Violation and Enforcement of Workplace Regulations: Evidence from Mexican Firm Inspections

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October 2025

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Abstract

This paper studies the characteristics of large formal firms that violate workplace regulations and analyzes how regulatory enforcement affects firms and workers. A stylized model of monopsonistic firms—in which employers set both wages and working conditions—shows that high levels of labor market power can lead to violations in workplace regulations, and that the enforcement of such regulations can raise firm employment through an expansion of labor supply. To test the model's predictions, we use administrative records of stratified random firm inspections in Mexico, which enforce compliance with workplace safety, health regulations, and mandatory worker training. We link these records to panel surveys and administrative employer-employee data for large manufacturing firms. We find that firms are more likely to be found violating regulations if they invest less in worker training, have lower productivity, employ a smaller share of women, and employ a larger share of the local labor market. Furthermore, inspections tend to increase regulatory compliance, reflected in greater investment in worker training, fewer workplace accidents, and a lower likelihood of future violations. Using a staggered difference-in-differences design, we estimate that inspections increase firm employment by 4 to 7% within one year. Average firm wages decrease by less than 2%, driven by changes in worker composition rather than changes in individual wage setting. Our results indicate that enforcing workplace regulations in large manufacturing firms can be an effective policy tool for improving working conditions, mitigating labor market power, and simultaneously increasing firm employment.

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Agustina Colonna and Lukas Rodrian are very grateful to their advisors Lorenzo Casaburi, David Dorn, Florian Scheuer, and Josef Zweimüller for their guidance and support. We are grateful to David Atkin, David Autor, Francesco Armillei, Fabrizio Colella, Guillermo Cruces, Amanda Dahlstrand, Eva Davoine, León Fernández Bujanda, Ronak Jain, Aino Linnunsalo, Michael Peters, Dina Pomeranz, Joel Slemrod, Julian Streyczek, and Climent Quintana-Domeque for helpful feedback and suggestions, and participants at the UZH Labor PhD Seminar, UZH Development PhD Seminar, the Uppsala Firms and Public Policy Workshop, and the 2025 IIPF Congress in Nairobi. We are extremely thankful to Natalia Volkow Fernández and her team at INEGI's Laboratorio de Microdatos. We also thank Claudia Velázquez Villegas and Osvaldo Veloz from Banco de México for their help and support with the data access. IMSS data was accessed through the Econlab at Banco de Mexico. The EconLab collected and processed the data as part of its effort to promote evidence-based research and to foster ties between Banco de Mexico's research staff and the academic community. Inquiries regarding the terms under which the data can be accessed should be directed to econlab@banxico.org.mx. The views expressed are only those of the authors and they do not reflect the views of INEGI nor Banco de México. Lukas Rodrian acknowledges support by the University of Zurich's Research Priority Program 'Equality of Opportunity'.

1 Introduction

Workplace regulations such as safety and health standards are a cornerstone of worker protection, yet compliance with these regulations remains a significant challenge in many lower- and middle-income countries. Consequently, working conditions in these settings are often precarious and even hazardous. For instance, in 2021, Mexico reported 7.7 fatal workplace accidents per 100,000 workers, more than double the rate of 3.6 in the United States (ILO, 2024). This issue is partly driven by the prevalence of the informal sector, which frequently operates outside the coverage of labor regulations (Ulyssea et al., 2023). Emerging evidence suggests that large formal firms in these environments also often violate legal labor standards (Harrison and Scorse, 2010; Weil, 2014; Boudreau et al., 2024). However, we still lack a comprehensive understanding of the mechanisms driving non-compliance among these firms, as well as the consequences of various enforcement approaches on both firms and workers.[1]

One potentially important factor contributing to poor working conditions among large formal firms is labor market power. Labor market power is widespread in low- and middle-income countries, allowing firms to suppress wages below the marginal product of labor (Felix, 2022; Amodio and De Roux, 2024). Yet monopsony power may not only depress wages (Manning, 2004) but also lead firms to offer substandard working conditions, such as inadequate safety measures, particularly in environments where the enforcement of minimum standards is weak. In this context, the enforcement of workplace regulations—for example, through labor inspections—can serve to reduce labor market power, thereby redistributing economic rents between firms and workers.

In this paper, we study the role of labor market power in explaining violations of workplace regulations and in shaping the effects of regulatory enforcement on large firms and their workers. We analyze both aspects theoretically and empirically, leveraging microdata on stratified random workplace inspections in Mexico merged with firm-level outcomes from surveys and administrative social security records. Our analysis focuses on the manufacturing sector, which is responsible for 43% of all workplace accidents and exhibits an accident rate per worker that is 60% higher than the national average (see Appendix Figures A.1 and A.2b).

We divide the paper into three parts. First, we develop a theoretical framework in which monopsonistic firms face an upward-sloping labor supply function that depends on both wages and non-wage amenities, with the latter capturing working conditions. Monopsony power enables firms to offer a combination of wages and working conditions below competitive levels. Firms are subject to a minimum mandated level of amenities (workplace regulations), which is imperfectly enforced. Firms providing amenities below this mandatory level are considered non-compliant and are fined if detected through inspections that occur with positive probability. The framework yields two main predictions, which we then test empirically. First, a firm's incentive to violate the minimum standard depends not only on the (expected) penalties associated with enforcement (Ulyssea, 2018), but also on firm-level characteristics. Specifically, low-productivity firms and those facing a labor supply that is relatively inelastic to non-wage amenities, i.e., firms with greater market power over amenities,

[1] Enforcement approaches include, e.g., responsible sourcing standards (ILO, 2016), labor provisions in trade agreements (ILAB, 2024), and labor inspections.

are more likely to violate. The second prediction concerns the consequences of enforcement for these non-compliant firms. We show that firms forced to comply can partially adjust by lowering wages when there is no downward wage rigidity. Crucially, if firms possess market power over amenities, then enforcement can lead to an increase in employment, driven by a rise in labor supply.[2]

Second, we empirically assess the model's first prediction by analyzing firm characteristics associated with being found violating workplace regulations during inspections. We leverage records of stratified random firm inspections that enforce compliance with health and safety standards, mandatory training, and general labor conditions. We link these records with detailed survey and administrative employer-employee panel data of large manufacturing firms. The stratification process, which we validate empirically, enables us to control for the inspection probability separately from firm, worker, and local labor market characteristics. We first show that empirical proxies for the variables determining violation in the model are correlated with non-compliance in both univariate and multivariate regressions, controlling for the inspection probability. Specifically, firms with lower productivity, a lower share of female workers (a proxy for a workforce with a lower relative valuation of non-wage conditions, see Maestas et al., 2023), a higher labor market share and location in more concentrated labor markets (proxies for higher labor market power, see Azar et al., 2022) are more likely to be found violating. Low levels of worker training, a proxy for violation of workplace regulation on training mandates, is also correlated with higher violation rates. A data-driven approach using Lasso regularization further supports our model by showing that the theoretical determinants of violation are indeed predictive of actual violations.

Third, we test the model's second prediction by studying the impact of enforcing workplace regulations through stratified random workplace inspections on a rich set of firm and worker characteristics. Our estimation is based on a staggered difference-in-differences design (Dube et al., 2025), comparing first-inspected firms to those not yet or never inspected within the same randomization group. The central mechanism underlying our analysis is that inspections create strong incentives for non-compliant firms to comply with workplace regulations. This mechanism is well-supported by the institutional context: follow-up inspections are frequent when firms are found non-compliant, and fines are imposed on those that fail to comply. We present several pieces of evidence consistent with increased compliance following inspections. First, inspected firms increase their investment in employee training, a mandated workplace benefit in this context.[3] Second, inspected firms are less likely to be found violating in subsequent stratified random inspections, suggesting lasting improvements in compliance. Third, inspections are associated with fewer workplace accidents at the sector-municipality level, indicating improved adherence to workplace safety regulations.

We next examine how firms respond to the enforcement of workplace regulations through inspections. Consistent with the model's second prediction, inspections lead to an increase in firm employment by 4–7%, driven primarily by increases in blue-collar and permanent workers. These effects are concentrated among firms found in violation during the inspection, consistent with these firms being most affected by enforcement. The increase in firm size is explained mainly by new hires

[2]This mechanism is analogous to the standard monopsony model, in which the imposition of a minimum wage can raise employment through a movement along the firm's labor supply curve.

[3]Under Mexico's Federal Labor Law, firms are required to provide mandatory employee training as part of their labor obligations (LFT, 2021).

rather than reduced separations, with most new hires coming from other formal firms. The average firm wage decreases only modestly (by less than 2%), an effect largely attributable to compositional workforce changes rather than adjustments in wage setting. At the worker level, we find precise null effects of inspections on wages for both incumbents and new hires.

Overall, the empirical results align with our theoretical model in which firms use their market power to offer substandard working conditions. When enforcement improves working conditions and wages are downward-rigid, firm employment rises through an increase in labor supply, which depends not only on wages but also on working conditions. Consistent with this mechanism, we find that the post-inspection increase in employment is more pronounced among firms operating in highly concentrated labor markets and firms with a large employment share—both proxies for greater labor market power. Finally, we present suggestive evidence from Mexico's National Employment Survey indicating that workplace conditions are an important factor in workers' decisions to quit or accept a new job. Additionally, the majority of workers in large manufacturing firms report finding their jobs through friends and family. Therefore, it is plausible that newly hired workers are relatively well-informed about workplace conditions and more likely to select into firms where these conditions have improved following an inspection.

We conclude by discussing three main alternative mechanisms that could potentially account for the observed increase in employment following inspections: the formalization of previously informal workers, a mechanical increase in labor required for compliance, and misinformation among firms regarding the potential costs and benefits of improving working conditions (Bertrand and Crépon, 2021). Our empirical evidence suggests that these explanations are unlikely to fully account for the magnitude of the effects—particularly on employment—or that they must operate in an environment where firms possess monopsony power over working conditions in order to explain the post-enforcement employment increase.

Related literature. This paper contributes to three strands of the literature. First, we add to the research on the effects of labor regulations (Besley and Burgess, 2004; Botero et al., 2004; Kugler, 2004), and more specifically to the growing literature on their enforcement. Much of the recent work on enforcement in low- and middle-income countries focuses on multinational corporations and global supply chains, yielding mixed findings. For example, Boudreau (2024) shows that occupational safety and health (OSH) committees among multinational apparel buyers in Bangladesh improve workplace safety without negatively affecting wages, employment, or productivity. In contrast, Alfaro-Ureña et al. (2025) find that responsible sourcing practices raise wages but reduce firm employment in Costa Rica. While this body of work focuses on globally integrated firms, there is a growing emphasis on how enforcement interacts with informality. For instance, Almeida and Carneiro (2012) show that stricter enforcement can push workers into informality by raising formal employment costs in Brazil. Brotherhood et al. (2024) present evidence that formal firms caught with informal workers during inspections in Brazil experience an increase in formal employment in the short run but a decrease in the long run. In a high-income setting, Johnson (2020); Johnson et al. (2023) examine the effects of health and safety inspections in the USA. These papers find improvements in compliance and reductions in violations, but do not observe other firm outcomes. Closest to our study, Samaniego de la Parra and Fernández Bujanda (2024) use Mexican firm inspection data linked to social security data and worker survey data across all sectors to show that enforced labor regulations temporarily increase formalization rates but lead to persistently lower overall formal employment. Our paper differs from Samaniego de la Parra and Fernández Bujanda (2024) in two main respects. First, while their analysis focuses on variation in the costs of employing workers informally, we focus on the role of labor market power in explaining workplace regulation violations in the formal sector and on the consequences of enforcement in a monopsonistic setting. This focus motivates our use of a sample of larger, more productive manufacturing firms (on average, firms in Samaniego de la Parra and Fernández Bujanda (2024) are about half the size of those in our sample). Inspections are likely less relevant to informality costs in our sample, given that informality rates are below 5 percent.[4] These sample differences likely explain the contrasting employment effects reported across the two papers: monopsony power over working conditions—which underlies our finding of positive employment effects—is likely much weaker among smaller firms. Moreover, our employment results are driven by inspections of safety, health, and training regulations instead of general labor conditions, which may include informality. Second, to our knowledge, this is the first paper to link inspection records to rich firm-level manufacturing surveys including information on production processes, costs, revenues, and profits. This linkage allows us to characterize firms found violating workplace regulations—an aspect absent from much of the literature—and to estimate the effects of stratified random variation in working conditions on a broad set of novel firm outcomes, including costs, revenues, and profits. By relying on stratified random inspections conducted by the government, our approach avoids the endogeneity concerns that characterize alternative enforcement channels, such as union bargaining or compliance pressures from multinational supply chains.

Second, this paper advances our understanding of the factors driving non-compliance with regulations. In recent years, the tax evasion literature has evolved significantly in this regard, with Slemrod (2019) providing a comprehensive overview. For instance, Carrillo et al. (2017) and Guyton et al. (2023) show that tax evasion behavior varies across the income distribution for Ecuador and the U.S., respectively. Additionally, this literature examines optimal auditing and enforcement strategies (e.g., Kapon et al., 2022; Elzayn et al., 2024; De Neve et al., 2021; Bergolo et al., 2023; Battaglini et al., 2023), including the role of third-party information provision (Naritomi, 2019; Pomeranz, 2015). More recently, research has also explored the welfare implications of tax enforcement (Boning et al., 2025; Brockmeyer et al., 2023). Comparable evidence in the area of non-compliance with workplace regulations is scarce. Campusano et al. (2024) measure compliance rates with labor regulations by conducting an RCT with SMEs in Chile. Marinescu et al. (2021) finds that in the U.S. at the local labor market level, higher compliance with labor regulations is associated with higher wages, lower labor market concentration, and higher union coverage. Johnson et al. (2023) find that occupational safety and health inspections reduce accidents in the U.S. We contribute to this literature in several ways. We provide novel evidence on firm, worker, and local labor market characteristics linked to labor regulation violations controlling for firm-specific enforcement probabilities. Additionally, our study distinguishes itself by linking these findings to a model of monopsony power, in contrast to the classical Allingham and Sandmo (1972) model, which assumes that evasion decisions occur in-

[4] Manufacturing has the lowest informality rates in Mexico yet remains the primary contributor to workplace accidents (see Appendix Figures A.1, A.3, as well as Samaniego de la Parra and Fernández Bujanda (2024)).

dependently of other economic choices, such as labor supply or production processes. Instead, in our context, workers may react to the level of non-compliance, thereby affecting firm incentives to violate workplace regulations.

Third, this paper contributes to a growing literature studying monopsonistic power among employers in both developed (Manning, 2004; Berger et al., 2022) and developing countries (Felix, 2022; Amodio and De Roux, 2024; Sharma, 2023). Most of this literature focuses on the impact of labor market power in suppressing worker wages and distorting the allocation of workers across firms. A smaller strand of research investigates how monopsonistic firms design total compensation, incorporating both wage and non-wage job attributes.[5] Allowing for differences in the degree of complementarity between worker wages and non-wage benefits among monopsonistic employers, Dube et al. (2022) model the interaction between wage and non-wage amenities in the U.S., including autonomy on the job, coworker relationships, and the quality of supervision. They find that an increase in the minimum wage has no negative effect on workplace amenities, consistent with the notion of wage-amenity complementarity. Lagos (2024) estimates for the case of Brazil non-negligible labor supply elasticities with respect to amenities measured in collective bargaining agreement clauses, including overtime pay, maternity leave, and vacations, implying that markdown estimates ignoring non-wage attributes may be biased. This literature examines various policies to counter labor market power, such as the minimum wage (Engbom and Moser, 2022; Berger et al., 2025) and collective bargaining (Lagos, 2024), typically affecting multiple firms, sectors, or geographic areas simultaneously. We contribute to this literature by leveraging within-firm variation in working conditions through stratified random inspections, allowing us to isolate firm-specific effects and circumvent endogeneity concerns, such as those arising in the context of collective bargaining. Additionally, we examine the interaction between labor market power and the (under-)provision of legally mandated workplace regulations, measured via training, safety, and health inspections. These non-wage amenities directly affect worker safety, particularly in hazardous environments, and are typically more difficult to observe than conventional job attributes such as overtime compensation or holiday policies. Finally, our results indicate that workplace inspections can serve as an effective policy tool to constrain labor market power, especially among large firms.

The remainder of the paper proceeds as follows. Section 2 introduces the theoretical framework. Section 3 provides an overview of the institutional context and a description of the data. Section 4 describes patterns in inspections and the randomization process. Section 5 shows characteristics of firms being found violating during inspections. Section 6 provides evidence that inspections increase compliance with workplace regulations and estimates the reduced-form effects of an inspection on firms and workers. Section 7 discusses potential mechanisms to explain our findings. Section 8 concludes.

[5] There is a more developed literature on how monopolistic market structures in the product market lead not only to higher prices but also to lower product quality (Spence, 1975; Crawford and Shum, 2007). Yet the analogous relationship between labor market power and job quality, including non wage attributes, is much less explored.

