condition on inspections finding FGTS violations. For each firm in our sample, we compute the share of workers who were fired without cause within one year of hire in the two years before inspection. We then split the sample into terciles, with the first tercile indicating low turnover firms and third tercile indicating high turnover firms.

Column (1)-(3) of table 5 report estimates from running equation 2 on the sample of stayers, splitting the sample by terciles of turnover risk and only for inspections which found

FGTS violations. As hypothesized, the impact of inspection on wages is predominantly on firms with high turnover risk, while the impact on wages is small and not statistically significant for firms with low turnover risk. As a placebo check, we expect that this pattern of differential impact between low and high turnover firms should only be present for FGTS inspections. Indeed, columns (4)-(6) of table 5 support this hypothesis and the wage effects for high turnover firms is not more negative than the wage effects for low turnover firms.

We also provide suggestive evidence that voluntary separations drop, implying that workers value this amenity. Appendix figure A6 displays plot of firm-level regression with share of voluntary separation as outcome, focusing on inspections with at least FGTS violations and separating the sample between low and high turnover risk. Voluntary separations decrease in the long run with larger decrease for firms with high turnover risk. Pooling the coefficients of relative time $r = 1, 2, 3, 4$, table A7 shows that there is a statistically significant (at 0.1 significance level) decrease in voluntary separations for high turnover firms, while the decrease for low turnover firms is not significant. As placebo, we run the same specification focusing on inspections without FGTS violations, presented in columns (3) and (4), and finds no significant effect on voluntary separation for inspections without FGTS violations.

Next, we explore the second mechanism, pass-through of costs to workers, by comparing the wage effects of firms in the tradable sector against firms in the non-tradable sector. The idea behind this exercise is firms operating in the tradable sector have their product market determined by larger markets (either nationwide or global). Prices are determined competitively at the market level and firms have no power to set prices of their goods. Therefore, firms of tradable sector are less able to shift the burden arising from increase in costs to their consumers. If cost passthrough is a mechanism, we should expect that wage effects due to inspection to be stronger in the tradable sector.

Table 6 columns (1)-(4) report the magnitudes of the aggregate difference-in-difference estimate, where columns (1)-(2) look at all inspections while columns (3)-(4) restrict to inspections with at least FGTS inspections. For both samples, we can see that the magnitude of the effects are more negative for firms in the tradable sector implying that pass-through of costs is one possible mechanism in which wages decline for stayers of inspected firms.

Another test to evaluate the pass-through mechanism that we perform is to look into heterogeneity by share of workers around the minimum wage. Consistent with section 6.1.1, we also find that establishments which hire a high proportion of workers around the minimum wage do not reduce their workers' wages as much compared to firms with small proportion of workers around the minimum wage. This may be because firms hiring a lot of workers around the minimum wage are more binded by the minimum wage, so do not have room to lower wages much in response to inspections. Column (5) of table 6 shows a statistically

significant decline in wages of around 0.4% for firms with low proportion of minimum wage workers, while the effect for firms with high portion of minimum wage workers is positive and not statistically significant. When focusing on inspections with FGTS violations, the negative magnitude is larger to around 1%.

Next, we explore two potential mechanisms behind the wage increase for firms violating informality: 1) spillover effects due to hiring of informal workers to incumbent workers around the minimum wage, and 2) large reorganization of the firm which induces reallocation of tasks across incumbents.

Similar to how increase in minimum wage spills over to workers above the minimum wage and increases their wages (Engbom and Moser, 2022), the hiring of informal workers could also induce the same effect. With informal workers being absorbed to the firm, lower earning incumbent workers would demand higher wages to earn above the newly formalized workers. This pattern can be seen in columns (1) and (2) of table 7, where the increase in wages is mainly driven by workers earning below 1.5 times the minimum wage at baseline.

A second potential mechanism is due to reorganization of the firm. Recall from section 5 that firms found to violate formalization regulations experience immediate increase in separation and hire. This may result in a large shake-up of the firm organization structure, leading to reallocation of tasks across workers and incumbents demanding higher wages. Columns (3) and (5) of table 7 provides evidence for this hypothesis, showing that the increase in wages is predominantly in firms with high share of "informal" admits and high share of separation immediately during the year of separation, while there does not seem to be any effects for firms with low share of "informal" admits and share of separation[17].

Columns (7) - (10) of table 7 splits the sample based on a combination of the two measures, share of "informal" admit and share of separation i.e. we split separately for high admit - high separation, high admit - low separation, low admit - high separation and low admit - low separation. As can be seen from the table, most of the action comes from the cell high admit - high separation (column (7)) and high admit - low separation (column (8)). These findings reflect both of the earlier hypotheses; firms with high "informal admits" and high separations are likely to be firms with large reorganization of the firm while firms with high "informal" admits and low separation are likely to be firms who retain the "informal" admits, thus generating the spillover effects.

[17]We split the sample by whether the firm's response in share of "informal" admit and separation at time $t = 0$ is above or below the median. "Informal" admits are defined as workers who are hired around the time of inspection

6.3 Effect on Leavers

The previous subsection documents that decline in wages for workers that stay in the inspected establishments, implying within-establishment changes in the wage structure driving the overall decline in wages after inspection. This section explores another key channel in which wages can decline after inspection - displacement of workers and reallocation costs associated with job transitions. To evaluate this channel, we look at the wages of workers who leave the baseline establishments in the year after inspection. To get the immediate impact of inspections, we restrict to individuals who leave the baseline establishments either during the year of inspection or one year after (relative time period $r = 0$ or $r = 1$. Our main specification for analyzing the leavers compare average wages for firm leavers of inspected establishments with the average worker of the control establishments (who may or may not move to other establishments after the event).

We also run a separate regression where we stratify both the treated and control group. That is, we compare voluntary leavers with voluntary leavers of the control group only, and involuntary leavers with involuntary leavers of the control group only. We interpret the analysis in which we do not stratify the control group as a combination of both the effects of leaving the baseline establishment due to the inspection (extensive margin) and the wage change arising from changing jobs due to inspection (intensive margin). In contrast, the interpretation of the analysis in which we stratify both the treatment and control group as only illustrating the intensive margin. If inspections do not differentially change the type of jobs that leavers get after leaving (match effects) or there is no selection in who leaves, then we should expect no effect in the intensive margin.

6.3.1 Results

Exploiting the detailed information of RAIS which allows us to separate the reasons of termination, Figure 8 panel (a) presents the results for voluntary and non-voluntary leavers. Pooling the estimates before and after inspection and comparing the difference, we find that wages increase for voluntary leavers by around 1.1% relative to the average worker of the control group. Meanwhile, wages decline by around 5.5% for involuntary leavers relative to the average worker of the control group. The increase in wages for voluntary leavers maybe because they are a selected group of workers and would only leave the establishment if they were able to find better jobs. On the other hand, the decline in wages for involuntary leavers could be explained by reallocation costs associated with job displacement and transitions.

Figure A7 presents the same results for voluntary and involuntary leavers, but also stratifying the control group. There is no statistically significant effect for voluntary leavers

while for the involuntary leavers, there is a marginally significant negative effect (at 10% significance level) of 0.5%.

Relative to estimates in the literature regarding effects of job displacement on wages, the effects we find here is somewhat small. For example, in Brazil, the negative effects on wages for displaced workers after mass layoff is as large as 40% (Amorim et al. (2023)). The large difference may be due to mass layoffs often associated with a decline in demand at the labor market-level, which may prevent displaced workers to get new jobs. On the other hand, our estimates is similar to displacement effects due to mergers and acquisitions in Brazil, which finds effects of around 6% (Lagaras (Forthcoming)).

Next, we examine how the impact of inspection on wages differ by type of violations found. As in section 6.2.2, we focus on formalization and FGTS infractions. Panel (a) of figure 8 displays the results for inspections which found at least formalization violations, while panel (b) shows the results for inspections which found at least FGTS violations. From panel (a), we can see that there is a non-statistically significant increase in wages for voluntary separations (3.7% increase) on average across the post-inspection years while involuntary leavers of establishments caught with formalization violations experience a drop in wages of around 5.9%. On the other hand, panel (c) of figure 8 shows the effects of having caught with FGTS violations. After caught with FGTS violations, voluntary leavers workers experience an statistically significant increase in wages of around 4.8% while for involuntary leavers, they experience a decline in wages of around 2.7%.

To understand what drives the negative impacts for involuntary leavers, we try to tease out the specific possible reallocation costs associated when transitioning to a different job. Specifically, we examine whether the costs are related to switching occupations of switching sectors. Figure 9 plots event study coefficients to illustrate the impacts of inspections for involuntary leavers, separating for those who switch occupation (panel (a)) and switch sector (panel (b)). Panel (a) suggests that involuntary leavers experience decline in wages more for those who switch occupation compared to those who did not switch occupation. Similarly, panel (b) also indicates that the reallocation costs when transitioning to different jobs are also higher for workers that switch sectors compared to those who did not.

7 Conclusion

Two prominent features of labor markets in developing countries are the prevalence of informal sectors and the lax labor regulation compliance. Recognizing the adverse impacts that these two problems can create, governments possess an array of tools to try address this problem. One such regulatory tool that governments commonly utilize is labor inspections, in

which government officers would visit establishments in-person and scrutinize whether these establishments conform to the labor regulations. While previous literature has examined the adverse impacts at the labor market level and on firm outcomes, not much have been studied on workers outcomes. In this regard, our study sought to fill this gap by exploring the effects on workers' labor market outcomes—a critical yet under explored aspect.

Overall, our study illustrated potential unintended negative consequences of such interventions. Utilizing administrative datasets on establishment-level inspection and matched employer-employee datasets from Brazil, we examined the repercussions of labor inspections on both firm and employee outcomes using a matched difference-in-differences design. In line with previous studies on firm outcomes, our findings revealed significant costs to firms, manifested in reduced employment levels and average wage. Furthermore, we also uncover interesting dynamics on firm hiring and separation, with hiring and separation increasing immediately post-inspection, and gradually decreasing in the later years after inspection.

Turning to the main results of our paper regarding worker-level outcomes, we find that these firm-level repercussions and costs reverberated through the workforce. Incumbent employees experience lower wages and increased likelihood of job instability following inspections. Digging deeper, we uncovered nuanced patterns in the impact across various worker demographics, suggesting potential distributional consequences of inspections. While vulnerable groups such as high-tenured, older workers, and females are found to bear a disproportionate brunt of inspection, we also obtain surprising results indicating adverse effects on college-educated and higher-wage workers. We provide evidence that these unexpected findings are due to the interplay between minimum wage and labor inspection.

The results drawn from post-inspection analysis at the worker level are in line with two main factors contributing to wage declines. Firstly, firms may adjust their wage structures in response to inspection outcomes, potentially driven by either cost pass-through or compensating differential due to increase in work non-wage benefits post-inspection. Additionally, job displacement costs incurred by workers leaving inspected firms may also play a role.