2 Theoretical Framework

In this section, we present a simple theoretical framework for assessing the factors that influence the firms' decisions to violate workplace regulations and how wages and firm employment respond to enforced compliance. In the model, a representative firm j with productivity z_j produces output $z_j f(n_j)$ with labor n_j. When hiring workers, the firm posts a compensation bundle consisting of a wage, w_j, and a non-wage amenity, a_j, that refers to working conditions. The government imposes a mandated minimum level of amenities per worker, denoted as a_{min}. However, enforcement of this minimum is imperfect, allowing firms to provide amenities below a_{min}. If a firm offers $a_j < a_{min}$ and is inspected, which occurs with probability p, it must pay a fine ϕ. Based on the institutional setting in Mexico (described in detail in Section 3) the expected fine $(p \cdot \phi)$ increases with the firm's total employment n_j and the extent of per worker non-compliance $(a_{min} - a_j)$ (STPS, 2014).[6] We assume firms are risk-neutral and normalize the price of output to one. Expected firm profits depend on the decision whether to provide amenities above or below the mandated minimum level a_{min} and are given by:

$$\Pi(w_j, a_j) = \begin{cases} z_j f(n_j) - w_j n_j - k a_j n_j & \text{if } \; a_j \geq a_{min}, \\ z_j f(n_j) - w_j n_j - k a_j n_j - p \cdot \phi n_j (a_{min} - a_j) & \text{if } a_j < a_{min}, \end{cases} \tag{1}$$

where k is the cost of each amenity unit per worker. We assume that $p \cdot \phi < k < \phi$.[7] If $a_j < a_{min}$, the firm provides a level of amenities below the mandatory level and is thus violating workplace regulations; otherwise, it is compliant. Following Lagos (2024), we assume the firm faces the following labor supply function, which is increasing in both components of total compensation.[8]

$$n^s(w_j, a_j) = \left(w_j^{\beta_w} \cdot a_j^{\beta_a}\right)^{\theta_j} \tag{2}$$

where $\beta_w + \beta_a = 1$. β_w and β_a define the relative elasticity of labor supply with respect to wages and amenities, and θ_j represents the elasticity of labor supply with respect to the value of a job, defined as: $\left(w_j^{\beta_w} \cdot a_j^{\beta_a}\right)$. A lower θ_j indicates greater monopsony power by the firm, leading to a lower job value. The ratio $\frac{\beta_a}{\beta_w}$ determines the composition of this value bundle in terms of wages versus amenities. We solve the model analytically with $f(n_j) = n_j$ in Appendix C and present the main predictions below.

Prediction 1. Compliance with workplace regulations. *If the firm faces a firm-specific labor supply function that is upward sloping in amenities ($\beta_a > 0$) and wages ($\beta_w > 0$), the optimal*

[6] We assume linearity in firm size and non-compliance for model tractability. This representation is isomorphic to a setting where fines depend on non-compliance and inspection probability increases with firm size.

[7] The price for each unit of amenity k per worker is greater than the expected fine per worker for each unit of forgone amenity, but smaller than just the per-worker, per amenity fine ϕ.

[8] We provide a microfoundation for this labor supply function in Appendix C. Note that this labor supply function assumes some level of complementarity between wages and amenities, consistent with empirical evidence (Dube et al., 2022; Maestas et al., 2023).

decision on whether to violate workplace regulations or not will be given by the following expressions:

Non-compliance (violation):
If $z_j < ka_{min} + (k - p\phi)a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a}\right)$ *the firm will violate and provide* $a_j < a_{min}$.

Constrained compliance:
If $z \in \left[ka_{min} + (k - p\phi)a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a}\right), ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)\right]$ *the firm will comply and* $a_j = a_{min}$.

Unconstrained compliance:
If $z_j > ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)$, *the firm will comply and provide* $a_j > a_{min}$.

A few notable patterns emerge from these expressions. First, all else equal, firms with higher productivity, z_j, are more likely to comply with workplace regulations. Second, all else equal and provided $k > p \cdot \phi$, firms with lower θ_j, meaning they possess greater labor market power, are more inclined to violate regulations. This occurs because workers in these firms are less responsive to unfavorable working conditions, allowing employers to reduce benefits without this impacting their capacity to hire and retain workers. Third, all else equal, non-compliance becomes more likely when $\frac{\beta_a}{\beta_w}$ is lower, as workers are relatively less sensitive to amenities than to wages. Fourth, all else equal, the prevalence of non-compliance decreases as the probability of inspection p increases, as stronger enforcement raises the expected cost of violating workplace regulations. Figure B.2 shows illustrative examples of firm profits as a function of working conditions a_j for different values of θ_j, our main parameter of interest. We focus on three values of θ_j that illustrate the profit function in the case where each of the cases is optimal: non-compliance, constrained compliance, and unconstrained compliance.

In Section C.1.4, we derive the optimal wages and amenities provided by firms in each case. In Section C.3.1, we show that if labor supply depends solely on the wage ($\beta_a = 0$), as in conventional monopsony models, the firm's decision to violate workplace regulations will only depend on the unit price of the amenity, k, on the probability of inspection, p and on the fine per worker ϕ. Thus, if we assume a_{min}, k, and ϕ are constant across firms, differences in compliance behavior should be entirely driven by variation in inspection probabilities, as in Ulyssea (2018).

Prediction 2. The effect of an inspection on non-compliant firms. *We model the enforcement of a regulation following an inspection as an increase in the inspection probability from* $p < 1$ *to* 1. *This shift forces previously non-compliant firms into constrained compliance.*[9] *For the newly complying firms:*

Prediction 2a. *In the absence of downward wage rigidity, wages will adjust downward.*

The expression for the proportional change in wages is given by:

$$\Delta\% w_j = \frac{z_j - ka_{min}}{z_j - p\phi a_{min}}\left(1 + \frac{\beta_a}{\frac{1}{\theta_j} + \beta_w}\right) < 1, \tag{3}$$

[9] By modeling enforcement in this manner, we assume that after receiving an inspection, a previously non-compliant firm remains compliant from the date of the inspection onward.

for all previously non-compliant firms.[10]

Prediction 2b. *If firms have labor market power over amenities, i.e. $\beta_a > 0$ and $\theta_j < \infty$ then employment may increase following enforcement.*

In Section C.2, we derive the effect of enforcement through inspections on wages and employment. The proportional change in employment is characterized by the expression in Equation (4) and may be positive if the reduction in wages is insufficient to offset the value of improved working conditions. The intuition is that when the firm-specific labor supply function is increasing in the amenity level a_j, an exogenous increase in a_j (due to enforcement of minimum working conditions) can generate a movement along the firm's labor supply function (see Equation 2), thereby increasing the quantity of labor supplied and raising employment. This mechanism is analogous to the standard monopsony model in which the imposition of a minimum wage can increase employment through a movement along the firm's labor supply curve (Manning, 2004).[11]

$$\Delta\% n_j = \left(\underbrace{\frac{a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p\phi)}{z_j - p\phi a_{min}}}_{>1} \right)^{\theta_j\beta_a} \left(\underbrace{\Delta\% w_j}_{<1} \right)^{\theta_j\beta_w} \lesseqgtr 1 \tag{4}$$

Note that in the presence of downward wage rigidity, possibly due to the presence of minimum wages, the proportional reduction in wages may be smaller, and, therefore, it is more likely that the proportional change in employment is positive.

In Section C.3, we solve the model under alternative scenarios: First, in Section C.3.1, we assume that labor supply responds only to wages ($\beta_a = 0$), as in a standard monopsony model (Card et al., 2018). Second, in Section C.3.2, we assume markets are perfectly competitive and firms take the value of a job as given, consistent with the theory of compensating differentials (Rosen, 1986). We show that in both cases the enforcement of benefits has an unambiguously negative effect on employment. Third, in Section C.3.3, we show that under the assumption that firms take wages as given and possess only amenity-setting power, enforcement can lead to positive effects on employment similar to our benchmark case. Therefore, observing a zero or positive employment effect from enforcement empirically suggests that firms have labor market power over amenities. Finally, in Appendix D, we generalize the labor supply function to a constant elasticity of substitution (CES) form, allowing for varying degrees of complementarity between w_j and a_j. While Prediction 2 relies on the Cobb–Douglas specification of the firm's labor supply in Equation (2), where the elasticity of substitution between wages and amenities is equal to one, we show that the enforcement effect on wages can even be positive when wages and working conditions are sufficiently strong complements for workers. In this case, inspections improve both wages and working conditions, leading unambiguously to an increase in labor supply and thus in firm employment.

[10] The expression is < 1 when $z_j < ka_{min} + (k - p\phi)a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a}\right)$, which is exactly the condition for firms to choose to be non-compliant.

[11] In this model, as labor depends on both w and a, a movement along the labor supply curve will occur along the three-dimensional space $\{w, a, n\}$. This can also be seen as a shift in the labor supply curve along the two-dimensional space $\{w, n\}$. In the standard monopsony model where labor supply depends only on w, a minimum wage increase induces a movement along the labor supply curve in the $\{w, n\}$ space.

In Section 5,we empirically test Prediction 1 by analyzing the firm-level characteristics that correlate with being found violating workplace regulations. In Section 6.3, we empirically assess Prediction 2 by estimating the effect of inspections on firm and worker outcomes. We show that both sets of results allow us to rule out both models of perfectly competitive labor markets and monopsony models in which labor supply responds only to wages.

3 Institutional Context and Data

3.1 Workplace Inspections in Mexico

The Mexican Secretariat of Labor and Social Welfare (STPS) conducts firm inspections to monitor and enforce compliance with workplace regulations (STPS, 2014). Most inspections are conducted by state-level branches of the STPS and generally focus on compliance in three main areas: health and safety regulations, training obligations, and general working conditions.[12]

Workplace inspections are classified into three main types: *ordinary* (stratified random), *extraordinary* (targeted), and *follow-up* inspections. In most cases, inspectors are assigned randomly across all inspection types, except when specific technical expertise is required (STPS, 2014). Ordinary inspections are typically announced 24 hours in advance and follow a scheduled plan. In contrast, extraordinary inspections are unannounced and initiated in response to workplace accidents, employee complaints, imminent risks, or suspected violations of labor laws. They may also be triggered by inconsistencies or false information found in previous inspections or employer-submitted documentation. Follow-up inspections occur after an initial visit and verify that identified violations are corrected and that the employer complies with any mandated remedial actions within a specified time frame.

This study focuses on ordinary inspections, for which firm selection follows a stratified random process. Throughout this paper, we use the terms "ordinary" and "stratified random" interchangeably. Each state-level STPS branch organizes firms listed in the National Firm Directory (DNE) into predefined strata, from which firms are randomly selected for inspection on a monthly basis. To ensure transparency, this selection process is conducted through the Labor Inspections Administration System (Sistema de Administración de la Inspección del Trabajo, or SIAPI) (STPS, 2015). These groups are stratified based on sector (25 categories), total employment (above and below 15 workers), years since last inspection (≤ 1, $(1, 2]$, $(2, 3]$, > 3), firm age (< 1 , $[1, 3]$, > 3), and risk class (5 categories).[13] This randomized approach provides a quasi-experimental setting for analyzing the effects of workplace inspections on compliance and firm-level outcomes.

If a violation is found during an inspection, labor inspectors grant employers deadlines ranging from 30 to 90 business days to correct identified non-compliance issues and provide necessary

[12] General working conditions include compliance with, e.g., providing formal work contracts, the minimum wage, the 13th salary (aguinaldo), mandatory vacation days, maximum working hours, mandatory breaks during the workday, payment of extra hours, and mandatory profit-sharing (STPS, 2013). In addition, firms in Mexico are mandated to provide training to their workers (LFT, 2021).

[13] This information on the variables used for the stratified randomization process was accessed through a right to information petition to STPS through Mexico's National Transparency Platform. We show their reply, including this information, in Appendix Figure A.4. The risk classification is determined by the firm's sector.

documentation. Extensions are allowed once for an equal period if requested in writing before the deadline, provided worker safety is not compromised. In cases where the violation poses an imminent danger for workers, the inspector should require immediate action to correct the violation, partial or total suspension of activities, or a restriction on worker access until necessary safety measures are implemented. If the employer fails to comply with the mandated measures or does not provide documentary proof of compliance within the specified deadlines, labor authorities will initiate an administrative sanction process. During this process, the employer may present a defense and submit evidence within ten business days. If the procedure results in a sanction, the employer will be subject to a fine. The amount of the fine will depend on factors such as whether the non-compliance was intentional, the severity of the violation and resulting damages, any prior infractions, and the company's financial capacity (STPS, 2014).[14] Fines may range from 50 to 5,000 times the Unidad de Medida y Actualización (UMA), a unit linked to the minimum wage, amounting to around 200 to 22,000 USD in 2018 (LFT, 2021).

3.2 Datasets

Inspection Data: Our primary dataset includes around 200,000 workplace inspections conducted by the STPS between January 2017 and June 2022. Firms are identified by their unique legal name and the state where the inspection takes place. The dataset provides details on the type of inspection (ordinary, extraordinary, follow-up), the topic of the inspection (health and safety, training, general labor conditions), and the outcome (no violation, violation without a fine, or violation with a fine). However, the exact fine amounts are not recorded. Approximately half of all inspections concern health and safety, while training and general labor conditions each account for about 25%. Although our main analysis focuses on ordinary inspections, we use all inspection records to calculate the time since the last inspection, which is important for stratification, as discussed in the previous subsection. Ordinary inspections account for approximately 23% of all inspections, with first-time ordinary inspections making up 50% of this subset. Inconsistencies identified during a first ordinary inspection can lead to extraordinary inspections, which may influence the timing and likelihood of future inspections. Therefore, we limit our main causal analysis in Section 6 to first-time ordinary inspections to mitigate potential endogeneity issues.

Monthly Manufacturing Establishment Survey (EMIM): Our main dataset for measuring firm-level outcomes is the Monthly Survey of Manufacturing Establishments (*Encuesta Mensual de la Industria Manufacturera*, or EMIM), which spans from 2017 to early 2023, collected and accessed through Mexico's national statistical office (INEGI). The dataset provides detailed monthly information on total employment, wage bills, production, revenues, and costs. Total employment measured in this survey includes both formal and informal workers. The survey follows a primarily deterministic design, with the same sample of establishments surveyed each month. For most sectors, the sampling methodology begins by ranking establishments within each 6-digit industry nationally by highest revenue. Establishments are then sequentially included in the sample until the

[14] Based on the inspection logs, a firm's financial capacity can be assessed using one of the following indicators: total number of workers, total profits, or total firm capital. In the model of Section 2, we model the fine as increasing in the number of workers.

survey captures a threshold level of national revenue—ranging from 60% to 85%, depending on the industry. The survey effectively functions as a monthly census of large, productive manufacturing establishments in Mexico, resulting in a balanced panel of around 9000 observations in our study.[15] Appendix Table A.1 reports summary statistics of this survey.

Additional Establishment Surveys: To complement the EMIM, we utilize two additional datasets collected by Mexico's national statistical office (INEGI). First, the Annual Survey of Manufacturing Establishments (*Encuesta Anual de la Industria Manufacturera*, or EAIM) provides yearly data on additional variables not captured in EMIM, such as profits and training expenditures. We draw on this survey to validate and extend our findings in Sections 6.2 and 6.3. Second, we use the 2019 Economic Census (carried out in 2018), which provides a comprehensive snapshot of all 4.6 million business establishments operating in Mexico that year, including small and informal firms across all sectors. Conducted every five years, the census contains rich information on firm-level characteristics such as revenues, value added, profits, investments, capital stock, workforce composition (including informal employees), and social security coverage.[16]

Administrative Employer-Employee Data (IMSS): Our second monthly establishment dataset consists of administrative records from the Mexican Social Security Institute (*Instituto Mexicano de Seguridad Social*, IMSS), accessed through the *Econlab* at Banco de México. It covers the universe of formal employment relationships in Mexico and is structured as a monthly panel, containing identifiers for firms, establishments, and workers. The dataset provides detailed information on each establishment (sector, municipality, and age) and each worker (gender, age, earnings, and contract type—permanent or temporary) from January 2004 to December 2023. Earnings are represented by the worker's daily taxable income (*salario base de cotización*), which includes various forms of compensation such as overtime, bonuses, commissions, the 13th salary (*aguinaldo*), and the mandatory vacation bonus (*prima vacacional*). However, it excludes profit-sharing benefits (PTU).[17] Earnings are bottom-coded at the minimum wage and top-coded at 25 UMAs (Units of Measure and Update).[18] This dataset does not include information on the number of hours or days worked per month. As of early 2015, it contains approximately 17 million worker identifiers and 850,000 unique firm identifiers (300,000 of which have at least six workers). We use this data to corroborate our findings on the effects of inspections on establishment-level wages and employment, as well as worker-level analyses, as discussed in Section 6.3. Appendix Table A.1 reports summary statistics of this data.

Accident Data: We also leverage data on the total number of accidents per municipality, month, and sector, provided by the Sistema de Avisos de Accidentes de Trabajo (SIAAT) and STPS. The dataset covers the period from January 2016 to April 2024, containing approximately 594,000

[15] Unfortunately, the statistical office (INEGI) was unable to provide information on the reasons for an establishment's exit from the sample (business closure vs. exit from the sample). However, attrition rates are very low, with approximately 1.5% of firms in the sample exiting each year.

[16] According to the statistical office, firms are promised strict data confidentiality to avoid misreporting in the survey.

[17] This exclusion was clarified in July 2023 by the Social Security Institute, which stated: *"Employee profit-sharing (PTU) is not part of the base salary, since according to Article 124 of the Federal Labor Law (LFT), it is not part of the integrated salary as stated in Article 84 of the LFT."* Only PTU exceeding the legal maximum of three months' salary is included in the base salary.

[18] `https://en.www.inegi.org.mx/temas/uma/` (last accessed 10/2025).

accident reports, with an average of more than 5,000 accidents per month (see Appendix Figure A.2a). 67% of accidents happen at the workplace, while 33% are commuting accidents. 250,000 of all accidents (42%) occur in the manufacturing sector. We use the number of accidents as one of several proxies for compliance with workplace regulations in Section 6.2, examining how inspections influence accident rates over time. We provide more descriptive results on accidents in Mexico in Appendix Figures A.1 and A.2.

Merging Datasets. A key step in constructing our main datasets involves merging inspection records with both establishment-level survey data and social security administrative data. To our knowledge, this is the first study to link administrative data on workplace inspections with detailed establishment survey data. To carry out this merge, we first match inspection logs with INEGI's National Statistical Directory of Economic Units (DENUE), a registry of all economically active units in the country, maintained through biannual on-site verification. Using unique legal firm names (*razón social*), we successfully match 77% of inspection records to DENUE. The DENUE contains a unique firm identifier used by INEGI (CLEE), which enables the link to the establishment-level survey data. We aggregate establishments at the firm level in each state, which is the level at which we observe workplace inspections. For the remainder of this paper, the term *firm* in the context of EMIM refers to the firm-state level.[19]

Table 1: Summary Statistics Inspections 2017-2022

Dataset	# firms	# firms inspected	# firms random insp	# inspections	% random inspections	% violations
Economic Census	4 628 822	36 171	13 445	124 177	23	23
Manuf. Survey EMIM	9 109	4 853	2 558	23 788	26	21
Manuf. Admin IMSS	8 930	3 280	1 598	13 594	24	22

Notes: This table shows the total number of establishments, inspected firms, randomly inspected firms, number of inspections, share of random inspections, and the share of violations found during inspections for the Economic Census, the manufacturing survey (EMIM), and the administrative manufacturing sample (IMSS) that is comparable to EMIM. Inspection data cover the years 2017-2022. The economic census includes all economic units that are active in 2018. The manufacturing survey and the administrative sample include all establishments that are active at some point between 2017 and 2022.

In a separate process, we merge inspection data with social security records. We follow Samaniego de la Parra and Fernández Bujanda (2024) in matching inspection records to the national establishment registry, the *Directorio Nacional de Empresas* (DNE), maintained by Mexico's Tax Administration Service (SAT), using unique legal firm names. This process yields a 91% match rate across all inspection records. The DNE contains the Federal Taxpayers Registry (RFC) firm identifier, which enables linkage to administrative employer-employee-wage records from IMSS.[20] In Table B.1, we

[19] We derive similar results when using establishment-level data or restricting the sample to firms with a single establishment per state (Appendix Figure A.13).

[20] The lower matching rate between the inspections data and DENUE than with DNE may be because inspections are randomized using the DNE as the population, and DNE may include establishments that do not appear in DENUE, primarily due to firms operating only briefly or remaining legally registered without economic activity. However, such cases are less likely in our context, which focuses on large manufacturing firms. Additional details on the matching process are provided in Appendix B.

show that the matched sample closely resembles the full inspection dataset in terms of inspection type, topic, and year.

Due to confidentiality restrictions, establishments in EMIM cannot be directly linked to those in IMSS. Instead, we construct an IMSS sample of around 9,000 manufacturing establishments comparable to EMIM. We explain the sample construction in more detail in Appendix B. Table 1 reports the number of matched inspections in the EMIM, Economic Census, and constructed IMSS datasets.

4 Inspection Probability and Randomization Process

In this section, we provide descriptive results on the distribution of ordinary inspections in Mexico in order to explore variation in inspection likelihood, and validate the stratified randomization process of inspections to establish the empirical foundation for our subsequent analysis. While inspections are randomly assigned within each stratification group, the likelihood of inspection may vary across groups if, for example, the Mexican Secretariat of Labor and Social Welfare (STPS) prioritizes certain sectors or firm size categories. In our setting, a key feature of ordinary inspections is that we know which characteristics STPS conditions on during the randomization process. This allows us to estimate the expected inspection probability, $\hat{p}_{gt}$, for firms in group g using information on the number of inspections conducted within a given period and the total number of firms in the stratum.[21] Appendix Figure A.5 displays the distribution of inspection likelihood across randomization groups between 2018 and 2021 for all firms in the Economic Census.[22] We observe substantial variation in inspection likelihood across randomization groups, suggesting that certain types of firms are more frequently targeted. Figure 1a further illustrates this pattern, showing that larger firms are more likely to be selected for an ordinary inspection, consistent with the notion that the expected costs of violations increase with firm size (Ulyssea, 2020). Finally, Figure 1b reveals that many subsectors within manufacturing exhibit particularly high inspection rates, although there is substantial variation. The manufacturing sector records the highest total and per-worker accident rates (see Appendix A.2), making it a natural target for enforcement efforts. This is reflected in the overall high inspection probabilities we observe for manufacturing firms. Our focus for the rest of this paper on large, productive manufacturing firms in Mexico aligns with this enforcement pattern and offers policy-relevant insights into the effects of targeting such firms.

While there is variation in inspection probability across randomization groups, the assignment of ordinary inspections within a group should be random. We empirically evaluate the validity of this stratified randomization process via the following univariate regression:

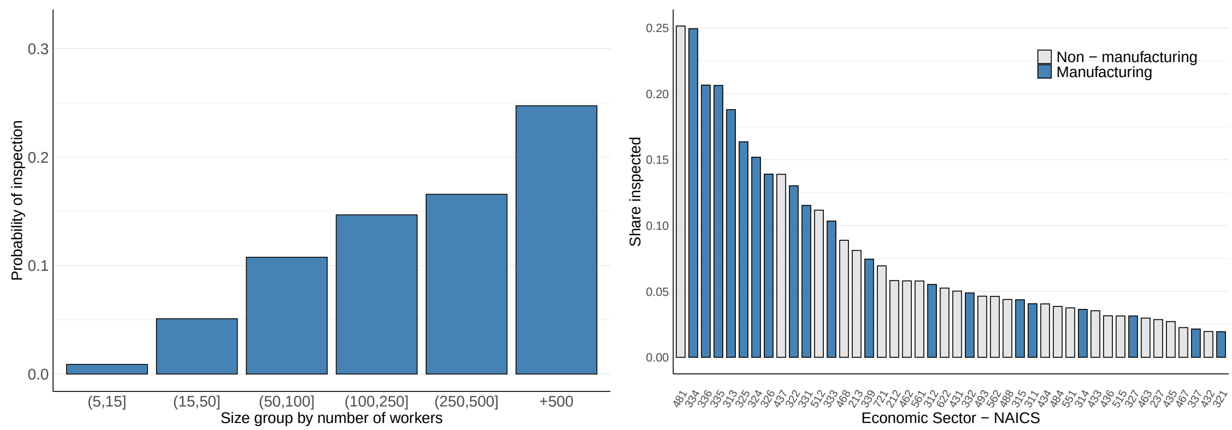
$$y_{i,t-1} = \beta \cdot inspection_{it} + \phi_{x,t-1} + \epsilon_{it}, \tag{5}$$

[21] We calculate the probability of inspection in a group in a certain year as the likelihood of being inspected at least once in the year, which is built as one minus the probability of not being inspected in any month of the year $= 1 - \left(1 - \frac{n\ monthly\ insp}{N_g}\right)^{12}$, where N_g is the number of firms in the randomization group and $n\ monthly\ insp$ is the average number of inspections carried out to firms in the randomization group each month. This probability measure accounts for the possibility of being inspected more than once.

[22] The estimated probability of inspection is strongly correlated across time. The correlation between $\hat{p}$ in 2018 and 2017 is 0.76, and between 2018 and 2019 it is 0.6.

where $y_{i,t-1}$ corresponds to a characteristic of firm i in the previous month, and $inspection_{it}$ is an indicator variable equal to 1 if the firm receives an ordinary inspection at date t (month-year). We estimate two different specifications, where $\phi_{x,t-1}$ is either a state-by-date fixed effect, or a stratified randomization group-by-date fixed effect. We compare the coefficient β across both specifications to evaluate the validity of the randomization. This procedure is analogous to a traditional means balance test between treatment and control groups in a stratified randomized controlled trial, but it explicitly accounts for the staggered timing of treatment in our design.[23]

Figure 1: Inspection Probability by Size and Sector



(a) Share of Firms Inspected by Size Group

(b) Share of Firms Inspected by Sector

Notes: This Figure shows the share of firms inspected in different size groups and 3-digit NAICS Economic Sectors. Sectors with less than 1.7% of firms inspected are excluded for visual purposes. Data from the 2019 Economic Census.

Table 2 reports the results for both the monthly manufacturing survey and the social security data. In Panel A, we document substantial differences between inspected and non-inspected firms when controlling for only state-by-date fixed effects. Specifically, inspected firms tend to be larger, employ fewer outsourced workers and women, exhibit lower average hours worked per worker, higher hires and separations, and a higher average wage. In contrast, Panel B shows that once randomization group fixed effects are included, these differences largely disappear and lose statistical significance.[24] The absence of significant differences between treated and control firms within each randomization stratum supports the notion that inspections are indeed randomly assigned within strata. Appendix Table A.3 reveals analogous patterns when focusing exclusively on *first* ordinary inspections, which form the core of our causal analysis in Section 6. As a placebo test, Appendix Table A.2 replicates the analysis for non-random inspections in the monthly manufacturing survey. In this case, differences between inspected and non-inspected firms persist even after accounting for randomization group-by-date fixed effects. These findings support the validity of the stratified randomization process for firm inspections.

[23] Stratified randomization is a stronger assumption than conditional parallel trends, our necessary assumption in the causal analysis of Section 6.3.

[24] Panel B shows a 10—13% reduction in sample size due to singleton groups—randomization groups containing only treated or control firms. Nonetheless, the resulting estimates are statistically insignificant not because of inflated standard errors but due to attenuated coefficient magnitudes.

Table 2: Correlation Between Survey Firm Characteristics and Ordinary Inspection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	EMIM						IMSS			
	Total workers	VA per worker	Revenue	Share blue collar	Share outsourced	Avg hrs worked	Share women	Separations	Hires	Mean salary
Panel A: Without randomization group fixed effects										
	70.37***	7.440	14,744.5***	-0.0037	-0.0690***	-0.0105***	-0.0167**	18.11***	17.73***	86.35***
	(20.86)	(8.313)	(5120.9)	(0.0043)	(0.0087)	(0.0012)	(0.00742)	(3.562)	(3.815)	(8.616)
N	201,267	200,336	200,070	164,448	200,336	198,813	190,710	190,710	190,710	190,710
Panel B: With randomization group fixed effects										
	26.07	18.34	11,449.6**	-3.22e-5	0.0041	-0.0010	-0.00681	2.753	2.961	10.99
	(22.83)	(13.13)	(4970.9)	(0.0039)	(0.0035)	(8e-04)	(0.00611)	(4.031)	(4.208)	(7.626)
N	181,900	180,935	180,769	148,391	180,935	179,487	165,605	165,605	165,605	165,605

Notes: This Table shows the results from estimating a regression using the sample of firms from EMIM (Columns 1-6) and IMSS (Columns 7-10) in 2018 and 2019, where we regress a firm-level outcome measured in month $t-1$ indicated in the different columns on a binary variable indicating an ordinary inspection in month t. The regressions in panel A include state-by-year-by-month fixed effects. The regressions in panel B include randomization-group-by-year-by-month fixed effects. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

5 Characteristics of Violating Firms

In this section, we examine the characteristics of manufacturing firms found to be non-compliant with workplace regulations. While compliance cannot be observed across the full sample, we identify violations within the sample of ordinary inspected firms. We first describe the key characteristics of these violating firms. Then, we test Prediction 1 from Section 2 relating non-compliance to productivity, labor market power, worker preferences, and inspection probability. First, we use a structured approach by selecting variables that serve as empirical counterparts to those in Prediction 1 and assessing their correlation with observed violations. Second, we employ a complementary data-driven approach where we evaluate whether the variables introduced in Prediction 1 indeed help predict observed violations.

Between 2018 and 2019, approximately one out of every four (ordinary) inspected firms in the manufacturing survey are found to be in violation. Figure A.7 shows that violation rates are highest in inspections related to safety and health, and above 20% in almost all sectors within manufacturing. The analysis of the correlation between firm characteristics and violations presents two empirical challenges. First, our sample of observed violations is drawn from the subset of firms that underwent inspection. Thus, there is potential selection bias when estimating the unconditional relationship between firm characteristics and violation rates (Heckman, 1979). Second, as shown in Section 2, a firm's decision to violate workplace regulations depends not only on its own economic characteristics, such as productivity and labor market power, but also directly on its (perceived) inspection probability. Consequently, when analyzing the determinants of non-compliance, it is important to consider that certain characteristics can directly and indirectly influence the firm's decision to violate through the probability of inspection.[25] To address both challenges, we control for each firm's estimated probability of inspection, described in Section 4. This approach allows us to isolate the relationship between firm characteristics and violation rates while accounting for differential inspection probabilities. This approach additionally allows us to assess the correlation between inspection probability and violation decisions. In particular, we run the following regression:

$$violation_{igt} = \delta X_{i,2018} + \gamma \hat{p}_{g,t-1} + \varepsilon_{igt} \tag{6}$$

where $violation_{igt}$ is an indicator for whether the firm is found violating workplace regulations during the inspection, $X_{i,2018}$ are pre-determined firm characteristics from the 2018 Economic Census, and $\hat{p}_{g,t-1}$ is the firm group's predicted probability of inspection before realized inspection. We correlate a rich set of predetermined firm characteristics from the 2018 Economic Census with the probability of being found violating during an ordinary inspection conducted after the Census. The 2018 Economic Census provides three key advantages over EMIM and IMSS for this analysis. First, it covers the universe of formal and informal establishments, enabling a more accurate measurement of local labor market characteristics. Second, it provides a more comprehensive set of firm characteristics. Third, it allows for mild data restrictions, allowing us to focus on all relatively large establishments with firm size above 20 in the manufacturing sector. We report the results of univariate regressions of Equation (6) in Appendix Table A.5. Specifications with and without controlling for the estimated probability of inspection are included. We categorize each explanatory variable based on whether it is related to a firm-specific characteristic, the firm's workforce, or the firm's local labor market, defined as municipality-by-2-digit-NAICS-sector cells.

We find that firms with a smaller investment in training per worker are more likely to violate. Insufficient training provision in itself can be a cause of violation; thus, this result reinforces that our violation variable indeed represents non-compliance with workplace regulations. In addition, firms with lower profits and lower value-added per worker are more likely to be found violating, consistent with Prediction 1 from the model that lower productivity z_j is associated with higher non-compliance. Furthermore, firms that rely more heavily on outsourcing are more likely to be found in violation, potentially because outsourcing facilitates avoidance of labor benefits, and has also been shown to correlate with high monopsony power (Casco et al., 2024; Estefan et al., 2024).[26] This correlation also relates to Prediction 1 that firms with higher labor market power are more likely to be non-compliant with workplace regulations. Moreover, firms with a lower proportion of female workers are more likely to violate, even when controlling for sector and state fixed effects. This may be because women value good working conditions relatively more than men (Maestas et al., 2023), interpreted as a higher β_a in our model, or because women's labor supply is more inelastic (Sharma, 2023). Finally, violating firms seem not to provide workers with higher wages, in line with previous literature showing that jobs with bad amenities do not necessarily compensate workers with

[25] For instance, a positive correlation between labor market concentration and violations could be driven by market power, as outlined in Section C, or simply by the fact that more concentrated labor markets receive fewer inspections, prompting firms to violate due to low enforcement.

[26] This period of analysis precedes the 2021 outsourcing ban, meaning that outsourcing in itself was not a violation at the time.

higher wages (Sorkin, 2018; Bell, 2025). While it is not a primary prediction of our model, this relationship indicates some complementarity between wages and working conditions in equilibrium, as monopsonistic firms offering lower wages are also more likely to violate workplace standards.

Regarding labor market conditions, firms operating in more concentrated labor markets and those with higher market shares are more likely to be found violating workplace regulations. These results align with the findings in Marinescu et al. (2021), who show that local industries with higher violation rates pay lower wages and exhibit more labor market concentration. High levels of market share and concentration have been shown to correlate with labor market power (Berger et al., 2022). Therefore, the results also align with our theoretical predictions in Section 2 indicating that firms with greater labor market power are more likely to violate workplace regulations.