An examination of employees remaining in inspected firms reveals wage increases for firms caught with informality violations and wage decreases for those caught with severance payment violations, indicating within-firm compensation adjustments as a contributing factor. Exploiting the detailed data on the types of infractions found in each inspection, we provide evidence that both improved amenities and cost pass-through contribute to these wage declines while wage spillover and large reorganization of the firm contribute to the wage increase. To investigate whether costs associated to job displacement also contributes to the overall wage drop, we focus on workers who leave these firms post-inspection. Our results show that involuntary departures lead to wage reductions, whereas voluntary separations

result in wage increases. More negative impacts of inspections are observed for involuntary leavers who change occupation compared to those that did not change occupation. Meanwhile, both sector switchers and non-switchers experience wage subsequent decreases.

Our study brings valuable insights to the discourse on the effects of labor market interventions designed to tackle informality and compliance. By shedding light on the interplay between labor inspections, firm adjustments, and worker outcomes, we provide policymakers with a clearer understanding of the trade-offs inherent in labor inspections.

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8 Tables

Table 1: Summary Statistics of Inspected and Control Establishments

	Inspected			Control			Difference
	N	Mean	SD	N	Mean	SD	
No. of Employees	12199	18.678	30.022	12199	18.249	22.942	0.429
							(0.878)
Log(Employee)	12199	2.573	0.749	12199	2.57	0.743	0.0265
							(0.0182)
Log(Total Wage)	12199	9.857	0.961	12199	9.861	0.951	-0.00399
							(0.037)
Log(Mean Wage)	12199	7.284	0.374	12199	7.291	0.373	-0.00687
							(0.011)
% White	12199	0.677	0.345	12199	0.689	0.336	-0.0114
							(0.009)
% Male	12199	0.552	0.304	12199	0.583	0.301	-0.0315***
							(0.009)
% High School Above	12199	0.735	0.284	12199	0.733	0.274	0.00162
							(0.006)
Agriculture	12199	0.004	0.06	12199	0.004	0.063	-0.000328
							(0.001)
Oil, Mining and Metals	12199	0.001	0.024	12199	0	0.02	0.000164
							(0.000)
Manufacturing	12199	0.103	0.304	12199	0.101	0.301	0.00205
							(0.006)
Construction	12199	0.014	0.119	12199	0.014	0.116	0.000738
							(0.002)
Retail	12199	0.623	0.485	12199	0.626	0.484	-0.00328
							(0.013)
Other Services	12199	0.255	0.436	12199	0.255	0.436	6.56E-04
							(0.010)

Note: Summary statistics of baseline firm-level variables. The difference estimates and standard errors in the last column are obtained from running a regression of $y_i m = \beta Inspect_{im} + \epsilon_i m$ where $y_i m$ are the characteristics, $Inspect_{im}$ is a dummy for whether firm i is inspecteds. Standard errors are clustered at the micro-region level

Table 2: Summary Statistics of Inspected and Control Workers

	Inspected			Control			Difference
	N	Mean	SD	N	Mean	SD	
Male	82455	0.593	0.491	84972	0.621	0.485	-0.0253***
							(0.00602)
Below HS	82455	0.375	0.484	84972	0.393	0.489	-0.0153**
							(0.00684)
White	82455	0.717	0.45	84972	0.738	0.44	-0.0134**
							(0.00527)
Tenure	82455	55.602	47.849	84972	57.513	49.164	-1.140*
							(0.650)
Hours Contract	82455	43.589	1.477	84972	43.691	1.287	-0.0763***
							(0.0292)
Log(Wage)	82455	7.438	1.006	84972	7.409	0.939	0.0365**
							(0.0167)
Age	82455	36.497	9.774	84972	36.656	9.746	-0.178*
							(0.102)

Note: Summary statistics of baseline worker-level variables. The difference estimates and standard errors in the last column are obtained from running a regression of $y_{im} = \beta Inspect_{im} + \alpha_m + \epsilon_{im}$ where $y_i m$ are the characteristics, $Inspect_{im}$ is a dummy for whether individual i worked at an inspected firm and α_m is micro-region fixed effects. Standard errors are clustered at the micro-region level

Table 3: Effects of Inspection on Wages Including RAIS Drop-Outs

	(1) Baseline	(2) Missing = 0	(3) Missing = MW	(4) Missing = Informal
Panel A				
Post X Inspect	-0.00466***	-0.0395***	-0.00920***	-0.00691***
	(.00132)	(0.00887)	0.00153	0.00129
Observations	1,056,749	1,451,405	1,451,405	1,451,405
R-squared	0.904	0.449	0.812	0.855
Panel B				
	NotFormal			
Inspect	0.0122***			
	(0.00290)			
Observations	183,772			
R-squared	0.032			

Note: Worker-level results from estimating equation 3 where the outcome in panel A is log average wages in a year and making assumptions regarding wages of workers who left RAIS. In panel A, column (1) reports the baseline specification dropping individuals who dropped out of RAIS, column (2) assumes that the wages of workers who leave RAIS to be zero, column (3) assumes the wages to be the average wage of the informal sector in the state, and column (4) assumes the wage to be the minimum wage. In panel B, we ran a cross section regression where the outcome is indicator variable denoting if worker i has ever dropped out of RAIS in the years after inspection. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race as well as tenure. Standard errors are clustered at the worker level.

Table 4: Heterogeneity of Impacts of Inspection on Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Wage Distribution at T-1		Education		By Minimum Wage		% of Workers <1.5 MW	
	Tercile 1	Tercile 3	HS/Below	College	<1.5 MW	>1.5 MW	Below Median	Above Median
Post X Inspect	0.00695***	-0.00970***	-0.00407***	-0.0125***	0.00983***	-0.0101***	-0.00877***	0.00322
	(0.00209)	(0.00262)	(0.00135)	(0.00477)	(0.00231)	(0.00159)	(0.00166)	(0.00223)
Observations	335,942	369,251	910,964	145,275	278,363	778,041	717,408	339,234
R-squared	0.685	0.864	0.868	0.919	0.677	0.887	0.895	0.819

Note: Worker-level results from estimating equation 3 where the outcome is log average wages in a year by baseline characteristics. Columns (1) and (2) split the sample by tercile of baseline wage distribution while column (3) and (4) split the sample based on education level in baseline year. Columns (5) and (6) split the sample by whether the worker's baseline wage at time $t^* - 1$ is above or below 1.5 times the minimum wage. Columns (7) and (8) split the sample according the firm-level share of workers earning below 1.5 times minimum wage. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

Table 5: Impacts of FGTS Inspections on Wages by Establishment Layoff Risk

	(1)	(2)	(3)	(4)	(5)	(6)
	At Least FGTS			No FGTS		
	T1	T2	T3	T1	T2	T3
Post X Inspect	0.002	-0.002	-0.018***	0.00496*	-0.00683***	0.00676**
	(0.005)	(0.006)	(0.006)	(0.00271)	(0.00246)	(0.00299)
Observations	64,425	55,962	53,705	201,819	279,462	185,054
R-squared	0.931	0.934	0.913	0.951	0.946	0.928

Note:Worker-level results from estimating equation 3 where the outcome is log average wages in a year restricting the sample to firm stayers in both the treated and control firm and split the sample by type of infraction as well as terciles of firm-level layoff risk. For each firm, we compute the share of workers who were fired without cause within one year of hire in the two years before inspection. Column (1) - (3) restrict the sample to inspections which find at least FGTS infractions, while column (4)-(6) limit the sample to inspections without FGTS violations. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

Table 6: Heterogeneous Impacts of Inspections on Stayers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any Infraction		FGTS Infraction		Any Infraction		FGTS Infraction	
	Tradable	Non-Tradable	Tradable	Non-Tradable	Low % <1.5MW	High % <1.5MW	Low % <1.5MW	High % <1.5MW
Post X Inspect	-0.00255	-0.00175	-0.0415***	-0.00196	-0.00402**	0.00274	-0.0110**	-0.00568
	(0.00336)	(0.00154)	(0.00939)	(0.00349)	(0.00163)	(0.00225)	(0.00448)	(0.00431)
Observations	132,846	630,885	24,856	131,348	569,765	283,275	90,281	87,291
R-squared	0.944	0.941	0.935	0.924	0.936	0.893	0.921	0.870

Note: Worker-level results from estimating equation 3 where the outcome is log average wages in a year by baseline firm characteristics restricting the sample to firm stayers in both treated and control group. Columns (1) - (4) split the sample by the sector in which the firm operates in, while column (5) and (8) split the sample by share of workers in the firm earning wages below 1.5x minimum wage at baseline. All regression includes worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

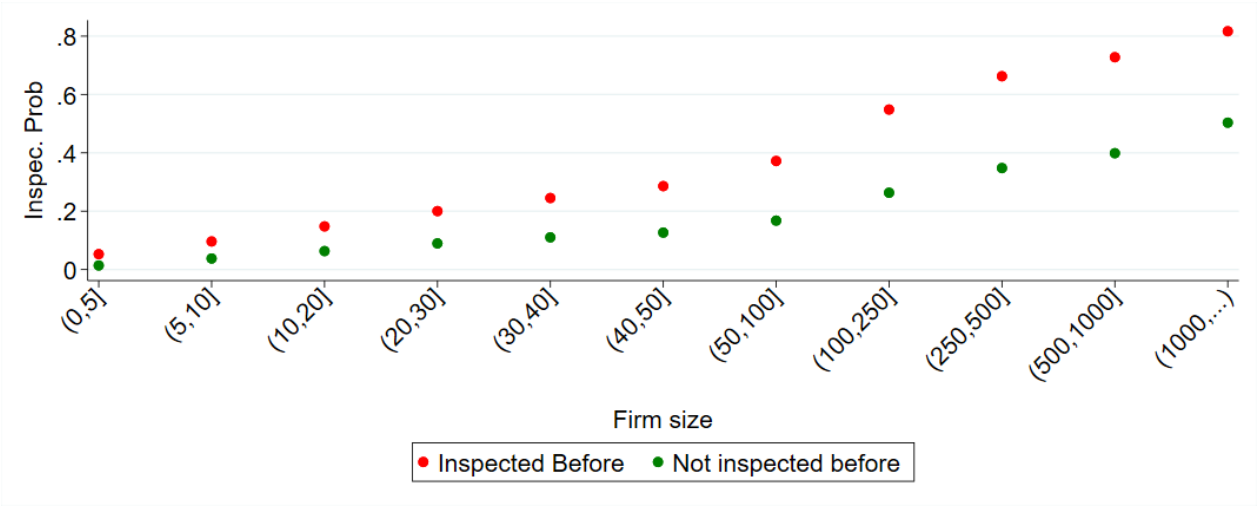
Table 7: Heterogeneous Impacts of Formalization Inspections on Stayers