The results remain robust when controlling for inspection probability (column 2), quintiles of inspection probability fixed effects (column 3), as well as sector and state fixed effects (column 4). In column 5, we use propensity score re-weighting (Hirano et al., 2003) instead of linear controls for inspection probability, yielding similar results and revealing a significant negative correlation between violations and firm age and average wage, as well as a positive correlation with average hours worked. Columns 6 and 7 show that the results remain robust when restricting the sample to inspections in which the firm is found in violation and sanction proceedings are initiated (see Section 3).

We do not find a statistically significant correlation between inspection probability and the likelihood of a firm being found violating workplace regulations. This may be because our measure of inspection probability is derived from the number of realized inspections per randomization group over the past year. This measure may differ from firms' perceived probabilities of inspection, potentially introducing measurement error and attenuation bias in our estimates. Nonetheless, the coefficient on inspection probability is consistently negative across specifications, consistent with the theoretical prediction in Section 2 that firms facing higher inspection probabilities are less likely to violate workplace regulations.

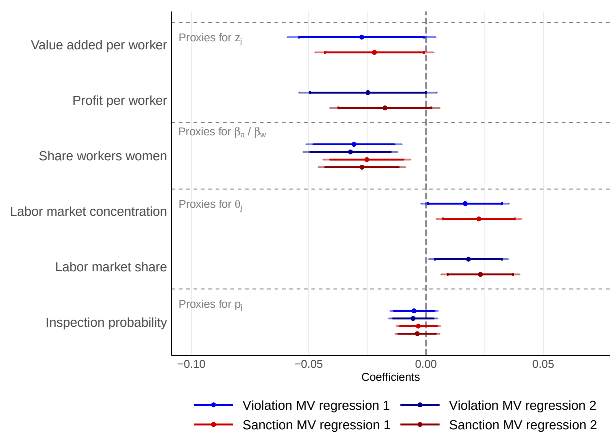
Given the large set of potentially correlated firm-level characteristics, we employ two different approaches to select a subset of variables to include in multivariate regressions. First, we rely on the structure of our theoretical framework to guide variable selection. Specifically, we include variables that serve as proxies for the characteristics identified in Prediction 1 as determinants of workplace regulation violations. The results from this multivariate regression can be seen in Figure 2a, where we indicate which variable serves as a proxy for each relevant variable in the model. Reassuringly, the main results from the univariate regressions in Table A.5 hold. In line with Prediction 1, firms more likely to be found violating: have a low value added per worker or profits per worker, proxies for productivity (low z_j); have a higher market share or operate in highly concentrated markets, proxies for labor market power (low θ_j); and have a lower share of female workers, a proxy for lower $\frac{\beta_a}{\beta_w}$. Coefficients for inspection probability p_j are negative, as predicted, though statistically insignificant.

Second, we adopt a data-driven approach using Lasso regularization to assess whether the variables determining non-compliance in the model are indeed predictive of violations. We then include variables selected by Lasso in multivariate Ordinary Least Squares (OLS) regressions, again controlling for inspection probability.[27] All regressions use heteroskedasticity-robust standard errors,

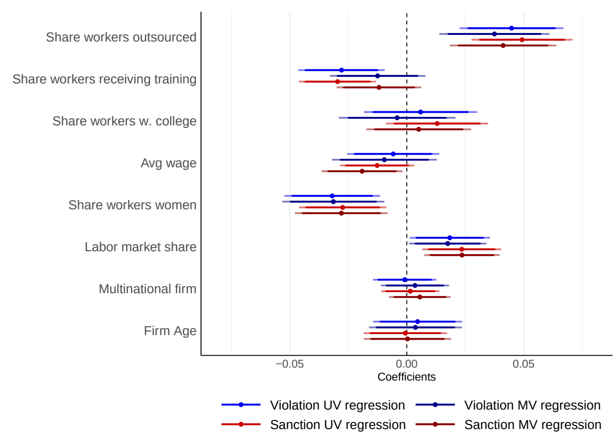
[27]The OLS post-Lasso estimator performs at least as well as Lasso in terms of the rate of convergence, while offering

following Belloni et al. (2014).[28] The coefficients from this multivariate OLS regression can be seen in Figure 2b. In line with our results from Appendix Table A.5 and our theoretical model, we again find that firms with higher levels of outsourcing, higher labor market shares, and a lower share of female workers are more likely to be found violating workplace regulations. The coefficients related to worker training become insignificant in multivariate regressions, further suggesting that low worker training is a direct proxy for poor working conditions, likely to be strongly correlated with other firm characteristics that predict violations. While the effect of higher wages is mostly not significantly associated with violations, this coefficient turns significantly negative in multivariate regressions for severe violations that include a fine. Neither profits nor value added per worker, two proxies for firm productivity, survive the lasso regularization. This may be explained by the positive correlation between wages and firm productivity. Appendix Figure A.8 shows that results are robust when controlling for state and sector fixed effects.

Figure 2: Coefficients of Violation Regressions



(a) Variables from Model Prediction



(b) Variables from Lasso Selection

Notes: This figure shows the coefficients and 90% and 95% confidence intervals from estimating regressions where the outcome is a binary variable indicating that a violation was found during the inspection (blue) or that a violation was detected *and* the sanction procedure was initiated (red). The explanatory variables are a subset of firm characteristics from the 2018 Economic Census. Coefficients are estimated on a sample of manufacturing firms with over 20 workers that receive an ordinary inspection between 2019 and 2021. Panel (a) shows results for two multivariate regressions for each outcome variable. In each MV regression, variables are selected based on Prediction 1 from Section 2. The empirical counterparts for some of the model parameters differ in each regression. z_j is proxied with VA per worker in regression 1 and profit per worker in regression 2. Labor market power is proxied with labor market concentration and labor market share in regressions 1 and 2, respectively. Panel (b) shows results from univariate regressions and multivariate regressions, where variables are selected using Lasso regularization. All regressions control for the estimated probability of inspection. All explanatory variables are standardized with mean 0 and standard deviation of 1.

Overall, these findings align with the theoretical framework presented in Section 2, which emphasizes the importance of firm productivity, worker preferences, inspection probability, and labor market power in influencing firms' decisions to provide bad working conditions.

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the advantage of a smaller bias (Belloni et al., 2010).

[28] Belloni et al. (2014) show that standard OLS inference remains valid after variable selection using Lasso.

6 Effect of Workplace Inspections

6.1 Empirical Strategy

In our main reduced-form analysis, we estimate the causal effect of first-time stratified random inspections on firm outcomes using a staggered difference-in-differences design. The treatment is random, stratified by the firm's group in the month prior to inspection (g_{t-1}). We apply the local projection difference-in-differences (LP-DID) approach (Dube et al., 2025). This methodology has two features that are particularly relevant for our setting. First, it allows for the estimation of causal effects in a staggered treatment setting. Second, unlike standard event-study TWFE specifications or most alternative estimators proposed in the recent literature, LP-DID allows us to control for pre-treatment values of time-varying covariates. This feature is crucial in our context because it enables us to control for the randomization group in the month prior to inspection, which varies over time as years since the last inspection, firm age, and firm size change. An advantage of this approach is that it is computationally fast, which is particularly important in our setting with many randomization group fixed effects.

We estimate separate regressions for $h \in [-12 : -2] \cup [0 : 11]$:

$$y_{i,t+h} - y_{i,t-1} = \beta^h \Delta D_{it} + \gamma^h_{g,t-1} + e^h_{it}, \tag{7}$$

where $y_{i,t+h}$ is the outcome of firm i at month-year $t + h$ and g is the randomization group of firm i at $t - 1$. $D_{it} = 1$ marks post-treatment periods, and $\Delta D_{it} = 1$ indicates the month of the first stratified random inspection. Thus, β^h can be interpreted as the effect of a first ordinary inspection in t on the outcome in $t + h$. The parameter $\gamma^h_{g,t-1}$ is a randomization group by date fixed effect.[29] Following Dube et al. (2025), we restrict the sample to newly treated observations ($\Delta D_{it} = 1$) or clean controls—those not yet or never treated ($D_{i,t+h} = 0$). The identifying assumptions of this approach are conditional no anticipation and conditional parallel trends (Dube et al. (2025), section 4.1), which, we argue, should hold after conditioning on randomization group fixed effects because of the random nature of the treatment. We apply this LP-DID approach to the monthly manufacturing sample (EMIM) and the comparable monthly administrative employer-employee data (IMSS), and cluster standard errors at the firm level.

The monthly manufacturing survey (EMIM) covers around 9,000 establishments each month, of which 980 receive their first random inspection between 2018 and 2019. We construct a comparable IMSS-based sample from aggregated EMIM information (as described in Appendix B), resulting in a balanced monthly panel with similar number of observations. Since we approximate the EMIM sample using average wages—a noisy proxy for productivity—we likely capture a similar but not identical group of firms. We observe fewer first inspections (490) in the sub-sample from the IMSS data and therefore extend the window through 2020, resulting in 740 first random inspections.[30]

[29] Importantly, 92% of firms receiving a first ordinary inspection belong to a randomization group which has both inspected and non-inspected firms. This within-group variation allows us to identify the effect of a first ordinary inspection on firm outcomes. The distribution of the share of firms inspected across randomization groups for treated firms can be seen in Appendix Figure A.6.

[30] We find similar but noisier results for considering only 2018-2019 in IMSS. EMIM results are robust to considering

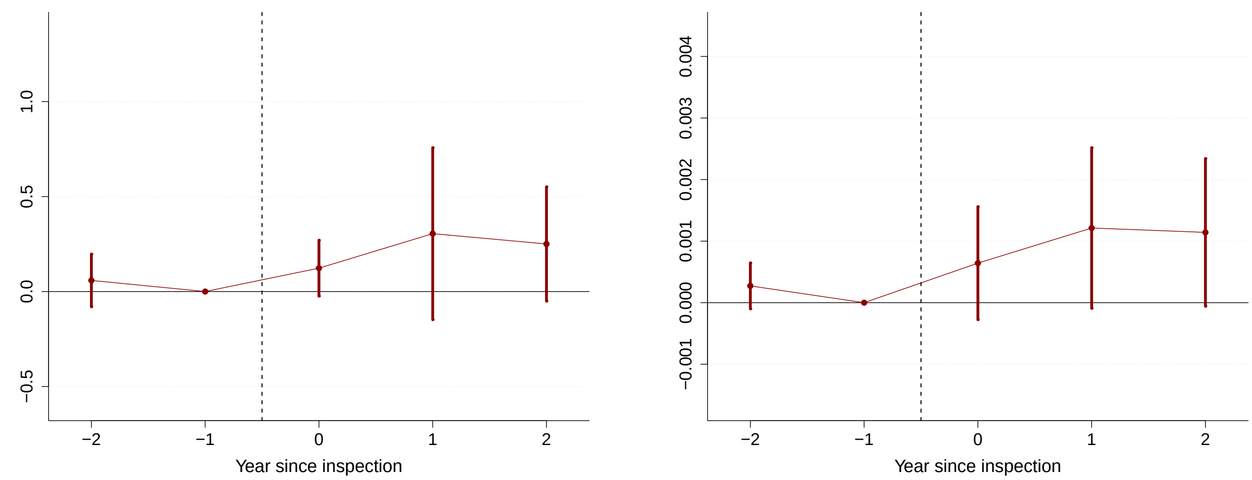
Finding similar results across both datasets increases confidence that our estimates are not driven by sample selection specific to EMIM.

6.2 Effect of Workplace Inspections on Compliance

The Mexican Labor and Social Security Secretariat (STPS) conducts workplace inspections to check and improve compliance with labor standards. Hence, the effect of inspections on compliance is an outcome of first-order importance. Measuring compliance with workplace regulations is generally challenging. Much of the existing literature has relied on survey-based estimates of informality—a specific form of non-compliance—and a few studies focus on field experiments involving small samples of firms. While we cannot directly measure compliance, we present four types of evidence that firms increase compliance after being inspected.

First, we analyze follow-up inspections, a specific type of inspection aimed at verifying compliance with workplace regulations after a violation is identified. Approximately 40% of inspections where a violation is found and firms are provided with additional time to fix their insufficient working conditions receive a follow-up inspection.[31] 82% of these follow-up inspections happen within 12 months of the inspection with the violation. These patterns suggest that follow-up inspections serve as a mechanism for the labor authorities to verify compliance after violations are detected.

Figure 3: Effect of Inspections on Training Costs



(a) Training Costs per Worker (b) Training Costs over Total Costs

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the yearly level for $h \in [-2:2]$. The outcome variables are training costs per worker and training costs as a percentage of total costs at the firm level. Regressions are estimated based on a balanced sample of firms from EAIM. Treated firms are those receiving a first inspection between 2018 and 2019. Control firms are those untreated until period h. Standard errors are clustered at the firm level.

Second, we assess the impact of inspections on worker training expenses, a direct and indirect proxy for compliance with workplace regulations. It is a direct measure as firms in Mexico are

first ordinary inspections from 2018-2020.

[31] In contrast, only around 6% of inspections in which no violation is found are followed by a follow-up inspection.

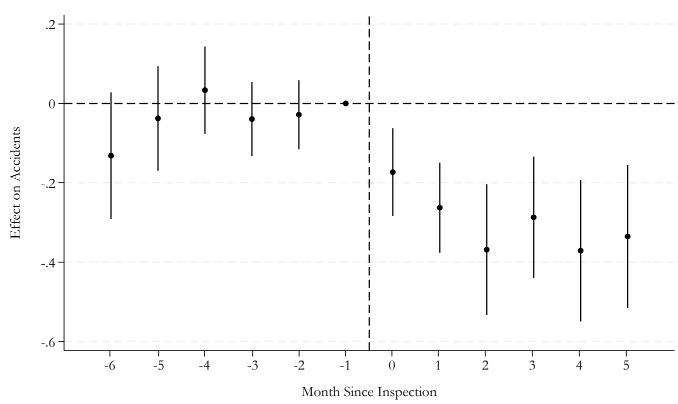
obliged by the Federal Labor Law (LFT, 2021) to provide annual training to their workers, and certain workplace inspections specifically check for compliance with this mandate. It is also an indirect proxy for compliance with workplace regulations, as better-trained workers should be more capable of securely operating machines in the manufacturing sector. We observe annual worker training expenses and apply our main empirical approach from equation (7) at the yearly level. Figure 3 depicts the effect of workplace inspections on firms' average training costs per worker and training costs over total costs. We observe that firms tend to increase their training costs following an inspection. This effect aligns with firms improving working conditions rather than just learning how to conceal poor practices.

Third, we find that stratified random inspections are associated with fewer accidents at the municipality level. While firm-level accident data is unavailable, we use month-municipality-level records on accident counts, as well as accident types or sectors. Out of 2,466 municipalities, we retain 1,588 that have at least one recorded accident, of which 1,003 had at least one ordinary inspection between January 2017 and June 2022. Similar to our main specification in Equation (7), we apply a LP-DID approach.[32] We estimate separate regressions for each month since inspection $h \in [-6, -2] \cup [0 : 5]$

$$WorkAccidents_{m,t+h} - WorkAccidents_{m,t-1} = \beta^h RandomInspections_{mt} + \gamma_t^h + \delta_m^h + v_{mt}^h, \quad (8)$$

where γ_t^h and δ_m^h capture monthly and municipality fixed effects, respectively, and v_{mt}^h is the error term. The coefficient β^h can be interpreted as the effect of an inspection in t on the number of accidents in $t + h$.

Figure 4: Effect of Inspections on Work Accidents



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (8) at the monthly level for $h \in [-6 : 5]$. The outcome variable is the number of work accidents at the municipality level. Regressions are estimated based on a balanced sample of municipalities from the accident data. Treated municipalities are those receiving a stratified random inspection between January 2018 and June 2019. Control municipalities are those with no new inspections until period h. Standard errors are clustered at the municipality level.

[32] Compared to our firm-level analysis, we cannot control for the inspection probability via the randomization groups.

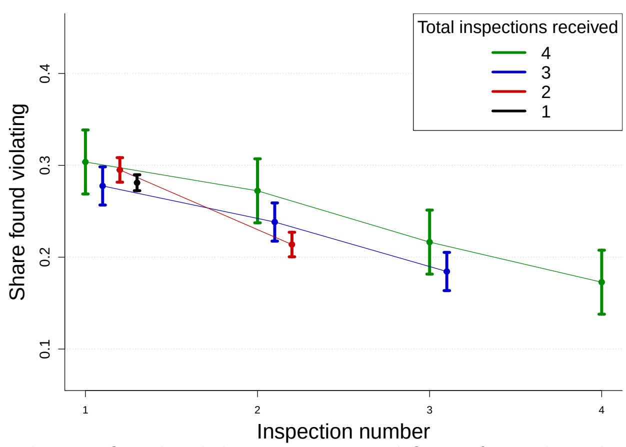
The regressions are based on a balanced sample, considering ordinary inspections from January 2018 until June 2019.[33] We keep only clean controls, i.e., municipality-month observations without additional inspections until $t + 5$.[34] Figure 4 depicts our main result. One more ordinary inspection in a municipality is associated with a decrease of around 0.3 (12%) accidents in this municipality compared to the pre-inspection month. The effect is statistically significant at the 5% level for all post-inspection periods, while the coefficients for periods before the inspection are not significantly different from 0. The effect size gradually decreases during the first three months after an inspection and remains constant thereafter. In Appendix A.9, we show that our results are robust to different changes in treatment, outcome, or considered time period.

Fourth, we examine how the likelihood of being found violating decreases in the number of ordinary inspections a firm has undergone via the following regression:

$$Violation_{in}^{(N)} = \sum_{n=1}^{N} \beta_n^{(N)} D_{in}^{(N)} + \zeta_{in}^{(N)}. \tag{9}$$

We estimate this model separately for each group of firms with total inspections $N \in \{1, 2, 3, 4\}$. Therefore, the sample to estimate each coefficient stays the same within each group of total inspections. Here, $Violation_{it}^{(N)}$ is a binary variable equal to one if firm i—which ultimately receives a total of N ordinary inspections—is found in violation during its n-th inspection, and $D_{in}^{(N)}$ is an indicator for that inspection number. The coefficients $\beta_n^{(N)}$ therefore represent the average violation rate at inspection number n for firms with total inspections N. Standard errors are clustered at the firm level.

Figure 5: Share of Firms Violating, by Inspection Number



Notes: This figure is constructed using firm-level data on inspected firms, from the subsample of inspections merged to DENUE. It displays the coefficients and 95% confidence intervals from regressing a binary variable indicating whether a violation occurred during inspection n on a variable indicating the number of ordinary inspections the firm has received up until inspection n. The sample is restricted to firms that received N ordinary inspections in total. Each color corresponds to a different $N \in [1 : 4]$. Standard errors are clustered at the firm level.

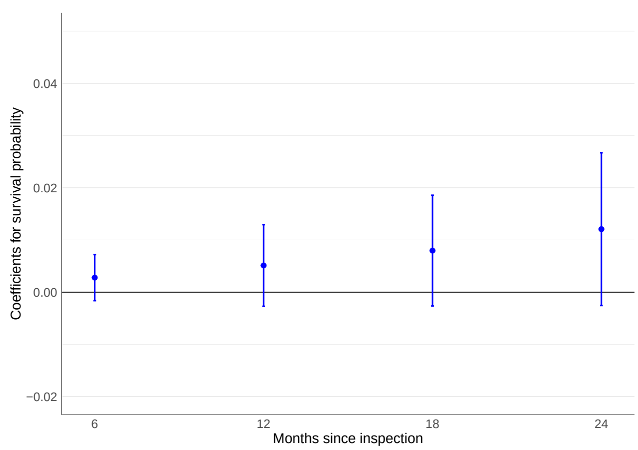
[33] In contrast to our main analysis, we do not consider inspections after June 2019 due to the pandemic.

[34] Further increasing h leads to similar results but reduces the effective sample size.

Figure 5 reports the estimated violation rates, showing the proportion of firms with a violation in their first, second, third, and fourth inspections (n), separately by the total number of inspections received (N). This allows us to compare coefficients within a group (same color) without different sample selections, as firms receiving a different number of total inspections (across colors) are likely to be quite different. This figure suggests that a firm's probability to be found violating workplace regulations decreases in the number of inspections they have already received. For instance, a firm is 4 to 8 percentage points (13–26%) less likely to be found in violation during its second inspection compared to its first.[35]

Firm Exit. We next address the potential concern that our findings on compliance and firm outcomes in the subsequent section are driven by firm exit. If less productive firms unable to comply with workplace regulations are more prone to exiting the market after an inspection, this attrition could bias our sample and, consequently, our results. As depicted in Figure 6, our analysis reveals that firms subjected to inspections do not exhibit a lower survival probability within 6, 12, 18, or 24 months post-inspection compared to their non-inspected counterparts in the monthly manufacturing survey (EMIM). In this context, we exclude firms that have not yet been inspected from the control group to avoid survival bias, as their continued operation is guaranteed until inspection. These findings are consistent with results by Samaniego de la Parra and Fernández Bujanda (2024) who show that labor inspections in Mexico do not affect firms' probability of survival in the formal sector. These results corroborate our earlier institutional analysis, indicating that inspections aim to promote compliance rather than generate revenue through penalties or induce firm closures.

Figure 6: Effect of Inspection on Firm Survival



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [6, 12, 18, 24]$. The outcome variable is an indicator variable equal to 1 if the firm has not exited at $t + h$. Regressions are carried out on a sample of firms from EMIM that have not yet exited until time t. Standard errors are clustered at the firm level.

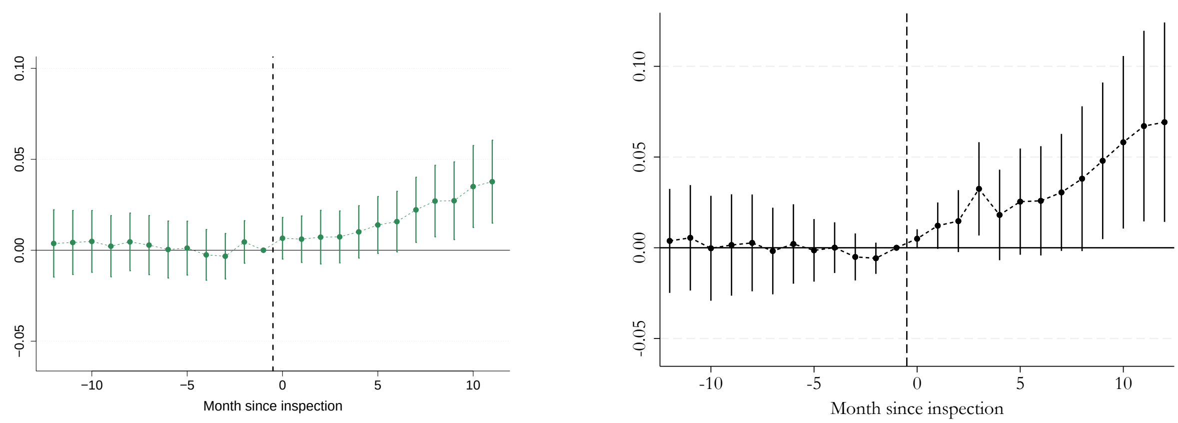
[35]In comparison, we report in Appendix Figure A.10 that the firm's probability of being found violating workplace regulations does not decrease with the number of extraordinary inspections.

6.3 Effect of Inspections on Firm Outcomes

6.3.1 Employment

Having shown that inspections tend to improve compliance, we now turn to evaluating Prediction 2 from Section 2 by estimating the impact of inspections on firm-level outcomes, beginning with total employment. Figure 7 displays the estimated coefficients from a LP-DID model for log total employment, over a 12-month pre- and post-inspection horizon. Panel (a) shows that in the EMIM sample, inspected firms experience a statistically significant increase in firm employment of approximately 4% one year after the inspection.[36] We observe qualitatively similar results in the IMSS data in Panel (b), where firm employment rises by about 7% one year following an inspection.[37] Appendix Figures A.11 and A.12 demonstrate that this positive effect on employment persists in the long run as well as for different aggregations on the firm- or establishment-level.

Figure 7: Effect of Inspections on Employment



(a) Log Firm Employment (Monthly Survey) (b) Log Firm Employment (Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. The outcome variable is log total firm employment. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020, or IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2019 in EMIM or between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

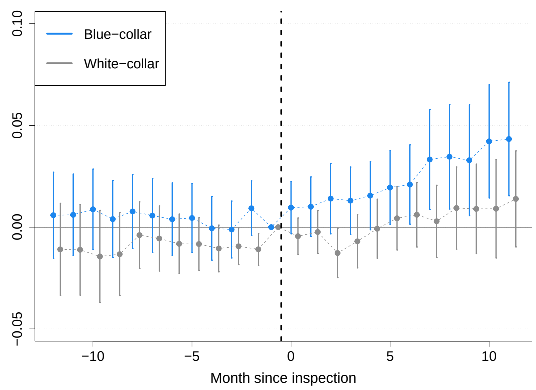
As shown in Panel 8a the increase is primarily driven by growth in blue-collar employment. In contrast, we find no statistically significant effect on white-collar employment. This is consistent with the focus of workplace regulation enforcement on areas such as safety, health, training, which predominantly affect blue-collar roles. Furthermore, Figure 8b reveals that the observed rise in employment is largely due to an increase in permanent contracts. This is consistent with the notion that, in the presence of regulatory enforcement, firms have incentives to hire permanent workers, since training and compliance investments are more easily recouped in longer-term employment relationships. These results highlight that it is crucial to consider compositional effects in interpreting

[36] Given an average pre-inspection firm size of 422 workers, this increase corresponds to roughly 17 additional workers.

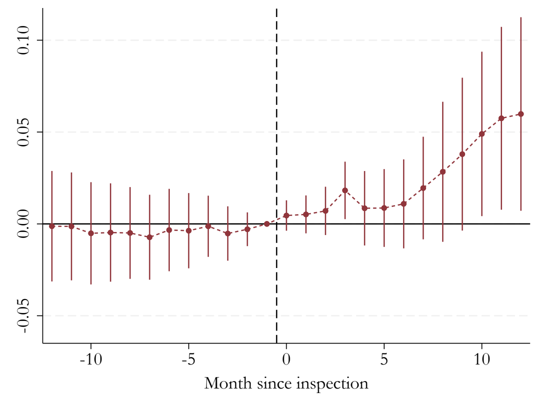
[37] This corresponds to an increase of roughly 42 workers, given an average firm size of 600 workers pre-inspection.

the overall employment impact. Finally, Figure A.23 shows that inspections have no effect on the share of outsourced workers. While this may appear at odds with the findings in Section 5, which documents a positive correlation between outsourcing and violation probability, both results are consistent with the institutional framework. Outsourcing likely facilitates the provision of substandard working conditions but was not itself illegal during this period.

Figure 8: Effect on Type of Employment



(a) Log Blue-collar and White-collar Workers (Monthly Survey)



(b) Log Permanent Workers (Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variables are total blue-collar workers and total white-collar workers at the firm in EMIM. In panel (b), the outcome variable is the total number of permanent workers in IMSS. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020 and from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2019 in EMIM or between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

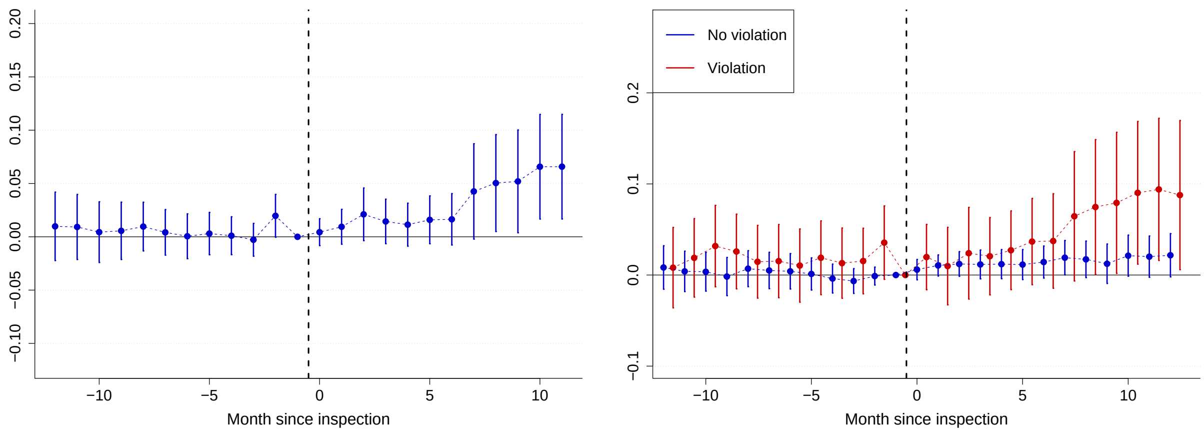
Figure 9a, shows that a similar increase in blue collar employment is observed when focusing exclusively on ordinary inspections related to safety and health. The estimated effects for inspections targeting training and general working conditions are presented in Appendix Figure B.6. We do not find statistically significant increases in employment one year after inspections in these two categories, although the magnitude of the effect for training inspections is comparable to that of safety and health inspections. Furthermore, we separately estimate the effects of inspections on the number of blue-collar workers for firms being found violating workplace regulations and for those not found violating. As shown in Figure 9b the positive employment effects are concentrated among firms found to be violating workplace regulations, which likely implement adjustments in response to the inspection.[38] These results are also consistent with our finding in Figure A.7, which shows that violation rates are highest for safety and health inspections—areas that predominantly concern blue-collar working conditions.

Appendix Figure A.13 demonstrates that these results are robust to several alternative specifica-

[38] If these firms had not complied after the inspection, we would not expect an increase in firm size, since poor working conditions would continue to make them unattractive to workers. This finding therefore provides additional evidence that inspections improve compliance with working conditions.

tions: extending the inspection period, restricting the sample to firms with only one establishment per state, and incorporating various sets of fixed effects within the EMIM sample. Overall, these results align with Prediction 2 in Section 2, which posits that an increase in firm employment is consistent with firms exercising monopsony power over non-wage job attributes.

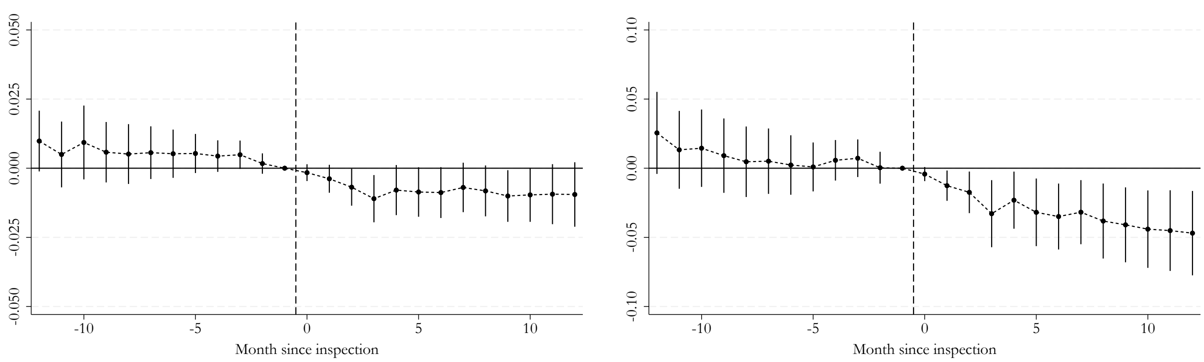
Figure 9: Heterogeneity Effects by Type of Inspection and Outcome



(a) Safety and Health Inspections (b) Log Blue-collar Workers - Violating vs Not

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variable is total blue-collar employment for safety and health inspections in EMIM. In panel (b), the outcome variable is the number of blue-collar workers, and we divide the treated firms into those that were found violating during the inspection and those that were not in EMIM. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019 in EMIM. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

Figure 10: Effect on Workforce Experience and Tenure

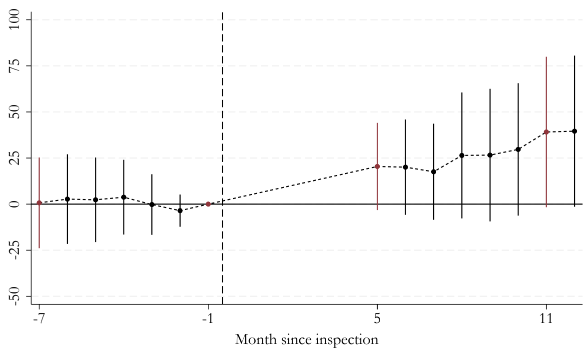


(a) Log Average Formal Labor Market Experience (b) Log Average Firm Tenure

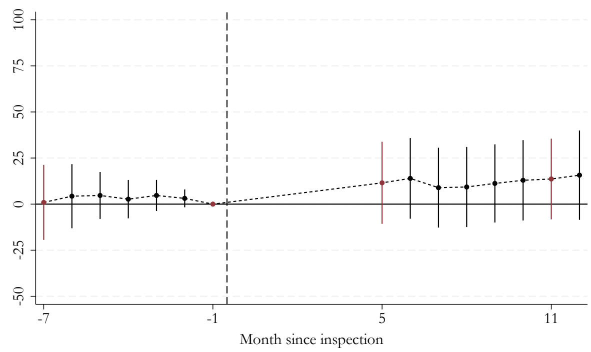
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variable is average formal labor market experience at the firm level. In panel (b), the outcome variable is average worker tenure at the firm level. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

The observed increase in firm employment affects workforce composition beyond the type of contract. Figure 10 shows a decrease in average firm tenure by around 5% and in labor market experience by around 1%. While the latter can be explained by the increase in blue-collar workers who are likely to be younger with a more recent entry into the labor market (see Appendix Figure A.21), the former also depends on the effect of an inspection on hires and separations. The analysis of hires and separations faces two key challenges. First, EMIM does not include information on hires or separations, which restricts our attention to administrative employer-employee matched data from IMSS. Second, hires and separations are flow variables and tend to be more volatile than stock variables such as total firm employment. To mitigate this issue, we aggregate hires and separations over a six-month period.