Dep Var: log(Wages)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	By Minimum Wage		Informal Admit		Separation		Informal Admit and Separation			
	<1.5 MW	>1.5 MW	High	Low	High	Low	High Admit, High Sep	High Admit, Low Sep	Low Admit, High Sep	Low Admit, Low Sep
Post X Inspect	0.0120***	0.0042*	0.0133***	-0.00202	0.0133***	0.000329	0.0169***	0.00985**	-0.0101	-0.00268
	(0.0037)	(0.0023)	(0.00287)	(0.00283)	(0.00359)	(0.00246)	(0.00408)	(0.00446)	(0.00826)	(0.00316)
Observations	101,387	320,387	209,443	212,485	142,218	279,708	114,257	94,874	34,961	177,296
R-squared	0.7506	0.9322	0.930	0.952	0.919	0.950	0.920	0.940	0.929	0.955

Note: Worker-level results from estimating equation 3 where the outcome is log average wages in a year by baseline firm characteristics restricting the sample to firm stayers in both treated and control group. Columns (1) and (2) split the sample by whether the worker earns below or above 1.5 X the minimum wage in the baseline year. Columns (3) and (4) split the sample by above and below median of share of "informal" admits post inspection, while columns (5) and (6) split the sample by above and below median of share of separation post inspection. Columns (7) - (10) split the sample based on a combination of these two measures. "Informal" admits are defined as workers who are hired around the time of inspection. All regression includes worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

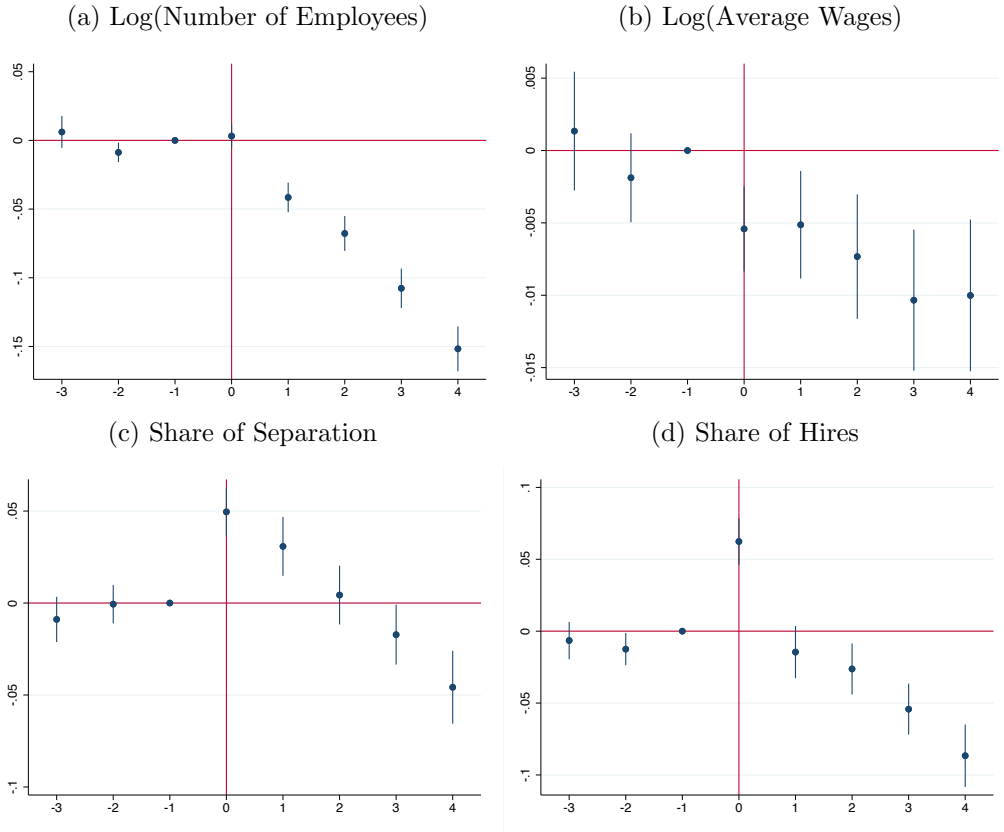
9 Figures

Figure 1: Probability of Inspection by firm size:



Source: RAIS 2011 to 2013.

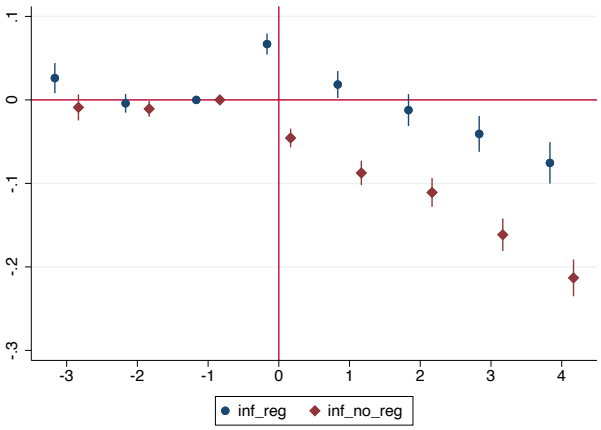
Figure 2: Event Study Plots for the Effects of Inspections on Establishment-level Outcomes



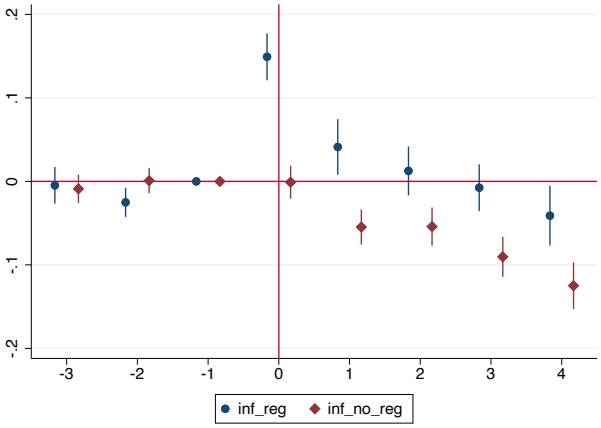
Note: Firm-level results from estimating equation 1 where the outcome is log of number of employees, log of average wage, share of separation and share of hire. All regression includes firm fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Standard errors are clustered at the firm level.

Figure 3: Event Study Plots for the Effects of Formalization Violations Inspections on Establishment-level Outcomes

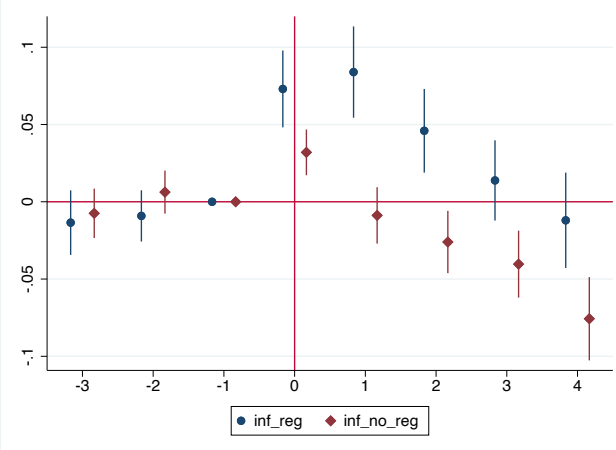
(a) Log(Number of Employees)



(b) Share of Hires



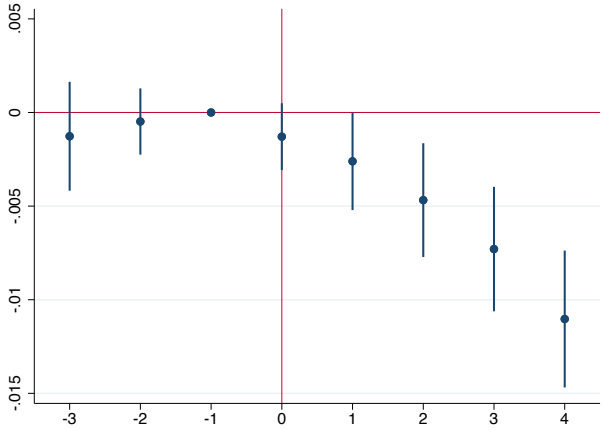
(c) Share of Separations



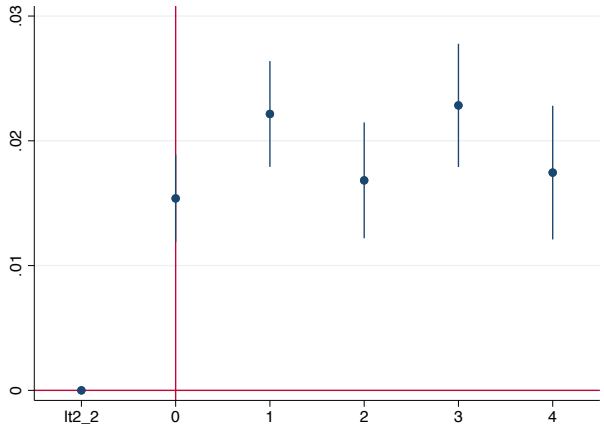
Note: Firm-level results from estimating equation 1 where the outcome is log number of employees, share of hires and share of separation as a fraction of number of employees at baseline by type of violation. Blue circles indicate coefficients from a regression restricting sample to inspections which find at least formalization infraction, while the red diamonds indicate coefficients from a regression restricting sample to inspections which find no formalization violation. All regression includes firm fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Standard errors are clustered at the firm level.