Figure 11: Effect on Hires and Separations



(a) Total Hires in the past 6 Months (Monthly Admin Data)



(b) Total Separations in the past 6 Months (Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variable is the sum of hires in the past 6 months. In panel (b), the outcome variable is the sum of separations in the past 6 months. Coefficients between 0 and 5 are not reported as they include hires from pre- and post-treatment periods. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

We present the estimated effects of first ordinary inspections on cumulative hires and separations in Figure 11a and Figure 11b, respectively.[39] While we cannot fully rule out that inspections caused a change in separations, this evidence suggests that a rise in hires helps explain the post-inspection increase in firm employment.[40] In Appendix Figure A.15, we show that the hiring rate—defined as the sum of total hires over the past six months divided by current firm size—is positive in post-inspection periods 5 and 11, although the estimates are not statistically significant. The firm-level

[39]The coefficient at -7 reflects the sum of hires (or separations) from 12 to 7 months before the inspection, while month -1 serves as the baseline period, representing the sum from months -6 to -1. The coefficient at month 5 corresponds to the post-inspection sum from months 0 to 5, and the coefficient at month 11 reflects the period from months 6 to 11. We also report intermediate coefficients before the inspection to test for the presence of pre-trends and include post-inspection coefficients between months 5 and 11 to examine the persistence of effects.

[40]The absence of a significant decline in separations could reflect a compositional shift within the firm. Improvements in working conditions following inspections may lower quit rates. However, if blue-collar workers, who account for the bulk of the employment increase, tend to have higher turnover rates, this could mechanically increase firm-level separation rates, offsetting the first effect.

separation rate remains close to zero and statistically insignificant.

Appendix Figure A.16 shows that the observed increase in hires is driven by both firm-to-firm moves and by individuals who are not registered in the IMSS system in the prior month (e.g., unemployed or out of the labor force) but who have prior formal sector experience. In contrast, the share of new hires coming from individuals never previously registered in the IMSS system is close to zero and statistically insignificant. As we discuss further in Section E, these results suggest that the increase in firm employment is unlikely to be driven by formalization alone.

Overall, the results in this section are in line with Prediction 2 in Section 2. When firms exercise monopsony power over non-wage job attributes, improved working conditions can raise total workforce through an increase in labor supply. Consistent with this mechanism, Figure A.14 in the Appendix shows that the positive employment effects are stronger for firms in more concentrated local labor markets and for firms with a high labor market share, both serving as proxies for greater labor market power. Because the positive effect is primarily driven by increased hiring, the evidence supports a mechanism in which better working conditions attract more workers. Section 7 presents direct empirical evidence that working conditions influence workers' labor supply decisions. In Mexico's National Employment Survey (ENOE), a majority of workers in large formal firms report finding their jobs through family and friends, suggesting that information about better working conditions is likely to be available to workers. However, if firms respond to improved working conditions by adjusting wages downward, workers may not value these improvements in working conditions sufficiently to increase their labor supply. In the next subsection, we examine the effect of first ordinary inspections on firms' labor costs and find no evidence of lower wage setting.

6.3.2 Labor Costs

Prediction 2a in Section 2 suggests that the enforcement of higher working standards can lead firms to reduce wages in the absence of adjustment frictions. This follows from the imperfect substitution between wages and working conditions in the labor supply function, which allows firms to decrease wages in response to an enforced improvement in working conditions. The extent to which firms adjust wages depends on the relative elasticity of labor supply with respect to working conditions and wages.

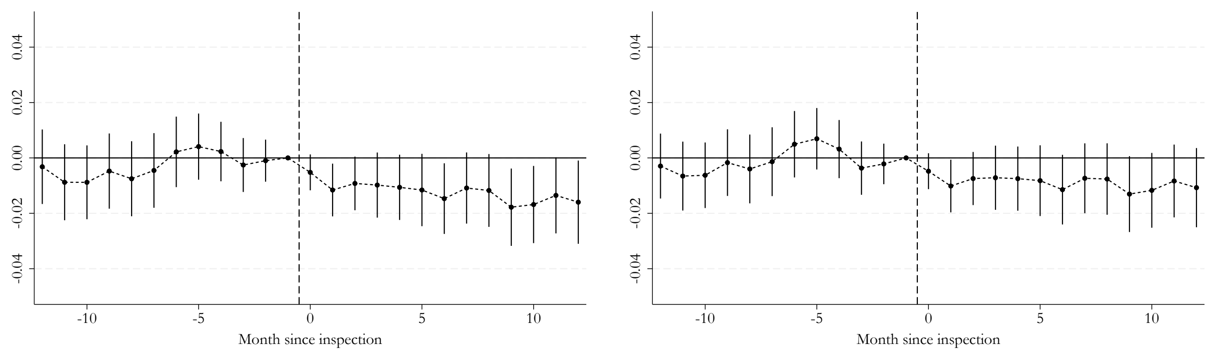
Consistent with an increase in firm employment, we observe a mechanical increase in total labor costs of around 3% one year after an inspection using both the monthly survey and social security data in Figure A.17.[41] However, we do not observe a significant change in the average labor costs per worker in EMIM. Panel (a) of Figure A.18 shows precisely estimated coefficients on average labor costs centered at zero, ruling out declines greater than 1.7%. This result may reflect three key factors: (i) labor benefits typically constitute a relatively small share of total firm costs and are measured in survey data with more noise than variables such as total employment; (ii) the effect on individual workers' wage-setting might be small; and (iii) changes in workforce composition following inspections—specifically, an increase in the share of blue-collar workers—may affect average labor costs, depending on how their wages and benefits compare to the firm average. To further explore

[41] Survey data from EMIM include benefits in total labor costs such as social security and profit-sharing on a monthly level.

the latter factor, we separately estimate the effect of inspections on average labor costs for blue- and white-collar workers. In Panels (b) and (c) of Figure A.18, we find no statistically significant effects for either group.

To gain additional insights into the effect on wage setting, we turn to administrative employer-employee data from IMSS, which offers more precise measures of individual wages. Figure 12a presents evidence of a statistically significant decline in the average firm wage one year after an inspection, which again reflects combined effects on wage setting and workforce composition. The estimated effect is less than -2% , with a lower bound of approximately -3%.[42]

Figure 12: Effect on Average Firm Wage



(a) Log Average Firm Wage (Monthly Admin Data) (b) Log Average Firm Wage (Monthly Admin Data) Controlling for compositional changes

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variable is the average firm wage. In panel (b), the outcome variable is the average firm wage, controlling for the average workforce year of entry into IMSS, the log number of men, age, tenure, and formal work experience. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

We next investigate whether the estimated decline in average firm wages reflects changes in workforce composition. While IMSS does not include explicit indicators for worker type (e.g., blue- vs. white-collar), we can control for observable characteristics related to workforce composition, including average year of first IMSS entry, log number of male workers, average age, tenure, and formal sector experience.[43] After including these five controls in the specification (Figure 12b), the estimated effect on average firm wage decreases to approximately -1% and is no longer statistically significant at the 95% confidence level. These results suggest that the negative wage effect is largely accounted for by shifts in workforce composition rather than changes in pay for comparable workers.

We can further disentangle the effects of composition and wage setting by assessing the impact of inspections on individual wages for both new and incumbent workers. First, we conduct a worker-level regression using a balanced panel of approximately 800,000 workers who are continuously employed at the same firm (treated or control) for at least 24 consecutive months. Similar to the firm-level

[42] We find similar effects for median firm wage and report the effect on different wages percentiles in Appendix Figure A.19. In addition, we find similar effects in the long-term or on the firm by state level, reported in Appendix Figure A.20.

[43] We report the effect on average firm age and year of worker registration in IMSS in Appendix Figure A.21.

analysis, we estimate the effect of first-ordinary inspections at the worker-level through separate regressions for $h \in [-12:-2] \cup [0:11]$:

$$y_{i,j,t+h} - y_{i,j,t-1} = \beta^h \Delta D_{jt} + \gamma^h_{gt-1} + \epsilon^h_{ijt}, \tag{10}$$

where $y_{i,j,t+h}$ is the log wage of worker j in firm i at month x year $t+h$ and g is the randomization group of firm i at $t-1$. $D_{jt} = 1$ marks post-treatment periods, and $\Delta D_{it} = 1$ indicates the month of the first stratified random inspection. Thus, β^h can be interpreted as the effect of a first ordinary inspection in t on the outcome in $t+h$. γ^h_{gt-1} is a randomization group x date fixed effect. Figure 13a compares the effect of first-time ordinary inspections on the wages of incumbent workers in treated to control firms and reveals a precise null effect. This finding aligns with labor law protections: under Article 51, Section IV of the Federal Labor Law, a reduction in nominal wages is illegal and constitutes just cause for resignation, entitling the employee to full severance benefits (LFT, 2021).

Second, we estimate the effect of the first ordinary inspection on new hires' wages. We defined treated workers as those who switch from a firm that has not yet been inspected to one that has already been inspected. Control hires are those switching between not-yet-inspected or never-inspected firms. We restrict treated and control workers to be employed in the firm for at least 12 months before and after the switch, respectively. We use the month of the job change as the event time in a difference-in-differences framework to compare how being hired from a firm with potentially unenforced workplace regulations to one with potentially enforced better working conditions affects workers' wages. We estimate

$$Y_{jt} = \alpha_j + \phi_y + \theta \text{Treat}_j \cdot \text{Post}_t + \epsilon_{jt}, \tag{11}$$

where Y_{jt} is log wage of worker j in period t relative to being hired by an already inspected ($Treat$) or not yet or never inspected firm, controlling for individual (α_j) and year of hire (ϕ_y) fixed effects.[44] Figure 13b shows no negative effect on workers' wages when being hired by an already inspected to a not yet or never inspected firm. This suggests that firms do not reduce wages when hiring into improved working conditions.[45] Given the null wage results for incumbent workers, which parallels the effect on incumbent workers, the absence of wage effects among new hires is in line with Article 86 of the Mexican labor code, which requires equal pay for equal work performed under the same conditions (LFT, 2021).

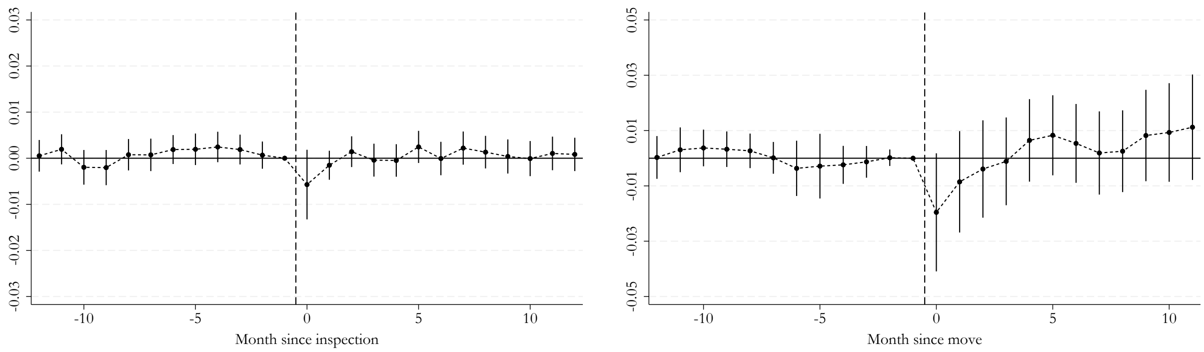
Overall, we observe a mild decrease in the average firm wage, which is likely driven by compositional changes rather than active wage setting. The absence of downward wage adjustments despite improvements in working conditions can be attributed to downward nominal wage rigidity in the given institutional setting. Notably, the results are in line with recent evidence (Dube et al., 2022), who analyze the reverse relationship—whether minimum wages influence non-wage benefits— and find no evidence of adjustment following a minimum wage increase. Appendix Figure A.25 presents

[44] The results remain unchanged when including "origin" firm randomization group $\times$ date fixed effects from the month prior to the job change, since these are not time-varying at the individual level and are absorbed by the individual fixed effect.

[45] We report basic difference-in-differences graph including parallel pre-trends for new workers in Appendix Figure A.22.

descriptive month-to-month wage comparisons, showing that nominal wages almost never decrease in Mexico. In Section D, we develop an alternative explanation for the absence of wage effects, based on the complementarity between wages and working conditions. We develop an extension of our model which allows for different degrees of complementarity between wages and working conditions: for example, good working conditions may be more valuable when wages are higher. This extension predicts that if wages and working conditions are strong complements, enforcement can have a neutral or even positive effect on wages.

Figure 13: Effect on Log Wage of Incumbent or New Workers



(a) Log Average Firm Wage of Incumbent Workers (Monthly Admin Data)

(b) Log Average Firm Wage of New Workers (Monthly Admin Data)

Notes: This Figure shows estimates of the effect of first ordinary inspections on wage changes for incumbent and new workers, respectively. Panel (a) reports β^h and 95% confidence intervals from estimating Equation 10 at the monthly level for $h \in [-12 : 11]$ for a balanced sample of workers staying at least 24 months within a control or eventually treatment firms. The outcome variable is the log of worker wage. In panel (b), the outcome variable is the log wage of hires who were employed for at least 12 months at firms that had not yet been or never would be inspected, and who subsequently switch to either an already inspected firm or another not yet or never inspected firm, remaining in the new firm for at least 12 months. Estimates are based on a balanced sample of workers from IMSS between 2017 and 2021 using the month of the job change as the event time in Equation 11. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

6.3.3 Production Process, Revenues, and Profits

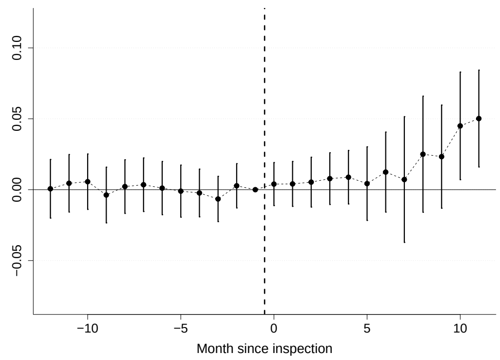
Figure 14 shows that the increase in firm employment is accompanied by significant rises in total hours worked, plant capacity utilization, total material costs, and total revenue. These real effects on the firm's production process may partly reflect a mechanical consequence of higher employment levels. They also indicate that the employment gains are not merely capturing formalization of previously informal workers, but rather a genuine expansion in firm size, as discussed in more detail in Appendix Section E.

We find no significant change in revenue per worker as shown in Appendix Figure A.23. While the coefficients for firm profits over value added are negative, the estimates are not statistically distinguishable from zero at the 95% confidence level, reported in Appendix Figure A.24.[46] This

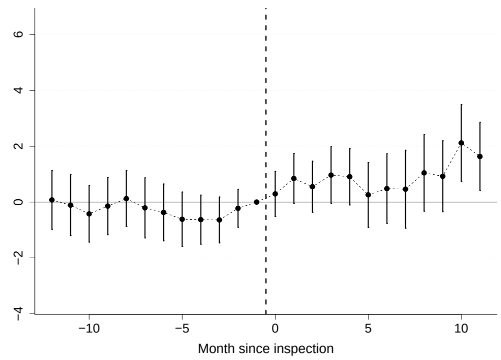
[46] We use the ratio of profits to value added, a measure of firm-level profit margins, as our main outcome variable, following prior studies such as Daruich et al. (2023); Nimier-David et al. (2023).

suggests that the observed increase in total costs—including labor, material, and compliance-related costs—is not fully offset by the rise in total revenue (Figure 14d), potentially leading to a modest decline in firm profitability.