Figure 4: Event Study Plots for the Effects of Inspections on Workers Labor Market Outcomes

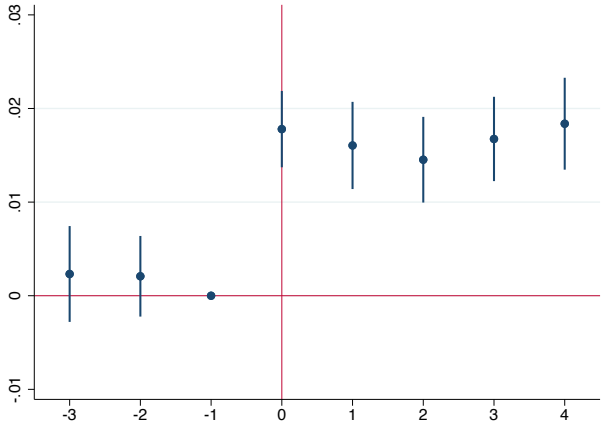
(a) Log(Average Wage)



(b) P(Leave Firm)



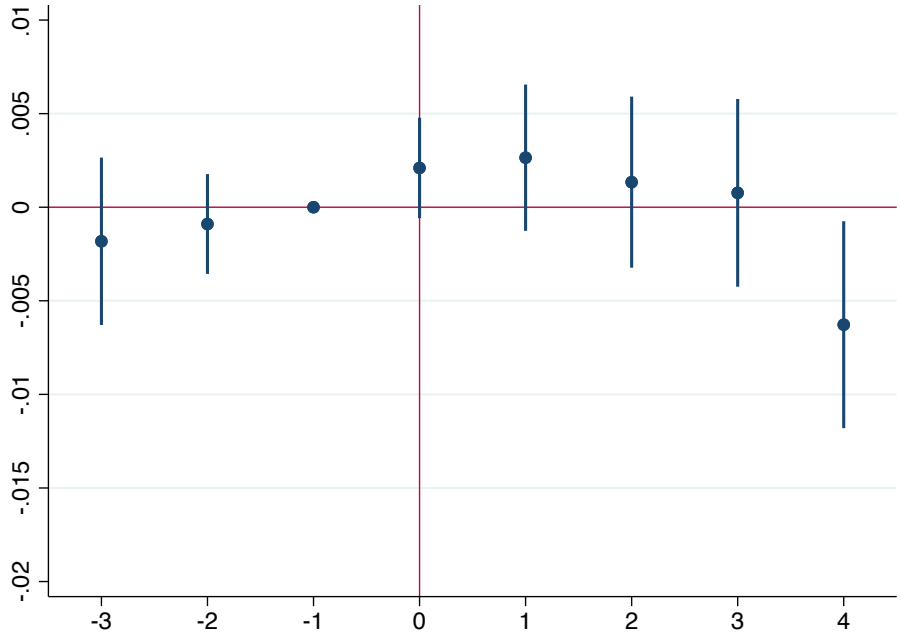
(c) P(Work < 12 Months of Full Time Work)



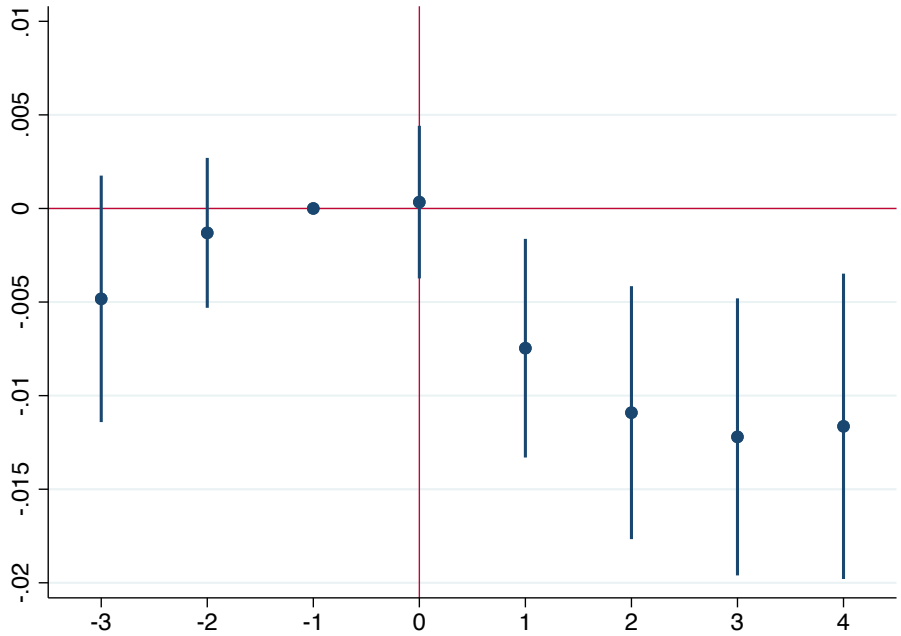
Note: Worker-level results from estimating equation 2 where the outcome is log average wages in a year (panel (a)), indicator for leaving the firm (panel (b)), and indicator for working less than 12 months of full time work (panel (c)). All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

Figure 5: Effect of Inspection Wages for Establishment By Type of Infraction

(a) At Least Formalization Infraction



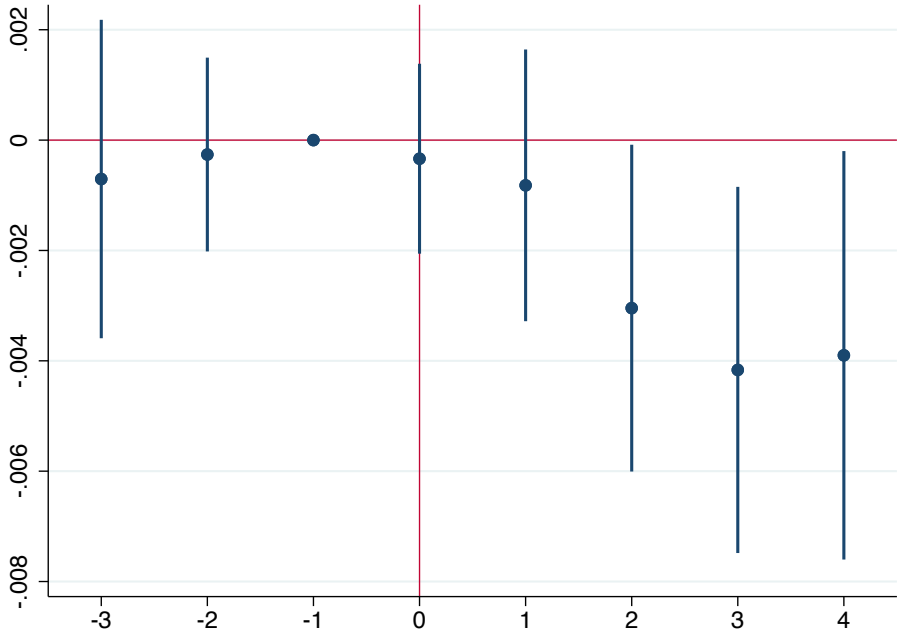
(b) At Least FGTS Infraction



Note: Worker-level results from estimating equation 2 by type of infractions found. The outcome variable is log average wages in a year. Panel (a) limits the sample to inspections which find at least formalization violations, while panel (b) restricts the sample to inspection which find at least FGTS violations. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

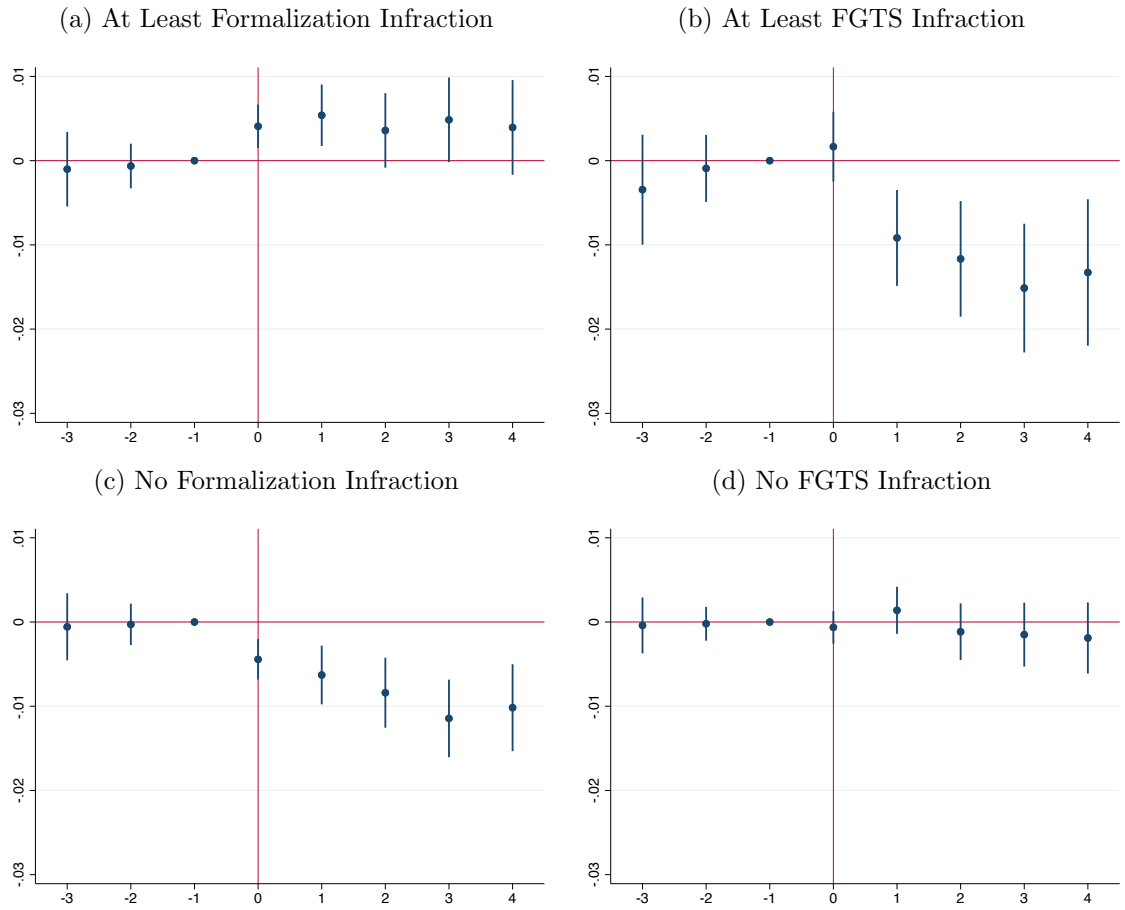
Figure 6: Effect of Inspection Wages for Establishment-Stayers

(a) Log(Average Wage)



Note: Worker-level results from estimating equation 2 restricting the sample to firm stayers for both inspected and control firms. The outcome variable is log average wages in a year. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

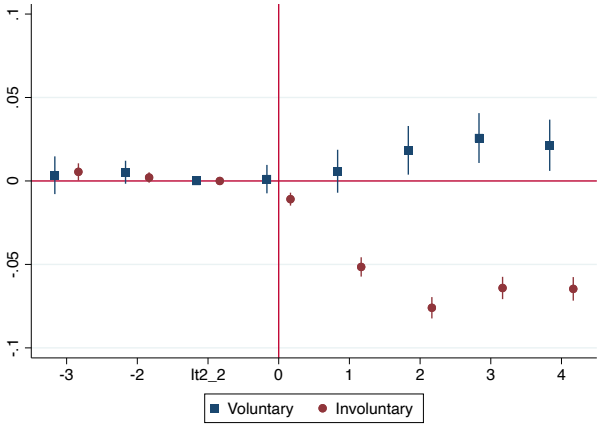
Figure 7: Effect of Inspection Wages for Establishment-Stayers By Type of Infraction



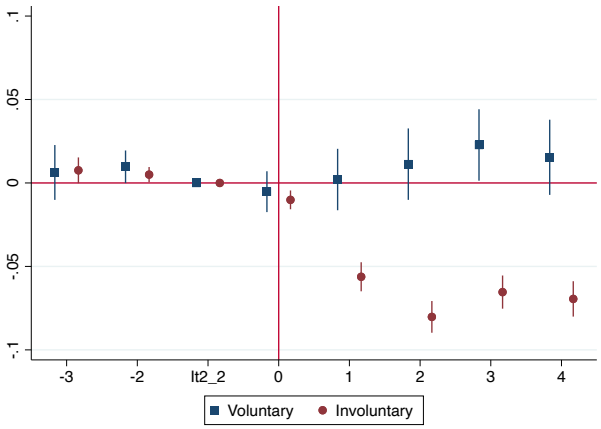
Note: Worker-level results from estimating equation 2 restricting the sample to firm stayers for both inspected and control firms by type of infractions found. The outcome variable is log average wages in a year. Panel (a) limits the sample to inspections which find at least formalization violations, while panel (b) restricts the sample to inspection which find at least FGTS violations. Panel (c) limits the sample to inspections with no formalization infraction. Panel (d) limits the sample to inspections with no FGTS infraction. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

Figure 8: Effect of Inspection on Leavers

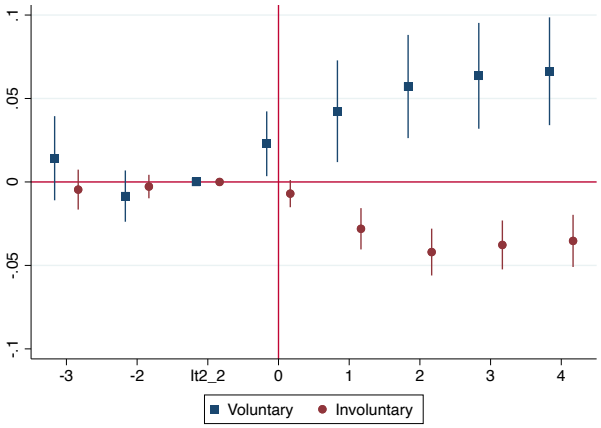
(a) Any Infractions



(b) At Least Formalization



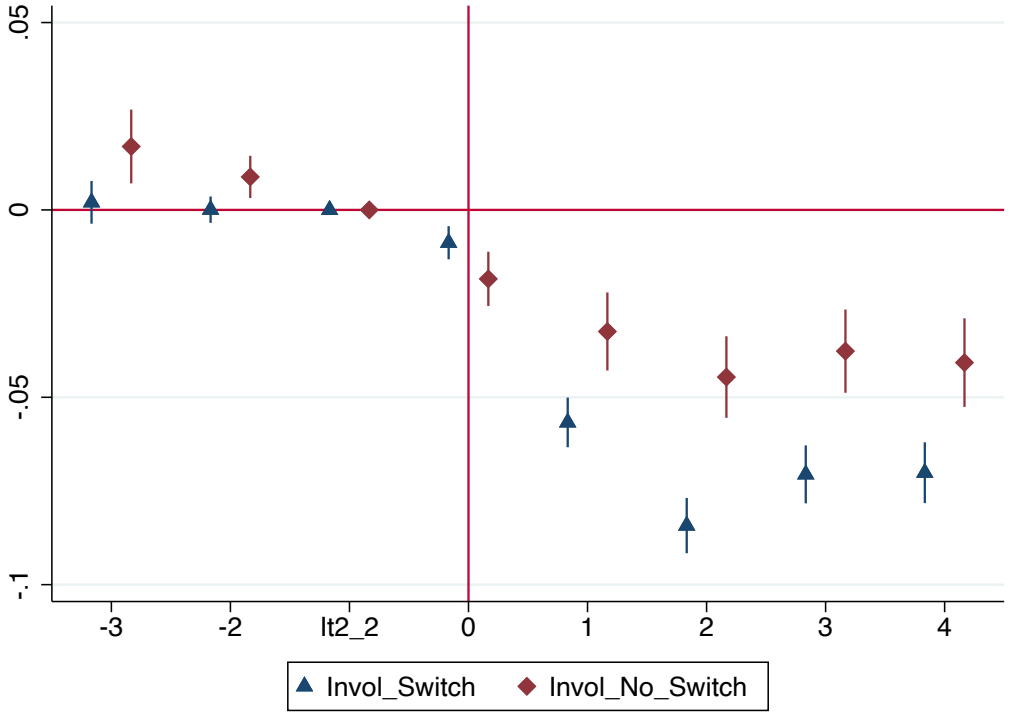
(c) At Least FGTS



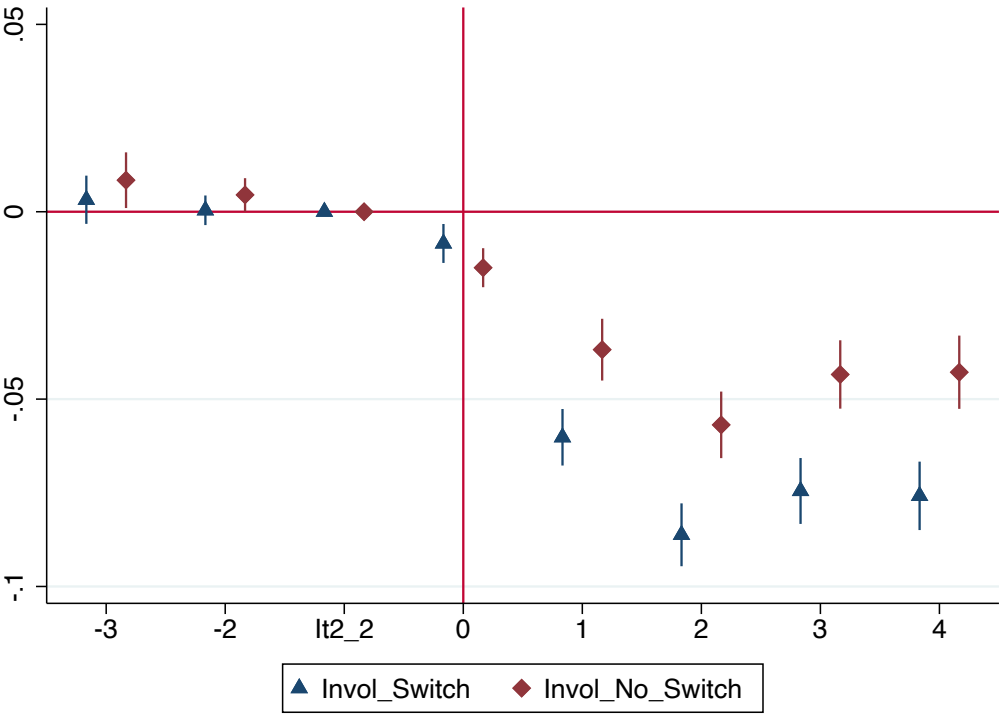
Note: Worker-level results from estimating equation 2 by type of infraction found, restricting the sample to firm leavers of the inspected firms, and all workers of the control firms. The outcomes are log average wages in a year. The figure presents results for voluntary leavers (blue squares) and involuntary leavers (red circles) separately. Panel (a) presents results for all inspections with at least one infraction. Panel (b) presents results for inspections with at least formalization infractions, while panel (c) shows results for inspections with at least FGTS infractions. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

Figure 9: Involuntary Leavers By Occupation and Sector Transitions

(a) Switch Occupation



(b) Switch Sector



Note: Worker-level results from estimating equation 2 by transition types, restricting the sample to involuntary firm leavers of the inspected firms, and all workers of the control firms. The outcomes are log average wages in a year. Panel (a) splits the sample by whether the employee switch occupation when they leave the firm, while panel (b) splits the sample by whether the employee switch sector when they leave the firm. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

A1 Appendix

A1.1 Model Derivations

In this section, we derive the two predictions of the model listed in section 6.2.1 assuming a more general production function, $G(L)$ that is increasing and concave.

Before we present the proofs, we simplify some notations. We drop the firm j notation for ease. From the firm-specific labor supply equation, we can re-write the labor supply when $I = 1$ to be $L = (\frac{1}{\tilde{B}_1})^{\beta} W^{\beta}$ where $\tilde{B}_1 = (\frac{1}{N\kappa e^{\alpha}})^{\frac{1}{\beta}} = (\frac{1}{N\kappa})^{\frac{1}{\beta}} (\frac{1}{e^{\alpha}})^{\frac{1}{\beta}} = \hat{B} e^{\frac{-\alpha}{\beta}}$. While, the labor supply when $I = 0$ is $L = (\frac{1}{\tilde{B}_0})^{\beta} W^{\beta}$ where $\tilde{B}_0 = \hat{B}$. Furthermore, let W^1 be the optimal wages when $I = 1$, and W^0 be the optimal wages when $I = 0$.

Prediction I: $W^0 > W^1$

Proof. Conditional on providing amenity, the firm has the following maximization problem

$$\max_{W} \quad AG[L(W; I = 1)] - L(W; I = 1)[W + C]$$

The first order condition for this problem is

$$AG_{L_1}[L_1(W)]\beta = W(\beta + 1) + \beta C \tag{4}$$

where $L_1(W)$ is the labor supply equation when $I = 1$. The solution to this first order condition is the optimal wage, W^1.

Meanwhile, conditional on not providing amenity, the firm has the following maximization problem

$$\max_{W} \quad AG[L(W; I = 0)] - L(W; I = 0)W - PL(W; I = 0)[C + F]$$

The first order condition is

$$AG_{L_0}[L_0(W)]\beta = W(\beta + 1) + \beta P(C + F) \tag{5}$$

where $L_0(W)$ is the labor supply equation when $I = 0$. The solution to this first order condition is the optimal wage, W^0.

The proof strategy is by contradiction. Suppose that $W^0 < W^1$. Since $P(C + F) < C$ and the right hand side of both equation 4 and 5 is increasing, we have that $RHS_0(W^0) <$

$RHS_1(W^0) < RHS_1(W^1)$ where RHS_0 is right-hand side of equation 4 and RHS_1 is right-hand side of equation 5.

Note that for all positive values of W, $L_1(W) > L_0(W)$ because $L_1(W) = (\frac{1}{\tilde{B}}e^{\frac{\alpha}{\beta}})^\beta W^\beta > (\frac{1}{\tilde{B}})^\beta W^\beta = L_0(W)$ as long as $\frac{\alpha}{\beta} > 0$. Concavity of G implies that G_L is decreasing i.e. $G_{L_1}[L_1(W)] < G_{L_0}[L_0(W)]$. Therefore, $LHS_1(W) < LHS_0(W)$. Since we assume that $W^0 < W^1$, it must be that $LHS_0(W^0) > LHS_0(W^1) > LHS_1(W^1)$. But this is a contradiction, since $LHS_0(W^0) = RHS_0(W^0) < RHS_1(W^1) = LHS_1(W^1)$ □

Prediction II: $\Delta = W^0 - W^1$ is increasing in α

Proof. Taking partial derivative of Δ with respect to α, we get $\frac{\partial \Delta}{\partial \alpha} = \frac{\partial W^0}{\partial \alpha} - \frac{\partial W^1}{\partial \alpha}$. Since W^0 is not a function of α, $\frac{\partial W^0}{\partial \alpha} = 0$.