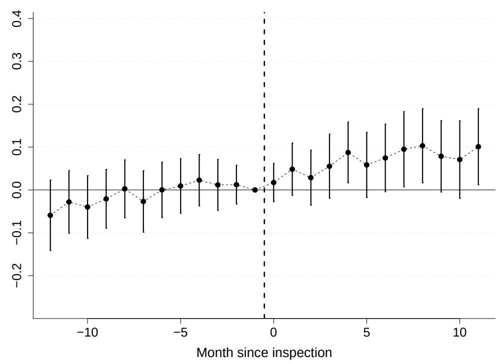
Figure 14: Effect on Production Process and Revenues



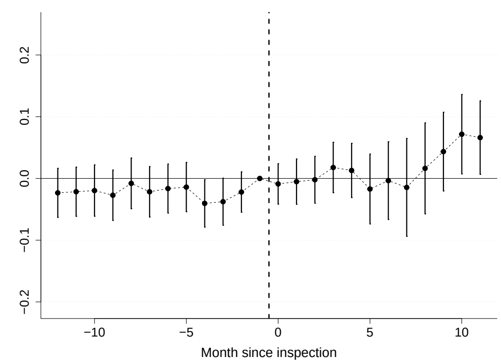
(a) Log Total Hours Worked



(b) Percent of Plant Capacity in Use



(c) Log Total Material Costs



(d) Log revenue

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variable is total hours worked. In panel (b), the outcome is plant capacity used. In panel (c), the outcome is total material costs. In Panel (d), the outcome is total revenue. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

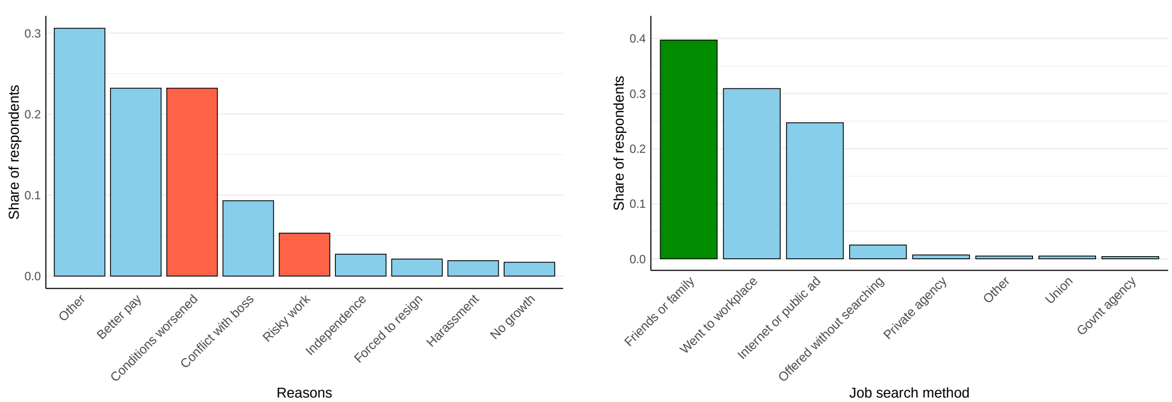
7 Mechanisms

Working conditions and labor supply. The results in Sections 5 and 6.3 are consistent with a monopsonistic labor market, in which labor supply depends positively on both wages and non-wage labor conditions. In this setting, employers facing a firm-specific labor supply function can maximize profits by offering substandard working conditions without fully offsetting them through higher wages, unlike in a perfectly competitive labor market. When better working conditions are mandated without corresponding wage reductions, the effective labor supply increases. As a result, large and productive—but previously inefficiently small—firms increase in size following inspections, despite incurring higher labor and compliance costs, including those related to worker training.

As outlined in Section 2, the mechanism driving our main results rests on the assumption that labor supply is upward sloping in working conditions. In this section, we provide evidence of two necessary conditions for this assumption to be valid. First, good working conditions are valued by workers and affect labor supply decisions. Second, workers are aware of a firm's working conditions when deciding to work for a firm, implying that the increase in firm size driven by an increase in hires can be explained by labor supply.

To empirically assess whether working conditions play a role in workers' labor supply decisions, we first draw on survey data from Mexico's National Survey on Occupation and Employment (ENOE), a nationally representative household survey conducted by INEGI. In this survey, workers report their reasons for quitting a previous job. We restrict the sample to workers who indicated that they had quit from their previous job, and whose previous job had been a formal firm in the private sector (N = 3477).[47] Panel (a) of Figure 15 illustrates the distribution of these responses for the 2018–2020 period.[48] We find that poor working conditions are a key reason for quits alongside better pay. Hazardous work environments—directly aligned with our empirical findings—also play an important role, with 5% of workers explicitly citing workplace risks as their main reason for quitting. This finding is consistent with our empirical evidence in Figure 9b, which shows that the increase in firm employment is concentrated on firms found violating during inspections. Prior to inspection, these firms do not meet minimum working condition standards and are therefore likely less attractive to workers than compliant firms. Following inspections, improvements in working conditions likely improve these firms' attractiveness, leading to an increase in labor supply and firm employment.

Figure 15: Job Quits and Search



(a) Reasons for Quitting Previous Job

(b) Search Method for Current Job

Notes: This Figure shows the distribution of responses to two questions from the National Occupation and Employment survey (ENOE). Panel (a) refers to the question: *What was the main reason for quitting your previous job?* and restricts the sample to workers who quit their previous job, excluding those that quit for family reasons, and whose previous job was at a formal firm in the private sector (N = 3477). Panel (b) refers to the question: *How did you find out about your current job?* and restricts the sample to workers in private formal firms in the manufacturing sector with over 20 employees (N = 46,779). The results are estimated based on responses from the first trimester of the years 2018-2020.

[47]We exclude people who indicated that they had quit their job for family reasons, as our focus is on firm characteristics explaining separation rates.

[48]This question is only included in a long version of the ENOE questionnaires, which is carried out in the first trimester of every year.

The second key assumption is that workers possess information about a firm's working conditions when deciding whether to accept a job. In practice, however, such information may be costly to acquire or imperfectly observed at the time of job search. We provide suggestive evidence on this point using data from the 2018-2020 waves of the ENOE, which includes a question on how individuals learned about their current job.[49] Panel (b) of Figure 15 displays the distribution of responses. Over 40% of workers report learning about their job through family or friends—sources likely to convey information about working conditions that would not be disclosed in a typical job advertisement.

To further explore the salience of working conditions in workers' job selection, we conducted a complementary survey on Prolific with a sample of 638 Mexican workers. Details on the sample characteristics can be found in Colonna and Aldeco (2025). Respondents were asked to indicate the job characteristics they considered when choosing their current employment. Table A.6 reports summary statistics, disaggregated by income group. Roughly 14% of respondents cite working conditions and nearly 35% highlight training opportunities as a salient factor in their employment choice.

Taken together, these patterns suggest that working conditions, training opportunities, and safe working environments are meaningful components of workers' labor supply decisions. Moreover, workers are very likely to have information on these firm characteristics when deciding whether to work for the firm.

Alternative mechanisms. In Appendix E, we consider three main alternative mechanisms that could potentially account for the observed increase in employment following inspections: (i) the formalization of previously informal workers, (ii) a mechanical increase in labor required for compliance, and (iii) firm misinformation regarding the potential costs and benefits of improving working conditions. We provide empirical evidence suggesting that these explanations are either unlikely to fully account for the magnitude of the observed effects, particularly in terms of employment, or they must operate within a framework in which firms exert monopsony power over working conditions to explain the employment increase post inspection.

First, formalization is unlikely to explain the increase in employment. The EMIM survey is designed to capture both formal and informal employment, and we observe a real increase in production without affecting productivity or social security payments per worker. Moreover, we observe that the increase in employment is particularly driven by hires from other formal firms instead of outside IMSS. In addition, the strongest employment effects follow inspections related to safety and training, rather than those targeting general labor conditions (including informality).

Second, while some inspections may lead firms to hire additional personnel to meet specific safety or compliance standards, such requirements are limited in scope and unlikely to explain the increase in total employment. Importantly, even in such cases, net employment gains would require firms to possess market power over working conditions.

Third, firm non-compliance may partly stem from misinformation, e.g., on the costs associated with dismissal regulations (Bertrand and Crépon, 2021). However, for enforcement to induce an increase in employment under such circumstances, firms must still face an upward-sloping labor supply function with respect to working conditions. Hence, even this mechanism relies on the presence of monopsonistic features in the labor market.

[49] The sample consists of workers employed in private manufacturing firms with more than 20 employees (N = 46,779).

8 Conclusion

Ensuring compliance with workplace regulations—such as safety, health, training, and general labor conditions—remains a persistent concern for worker welfare, even among formal firms. This paper contributes to understanding how labor market power helps explain non-compliance with workplace regulations and shapes the consequences of enforcement for firms and workers. We address this question by combining stratified random inspection records with rich administrative and survey data covering large manufacturing firms in Mexico. We find that firms that invest less in worker training, employ a lower share of female workers, have lower productivity, and have a greater labor market share are more likely to be found violating workplace regulations during inspections. We show that enforcement through these inspections enhances compliance, as evidenced by increased investment in training, reductions in workplace accidents, and a lower probability of future violations. An inspection leads to a 4–7% increase in firm-level employment within one year. Average firm wages decrease only modestly, primarily due to compositional changes rather than changes in wage-setting. These empirical results align with a theoretical model in which firms use their market power to offer below-minimum workplace standards. When enforcement improves working conditions and wages are rigid, firm employment rises through labor supply responses, as workers value improved working conditions.

These results carry important implications. A common view is that workplace regulations and their enforcement raise firms' labor costs and thereby reduce employment, creating a trade-off between firm performance and better working conditions. We show, however, that the presence of monopsony power over working conditions can mitigate this tradeoff: in our setting inspections raise compliance, expand firm employment, and can mitigate labor market power. These findings can inform the optimal design of targeting inspections, particularly in settings with limited enforcement capacity, where governments may seek not only to maximize detection rates to improve worker well-being but also to target inspections toward firms where enforcement delivers the greatest economic returns relative to its costs.

Ultimately, the efficiency implications of enforcement depend on general equilibrium effects, including the reallocation of workers across firms, which is beyond the scope of this paper. The optimal design of labor inspections is thus a promising avenue for future research. Moreover, this paper remains agnostic about the underlying sources of labor market power over working conditions—whether driven by heterogeneous worker preferences, the salience of job attributes, or other factors. Advancing our understanding of these determinants is another important research direction, as it would guide the design of complementary policies to curb labor market power.

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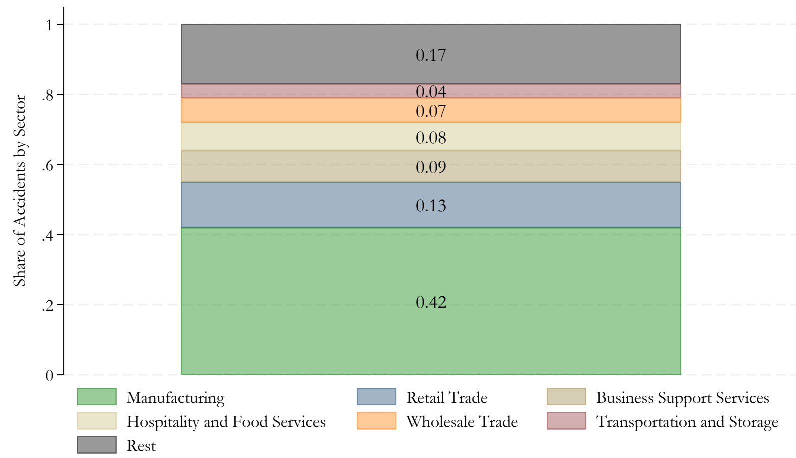
A Appendix: Additional Tables and Figures

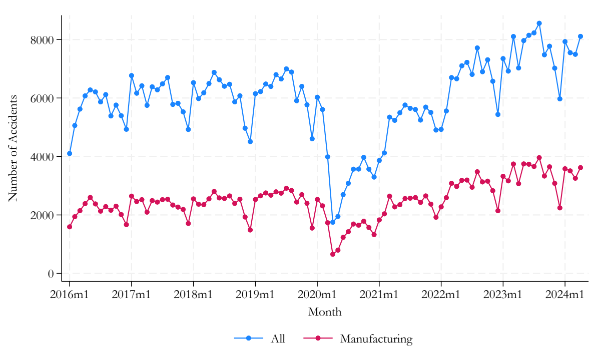
A.1 Figures Appendix

Figure A.1: Share of Accidents by Sector

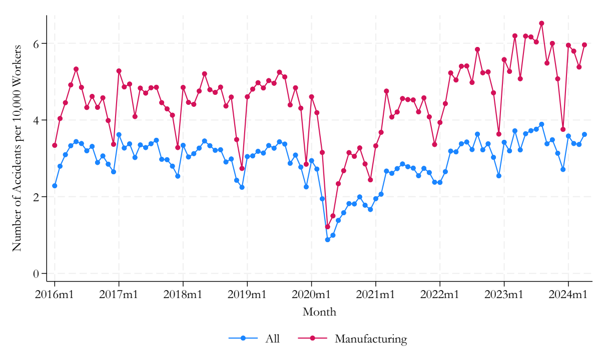


Notes: This Figure shows the share of total work-related accidents by sector. The accident data covers the period from January 2016 until April 2024, accessed via the Sistema de Avisos de Accidentes de Trabajo (SIAAT) and the Mexican Secretariat of Labor and Social Welfare (STPS). The "Rest" category aggregates 15 sectors with the lowest accident shares.

Figure A.2: Accidents over time



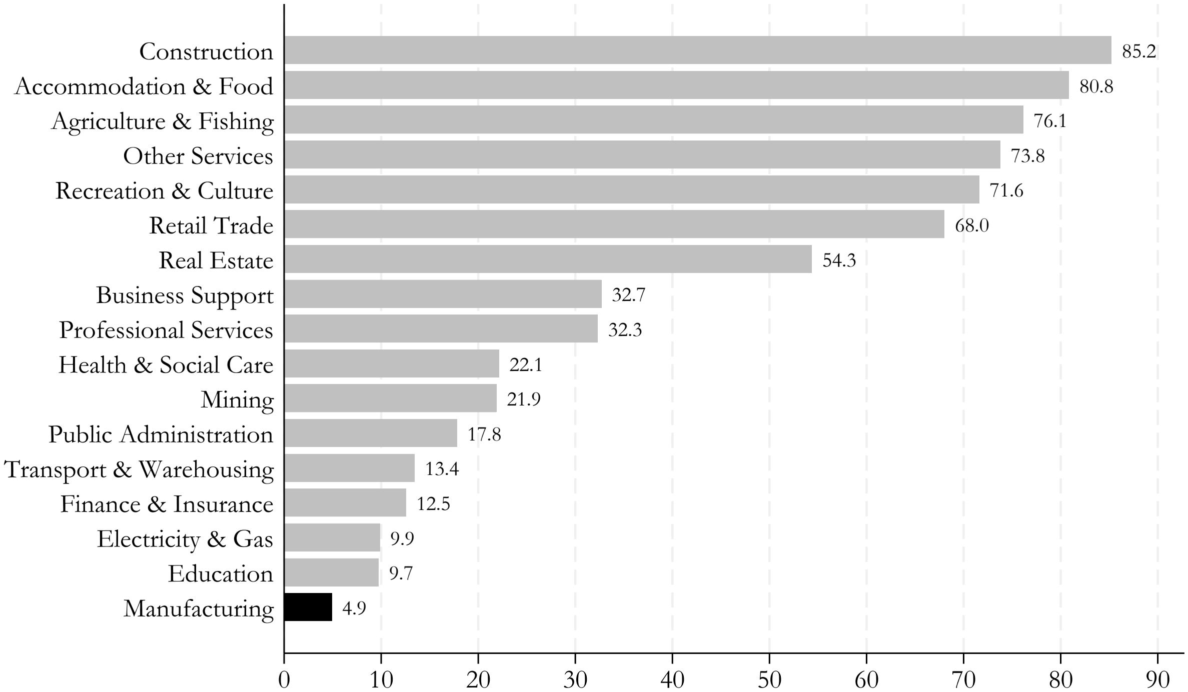
(a) Accidents over Time



(b) Accidents over Time per 10,000 Workers

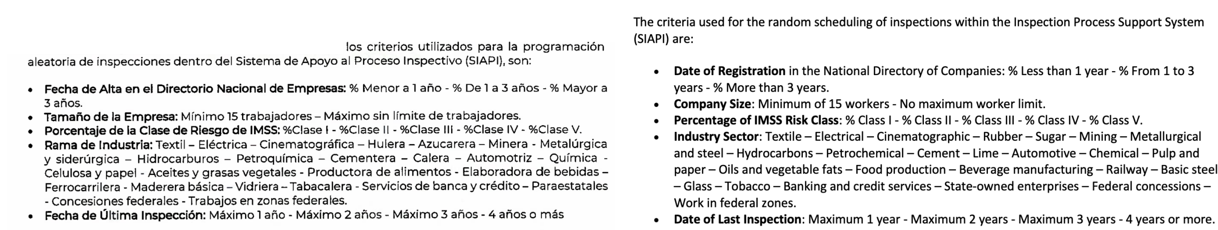
Notes: This Figure shows work-related accidents on a monthly basis, covering the period from January 2016 until April 2024 and accessed via the Sistema de Avisos de Accidentes de Trabajo (SIAAT) and the Mexican Secretariat of Labor and Social Welfare (STPS). Panel (a) displays all accidents and all accidents in the manufacturing sector. Panel (b) shows accidents per 10,000 workers across all sectors and within the manufacturing sector. The data on the number of workers per month and sector comes from administrative employer-employee data (IMSS).

Figure A.3: Share of Informality by Sector



Notes: This Figure shows the share of informal workers by industrial sector in Mexico during the second quarter of 2022. These shares are comparable across other quarters and years. Data source: `www.economia.gob.mx/datamexico/en/profile/geo/mexico?occupationMetrics=workforceOption` (INEGI). Last accessed: September 23, 2025.