To get $\frac{\partial W^1}{\partial \alpha}$, we implicitly differentiate equation 5 with respect to α. We get

$$A\frac{\partial G_L}{\partial L_1}\left(\frac{\partial L_1}{\partial \alpha} + \frac{\partial L_1}{\partial W}\frac{\partial W}{\partial \alpha}\right)\beta = \frac{\partial W}{\partial \alpha}(\beta + 1)$$

Rearranging to solve for $\frac{\partial W}{\partial \alpha}$, we get

$$\frac{\partial W^1}{\partial \alpha} = \frac{-A\beta\frac{\partial G_{L_1}}{\partial L_1}\frac{\partial L_1}{\partial \alpha}}{A\beta\frac{\partial G_{L_1}}{\partial L_1}\frac{\partial L_1}{\partial W} - (\beta + 1)}$$

Noting that $\frac{\partial G_{L_1}}{\partial L_1} < 0$, $\frac{\partial L_1}{\partial \alpha} > 0$ and $\frac{\partial L_1}{\partial W} > 0$, we get that $\frac{\partial W^1}{\partial \alpha} < 0$. In other words, the higher is the utility on amenity, the lower is the optimal wage. This is in line with compensating differential theory.

Therefore, we have that $\frac{\partial \Delta}{\partial \alpha} = -\frac{\partial W^1}{\partial \alpha} > 0$

□

A1.2 Tables

Table A1: Count of Inspections by Infraction

	N	%
Total Inspections	12,199	
Formalization	5,310	43.5
FGTS	3,585	29.3
Salary	1,984	16.2
Child Labor	1,609	13.2
Journey	2,146	17.6
Rest	1,862	15.3
Others	4,428	36.2

Note: Count of number of inspections by infraction found. An inspection can have multiple infractions so adding the count column by infraction will not add up to the total number of inspections.

Table A2: Robustness to Sample Selection and Exit

VARIABLES	(1) log(Employees)	(2) log(Employees)
Post X Inspect	-0.0702***	-0.151***
	(0.00571)	(0.00554)
Observations	194,714	281,100
R-squared	0.861	0.796
Balanced	Yes	No
Robust standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Note: Firm-Level results from estimating firm-level version of equation 3 where the outcome is log number of employees applying different sample restrictions. Column (1) restricts to firms that do not exit in both the treated and control group, thus the treatment group is more positively selected. Column (2) allows treated firms to exit, therefore the control group is more positively selected. All regression includes firm fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Standard errors are clustered at the firm level.

Table A3: Summary Statistics of Worker Characterics by "Informality" Status

	Informal			Formal			
	N	Mean	SD	N	Mean	SD	Difference
Age	23889	27.994	10.012	33935	29.368	9.732	-1.473***
							(0.101)
Experience	23889	10.718	10.384	33935	11.963	10.193	-1.414***
							(0.101)
Male	23889	0.54	0.498	33935	0.549	0.498	-0.00619
							(0.00430)
Below High School	23889	0.387	0.487	33935	0.336	0.472	0.0466***
							(0.00523)
White	23889	0.634	0.482	33935	0.644	0.479	-0.00331
							(0.00478)
Average Wage	23889	1451.855	1260.618	33935	1589.016	1555.901	-142.0***
							(24.44)
Hours	23889	41.094	7.311	33935	42.385	5.649	1.605***
							(0.0771)
Full Time	23889	0.879	0.326	33935	0.942	0.234	-0.0769***
							(0.00366)

Note: Summary statistics of baseline worker-level variables by "informality" status. "Informal" workers are workers who are hired around the time of inspection, while formal workers are workers hired during the 6 months before inspection. Difference column is obtained from running a regression $Y_if = \beta Informal_{if} + \delta_f + \epsilon + if$ where δ_f is firm fixed effects. Standard errors clustered at the firm level

Table A4: Heterogeneity Impacts of Inspection on Wages By Worker Characteristic

	(1)	(2)	(3)	(4)	(5)	(6)
	Tenure		Gender		Age	
	0-3 Years	Above 3 Years	Male	Female	>= 35	<35
Post X Inspect	-0.00141	-0.00703***	-0.00199	-0.00857***	-0.00609***	-0.00358*
	(0.00214)	(0.00167)	(0.00171)	(0.00212)	(0.00171)	(0.00202)
Observations	457,378	599,178	647,377	409,011	537,775	518,602
R-squared	0.862	0.928	0.901	0.906	0.928	0.875

Note: Worker-level results from estimating equation 3 where the outcome is log average wages in a year by baseline worker characteristics. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

Table A5: Lee (2009) Bounds for Stayers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	At Least Formalization					At Least FGTS				
Year Relative to Inspection	t = 0	t = 1	t = 2	t = 3	t = 4	t = 0	t = 1	t = 2	t = 3	t = 4
Upper Bound	0.0062***	0.0137***	0.0194***	0.0257***	0.0279***	0.0011	-0.0084	-0.0070	-0.0115	-0.0073
	(0.0023)	(0.0035)	(0.0044)	(0.0049)	(0.0054)	(0.0039)	(0.0063)	(0.0076)	(0.0085)	(0.0093)
Lower Bound	0.0027	0.0042	0.0018	-0.0006	-0.0043	-0.0005	-0.0124***	-0.0112**	-0.0191***	-0.01678*
	(0.0021)	(0.0028)	(0.0036)	(0.0040)	(0.0051)	(0.0027)	(0.0044)	(0.0055)	(0.0062)	(0.0083)

Note: Worker-level results from estimating equation 3 where the outcome is log average wages in a year or the wage at the baseline firm applying the Lee (2009) bounds procedure. We applied the Lee (2009) bounds procedure for each time. The sample is restricted to firm stayers.

Table A6: Impacts of Inspections By Infraction Types for Stayers

	(1) At Least Formal	(2) At Least FGTS	(3) Formal, No FGTS	(4) FGTS, No Formal	(5) Formalization Only	(6) FGTS Only
Post X Inspect	0.00490**	-0.00650**	0.00690***	-0.00619*	0.00336	-0.00176
	(0.00195)	(0.00296)	(0.00215)	(0.00368)	(0.00397)	(0.00583)
Observations	422,273	177,794	362,011	117,594	133,457	51,028
R-squared	0.944	0.923	0.945	0.930	0.952	0.938

Note: Worker-level results from estimating equation 3 restricting the sample to firm stayers for both inspected and control firms by type of infractions found. The outcome variable is log average wages in a year. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

Table A7: Impacts of FGTS Inspections on Voluntary Separation by Establishment Layoff Risk

	(1)	(2)	(3)	(4)
	At Least FGTS		No FGTS	
	BelowMedian	AboveMedian	BelowMedian	AboveMedian
Post X Inspect	-0.00161	-0.0237*	0.00755	-0.00443
	(0.0114)	(0.0132)	(0.00727)	(0.00615)
Observations	22,845	22,380	76,648	76,502
R-squared	0.708	0.623	0.717	0.651

Note: Firm-level results from estimating equation 3 where the share of voluntary separation as a fraction of total employees at baseline. The estimates presented are long-term effects of being caught with FGTS violation on voluntary separation, pooling together coefficients relative time $r = 1, 2, 3, 4$. Column (1) and (2) restrict the sample to inspections which find at least FGTS infractions, while column (3) and (4) limit the sample to inspections without FGTS violations. All regression includes firm fixed effects fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Standard errors are clustered at the firm level.

Table A8: Heterogeneous Impacts on Leavers by Type of Infraction Found

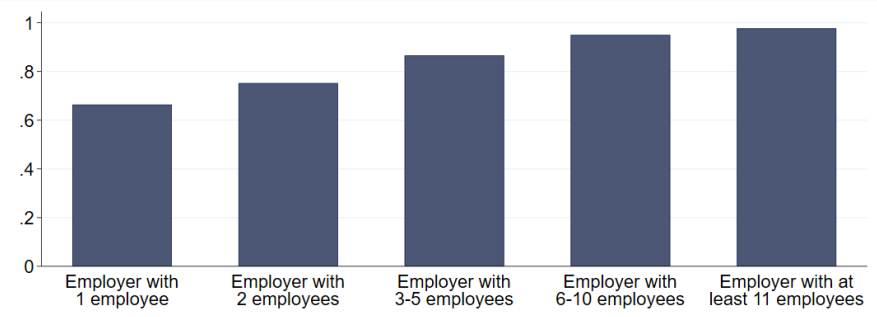
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Formalization, No FGTS		Formalization only		FGTS, No Formalization		FGTS Only	
	Voluntary	Involuntary	Voluntary	Involuntary	Voluntary	Involuntary	Voluntary	Involuntary
Post X Inspect	0.0226**	-0.0375***	0.0133	-0.0500***	0.0741***	-0.0264**	0.0478*	-0.0258
	(0.0107)	(0.00641)	(0.0174)	(0.0105)	(0.0215)	(0.0113)	(0.0291)	(0.0201)
Observations	348,852	400,112	125,854	143,585	113,821	130,402	47,598	54,315
R-squared	0.712	0.696	0.728	0.718	0.691	0.678	0.708	0.688

Note: Worker-level results from estimating equation 3 where the outcome is log average wages in a year by type of violations found. The sample is restricted to firm leavers of the treated group, while the control group is not stratified. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.

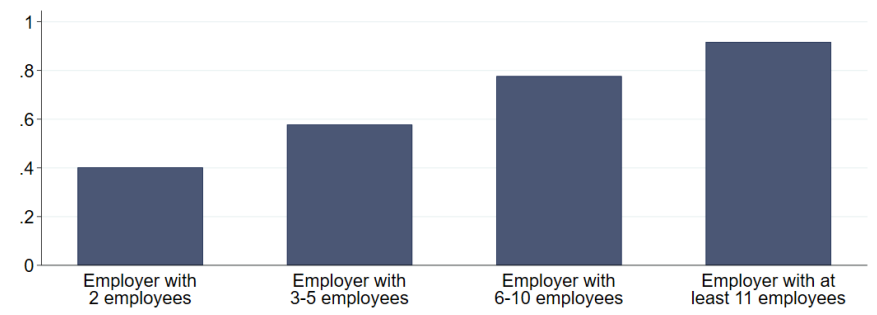
A1.3 Figure

Figure A1: Firms and workers formality by firm size:

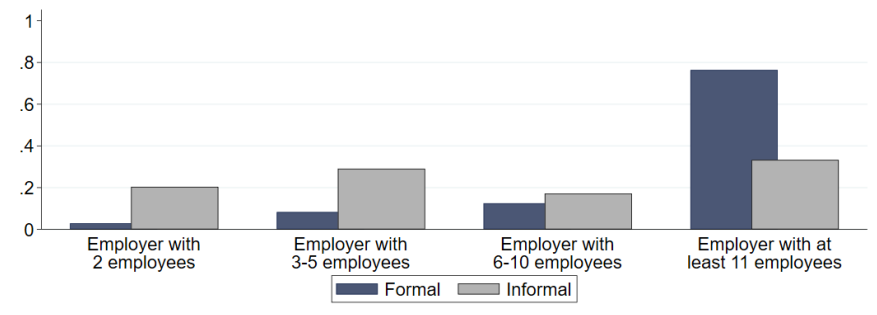
(a) Share of formal employers:



(b) Share of formal employees:

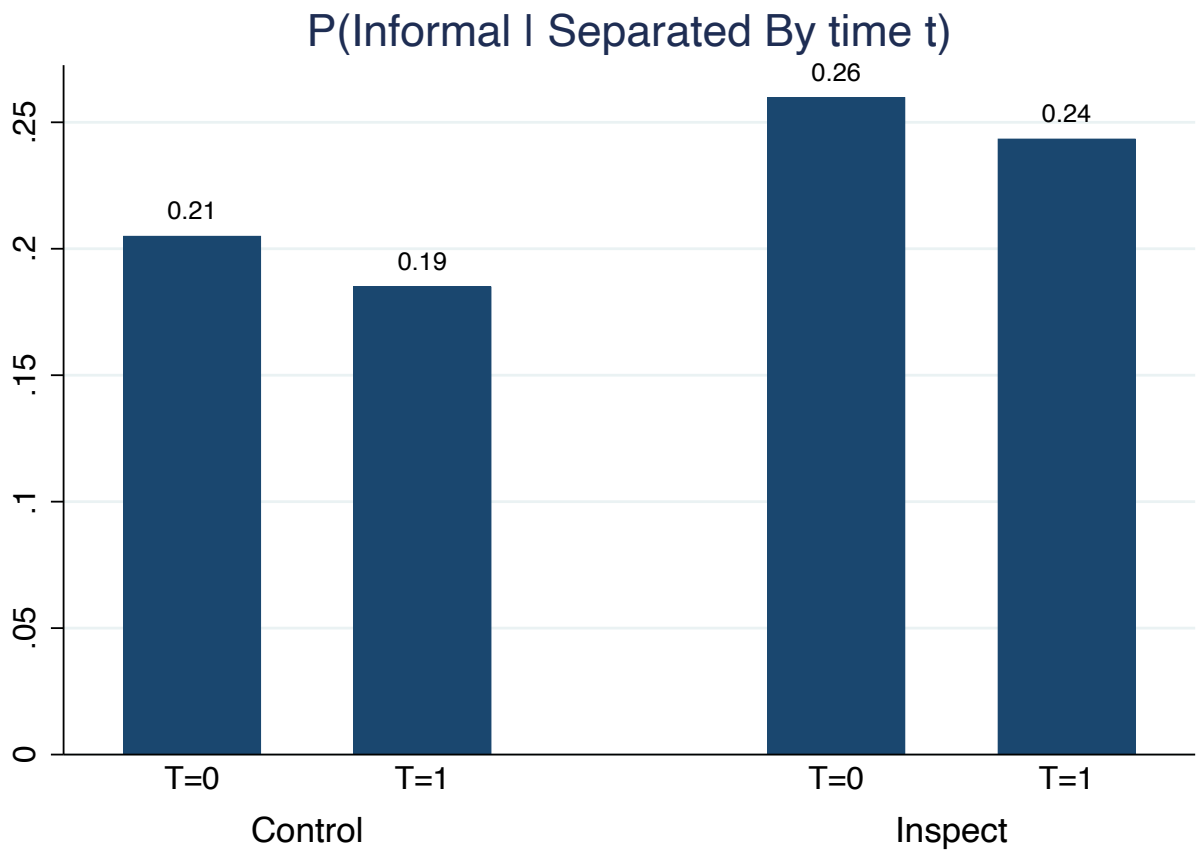


(c) Distribution of formal and informal employees by firm size:



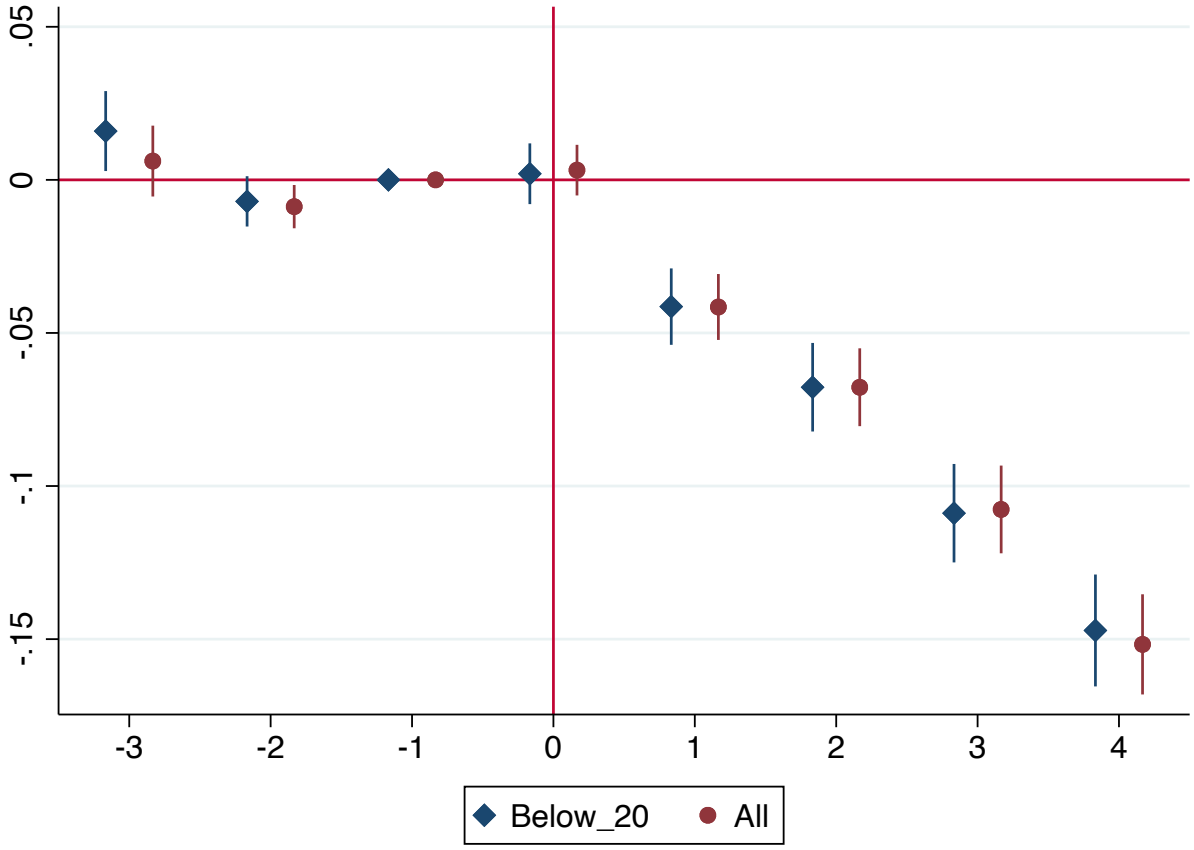
Source: PNAD 2011 to 2013. Household survey.

Figure A2: Probability of "Informal" Conditional on Separated by time t



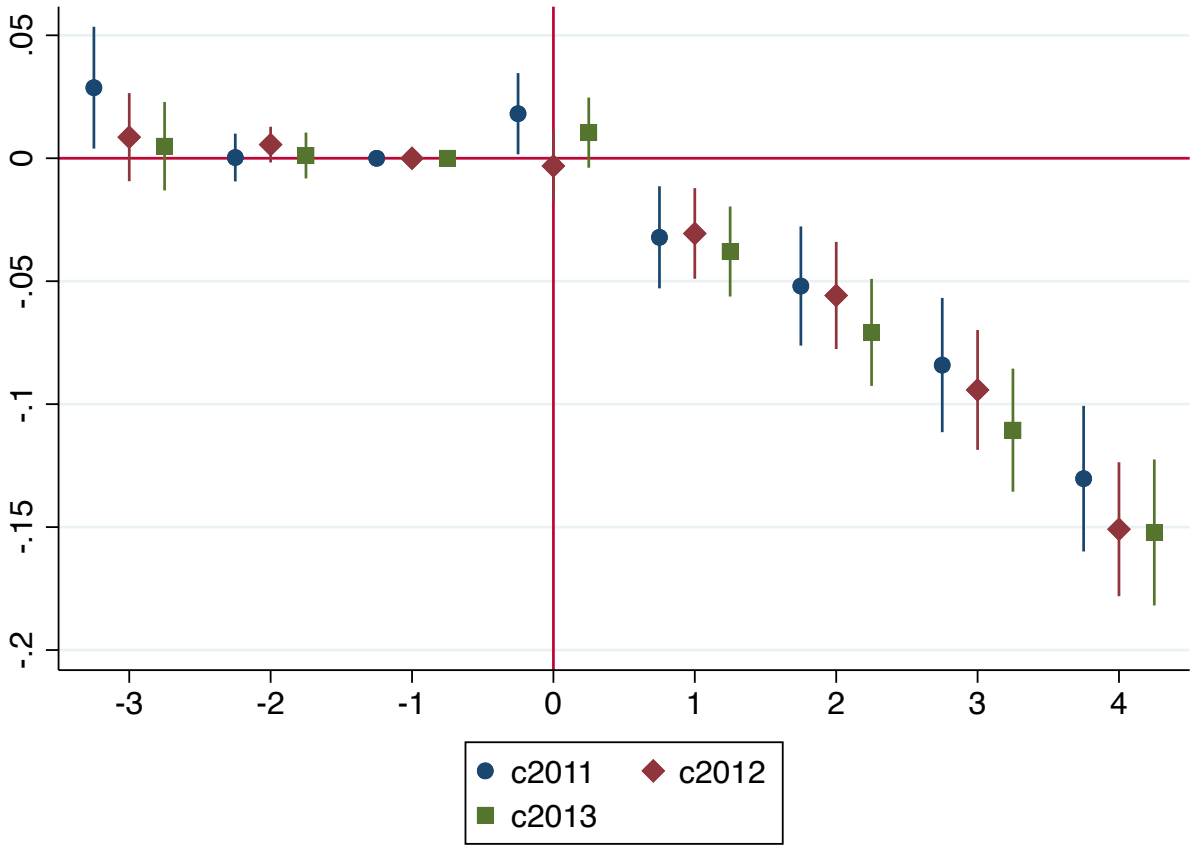
Note: Bar chart showing probability of a worker being "informal" given that she separates by time $t = 0, 1$ by and treatment status. "Informal" is define as workers who are hired around the time of inspection.

Figure A3: Event Study Plots for the Effects of Inspections on Number of Employes For Firms with Less than 20 Employees



Note: Firm-level results from estimating equation 1 where the outcome is log number of employees for firms with less than 20 employees at baseline. All regression includes firm fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Standard errors are clustered at the firm level.

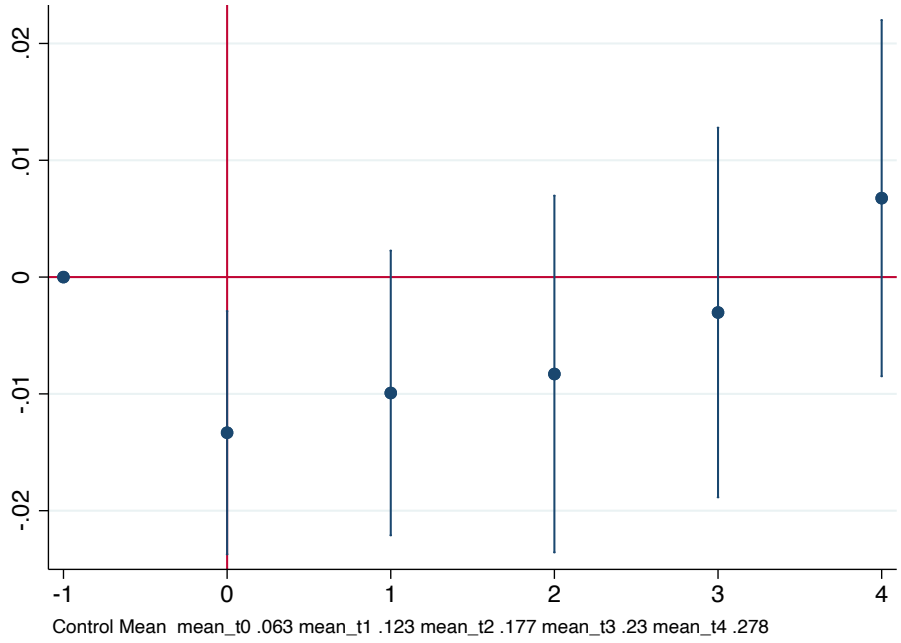
Figure A4: Event Study Plots for the Effects of Inspections on Number of Employes By Cohort



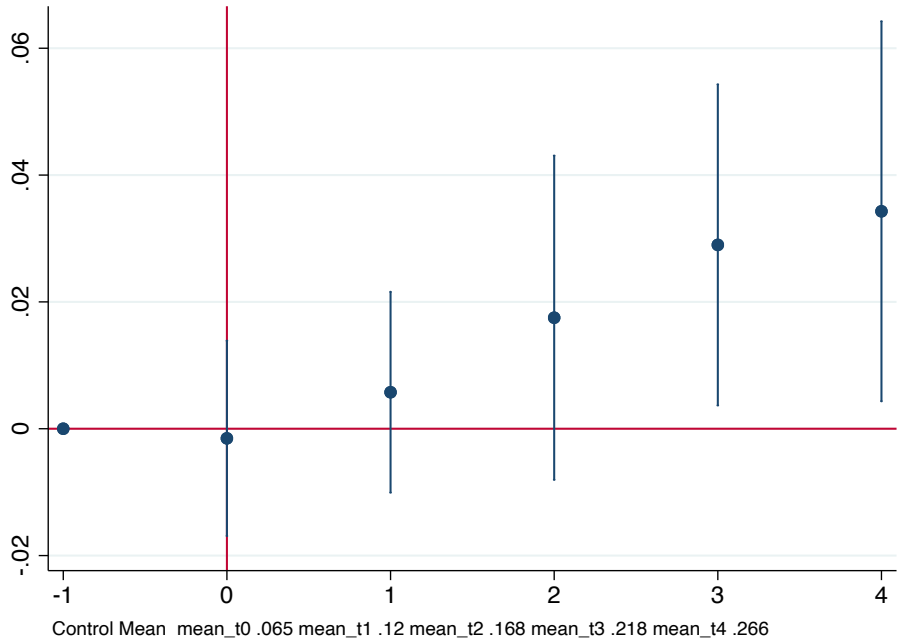
Note: Firm-level results from estimating equation 1 where the outcome is log number of employees by inspection cohort. All regression includes firm fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Standard errors are clustered at the firm level.

Figure A5: Exit Rates of Inspected Firms

(a) All Inspections with at least one Infraction

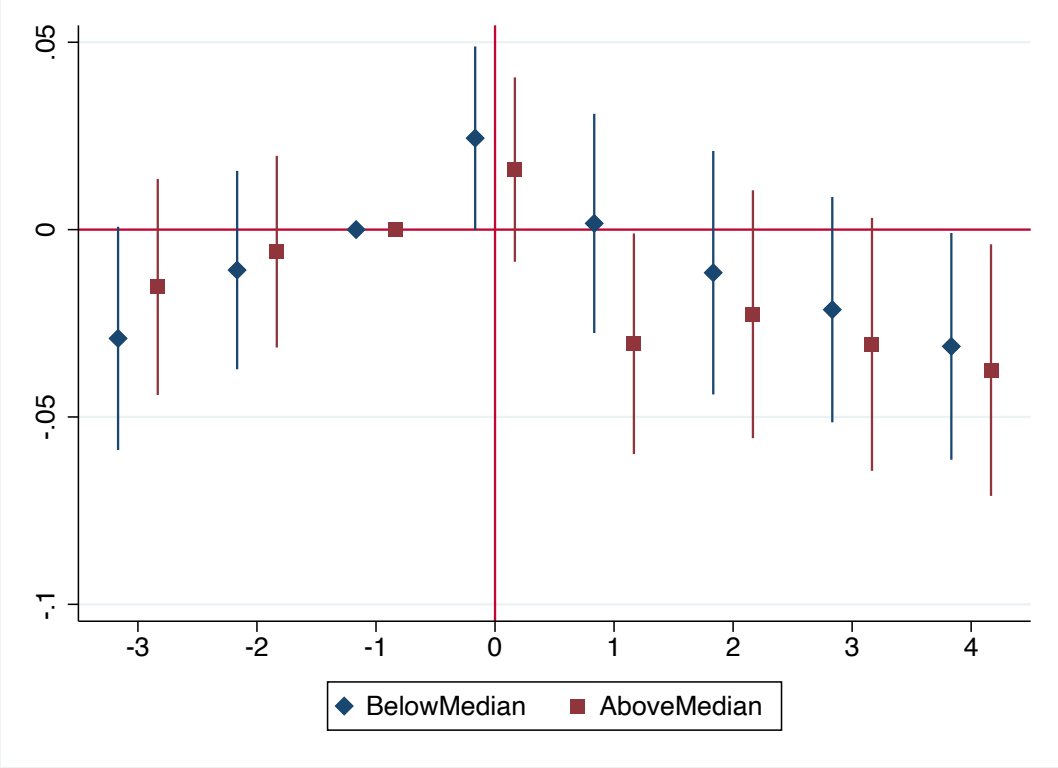


(b) Inspection During 1st Quarter of Calendar Year



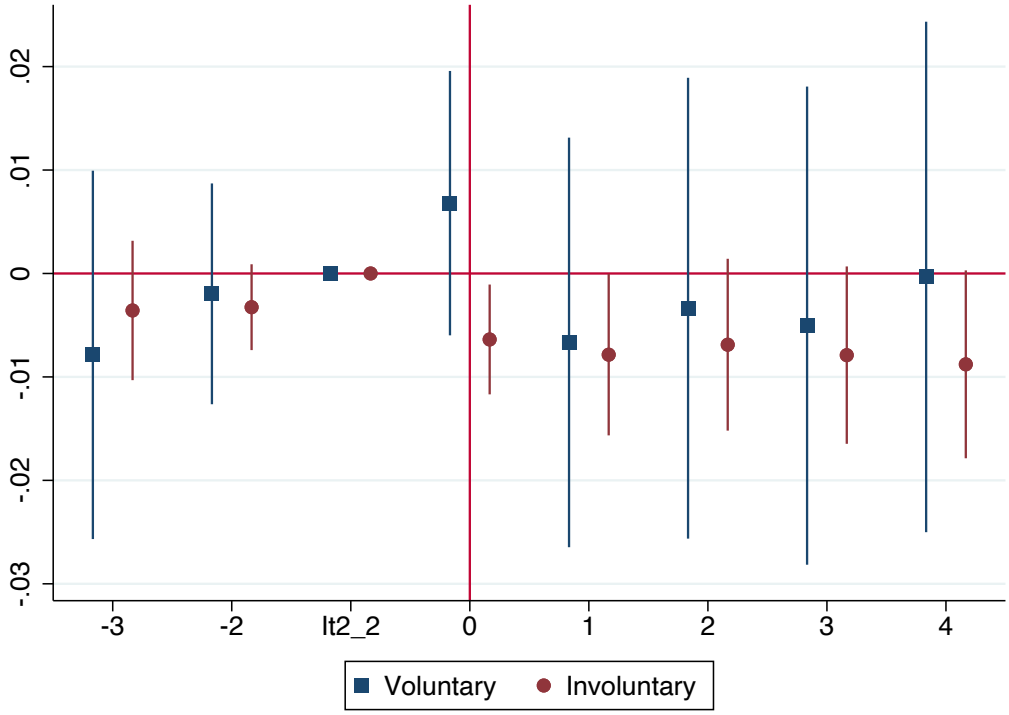
Note: Firm-level results from estimating a cross section regression $I(ExitAtTime = t)_i = \beta Inspect_i + \delta X_i + \epsilon_i$ where the outcome is indicator variable indicating if firm i is not present in the data at time t. Not present in the data at time t indicates zero workers at the end of year t. As the control group, we used never inspected firms and we choose control group by applying matching based on observables the year before inspection. Panel (a) reports results using all inspections with at least one type of infraction, while panel (b) displays results for inspection during 1st quarter of the calendar year.

Figure A6: Impacts of FGTS Inspections on Firm-Level Voluntary Separation by Establishment Layoff Risk



Note: Firm-level results from estimating equation 1 restricting the sample to inspections which find at least FGTS violations. We split the sample by whether the firm's baseline layoff risk is below or above the median. The outcome variable is share of voluntary separation as a fraction of number of employees at baseline. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Standard errors are clustered at the firm level.

Figure A7: Leavers All Infractions (Stratified)



Note: Worker-level results from estimating equation 2 restricting the sample to firm leavers of both the inspected firms control firms. The outcomes are log average wages across all jobs in a year. The figure presents results for voluntary leavers (blue squares) and involuntary leavers (red circles) separately. All regression includes firm fixed effects and worker fixed effects. Firm-level controls include interaction between year fixed effects and firm level baseline variables including share of male workers, and (log) number of employees in baseline. Individual level controls include age, age-squared, experience, and interaction between year fixed effects and demographics such as gender, education, race, (log) wages in baseline as well as tenure. Standard errors are clustered at the worker level.