Figure A.4: STPS Response to Right to Information on Strata Definition in SIAPI



los criterios utilizados para la programación aleatoria de inspecciones dentro del Sistema de Apoyo al Proceso Inspectivo (SIAPI), son:

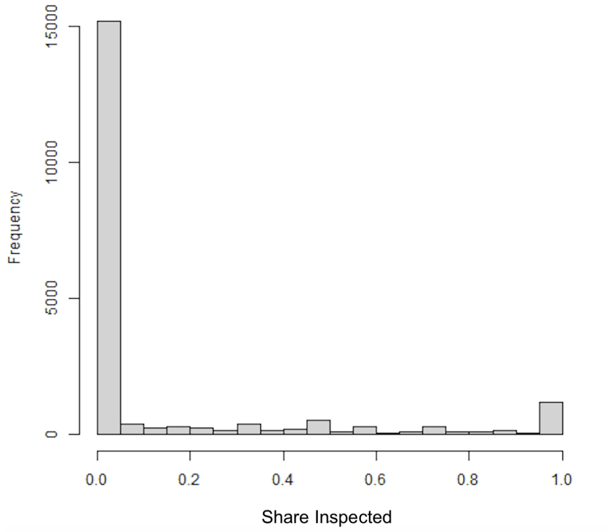
- **Fecha de Alta en el Directorio Nacional de Empresas:** % Menor a 1 año - % De 1 a 3 años - % Mayor a 3 años.
- **Tamaño de la Empresa:** Mínimo 15 trabajadores – Máximo sin límite de trabajadores.
- **Porcentaje de la Clase de Riesgo de IMSS:** %Clase I - %Clase II - %Clase III - %Clase IV - %Clase V.
- **Rama de Industria:** Textil – Eléctrica – Cinematográfica – Hulera – Azucarera – Minera - Metalúrgica y siderúrgica – Hidrocarburos – Petroquímica – Cementera – Calera – Automotriz – Química - Celulosa y papel - Aceites y grasas vegetales - Productora de alimentos - Elaboradora de bebidas – Ferrocarrilera - Maderera básica – Vidriera – Tabacalera - Servicios de banca y crédito – Paraestatales - Concesiones federales - Trabajos en zonas federales.
- **Fecha de Última Inspección:** Máximo 1 año - Máximo 2 años - Máximo 3 años - 4 años o más

The criteria used for the random scheduling of inspections within the Inspection Process Support System (SIAPI) are:

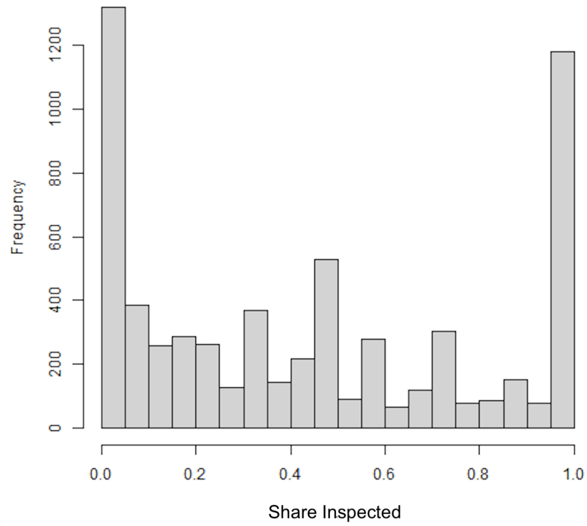
- **Date of Registration** in the National Directory of Companies: % Less than 1 year - % From 1 to 3 years - % More than 3 years.
- **Company Size**: Minimum of 15 workers - No maximum worker limit.
- **Percentage of IMSS Risk Class**: % Class I - % Class II - % Class III - % Class IV - % Class V.
- **Industry Sector**: Textile – Electrical – Cinematographic – Rubber – Sugar – Mining – Metallurgical and steel – Hydrocarbons – Petrochemical – Cement – Lime – Automotive – Chemical – Pulp and paper – Oils and vegetable fats – Food production – Beverage manufacturing – Railway – Basic steel – Glass – Tobacco – Banking and credit services – State-owned enterprises – Federal concessions – Work in federal zones.
- **Date of Last Inspection**: Maximum 1 year - Maximum 2 years - Maximum 3 years - 4 years or more.

Notes: This Figure shows the official answer to our information request regarding randomization groups. The left-hand-side shows the original Spanish answer. The right-hand-side shows the English translation.

Figure A.5: Distribution in Probability of Inspection across Randomization Groups



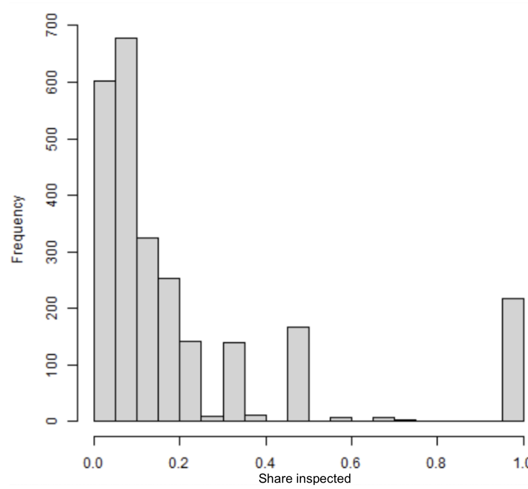
(a) All groups



(b) Groups with positive inspection probability

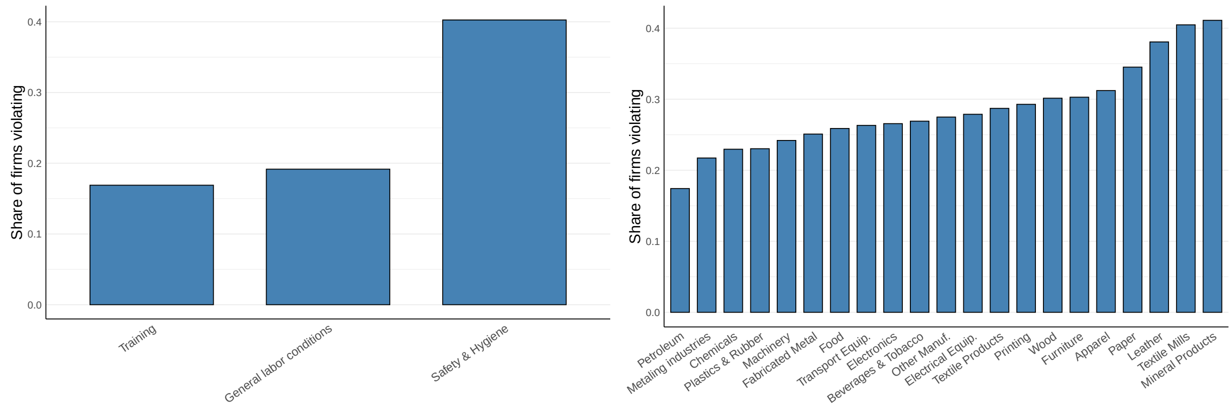
Notes: This figure shows the distribution of the estimated probability of inspection across randomization groups in the Economic Census. Each randomization group is a sector x age group x size group x years since last inspection group x state stratum. The probability of inspection for each group is calculated as the likelihood of being inspected at least once $= 1 - \frac{N_g-1}{N_g}^{n\ insp_g}$, where N_g is the number of firms in the randomization group and $n\ insp_g$ is the number of inspections carried out to firms in the randomization group. Back to Section 4.

Figure A.6: Share Firms Receiving First Inspection across Randomization Groups



Notes: This Figure shows the distribution of the share of firms receiving the first ordinary inspection across different randomization groups x date. The Figure was built using the sample of firms receiving a first inspection in the EMIM data.

Figure A.7: Patterns in Firms Found Violating Workplace Regulations



(a) Share of Firms Violating by Inspection Topic (b) Share of Firms Violating by Firm Sector

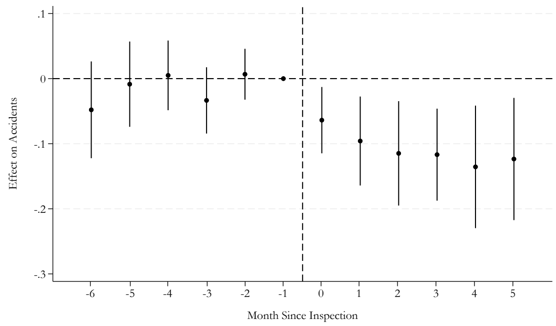
Notes: This Figure shows the share of firms that are found violating workplace regulations by inspection topic (Panel a) and by 21 manufacturing subsectors (Panel b). Data from the 2019 Economic Census.

Figure A.8: Coefficients of Violation Regression - Robustness

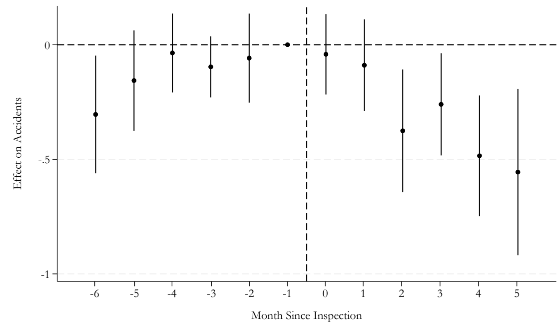
(a) Variables from Model Prediction (b) Variables from Lasso Regularization

Notes: This figure shows the coefficients and 90% and 95% confidence intervals from estimating regressions where the outcome is a binary variable indicating violation (blue) or violation and sancion (red) and the explanatory variables is a subset of firm characteristics from the economic Census. Coefficients estimated on sample of manufacturing firms with over 20 workers from Economic Census which received an ordinary inspection between 2019 and 2021. Panel (a) shows results for multivariate regressions where variables are selected based on Prediction 1 from Section 2. Panel (b) shows results from uni-variate regressions, and multivariate regressions, where variables are selected using Lasso regularization. All explanatory variables are standardized. All regressions control for estimated probability of inspection, state fixed effects and sector fixed effects. [Back to Section 5]

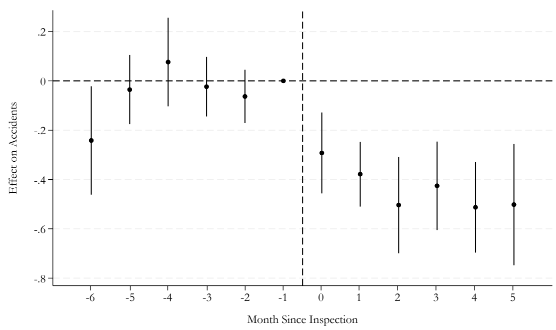
Figure A.9: Robustness Results Effect of Inspections on Accidents



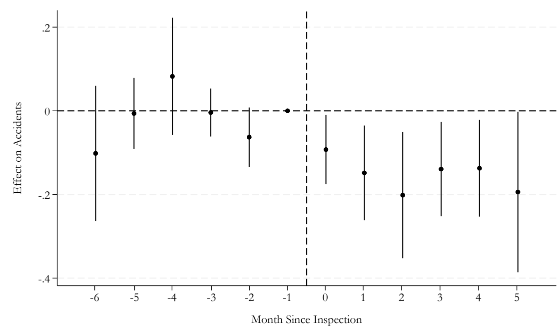
(a) Effect of Ordinary Inspections on Work Accidents - Extended Window



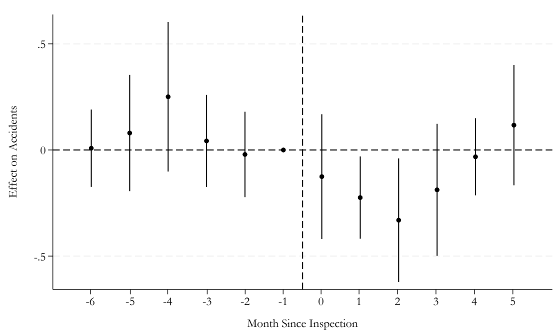
(b) Effect of First Ordinary Inspections on Work Accidents



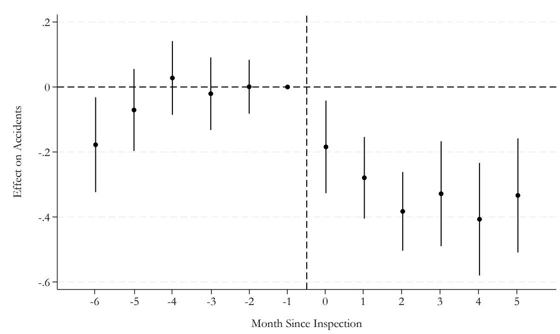
(c) Effect of Ordinary Inspections on Total Accidents



(d) Effect of Ordinary Inspections on Total Accidents in the Manufacturing Sector



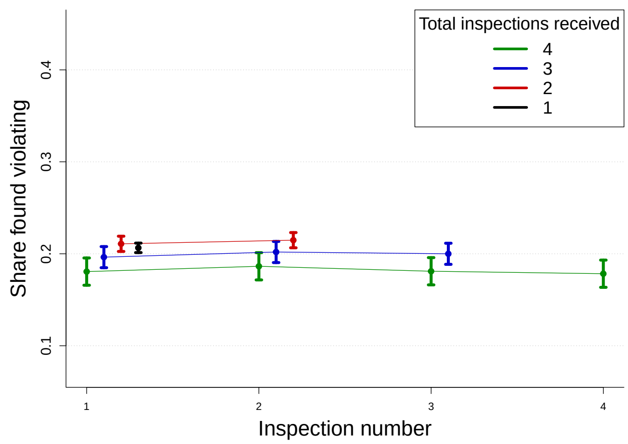
(e) Effect of Ordinary Inspections in Manuf. Sector on Total Accidents in the Manufacturing Sector



(f) Effect of Ordinary Inspections in (non-) Manufacturing Sector on Total Accidents in (non-) Manufacturing Sector

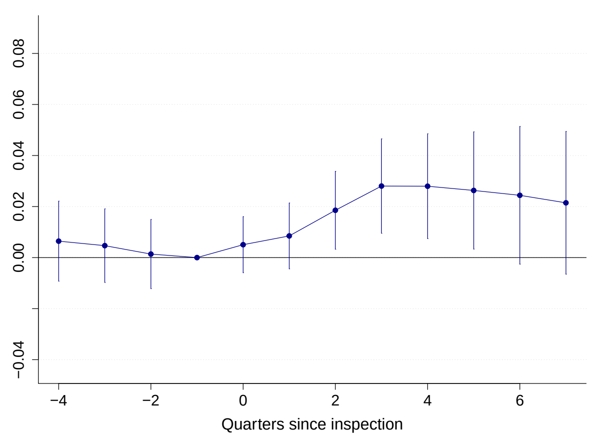
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 8 at the monthly level for $h \in [-6 : 5]$. Regressions are estimated based on a balanced sample of municipalities from the accident data. Treated municipalities are those receiving a stratified random inspection between January 2018 and June 2019. Control municipalities are those with no new inspections until period h. Standard errors are clustered at the municipality level. The outcome variable is number of work accidents on the municipality level. In panel (a) we extend the treatment window to July 2017 until June 2022. In panel (b) we consider only *first* stratified random inspection between January 2018 and June 2019. Panels (c) and (d) show the effects of ordinary inspections on total accidents and the effect of ordinary inspection on total accidents in the manufacturing sector, respectively. In panel (e) we show the effect of ordinary inspections in manuf. sector on total accidents in the manufacturing sector. In panel (f) we expand our dataset by splitting total accidents and inspections into manufacturing and non-manufacturing sectors and run the same specification 8 but cluster on the municipality x (non-) manufacturing level.

Figure A.10: Share of Firms Violating, by Inspection Number - Extraordinary Inspections

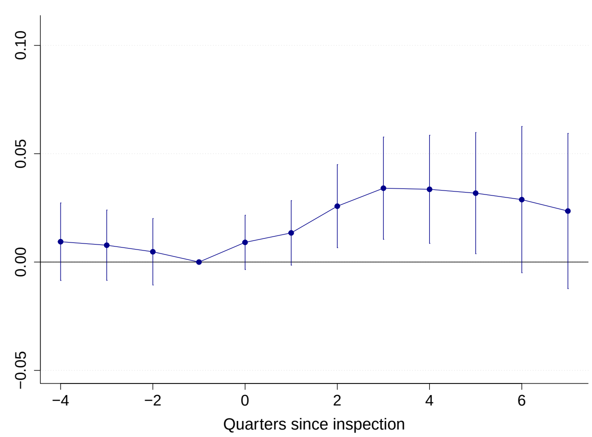


Notes: This figure is constructed using firm level data on inspected firms, from the subsample of inspections merged to DENUE. It shows the coefficients and 95% confidence intervals from regressing a binary variable indicating violation during extraordinary inspection t on a variable indicating how many extraordinary inspections the firm has received until period t. The sample used to estimate the coefficients is restricted to firms which received N extraordinary inspections in total. Each color corresponds to a different $N \in [1:4]$. Standard errors are clustered at the firm level.

Figure A.11: Effect on Log Workers - Quarterly Outcomes



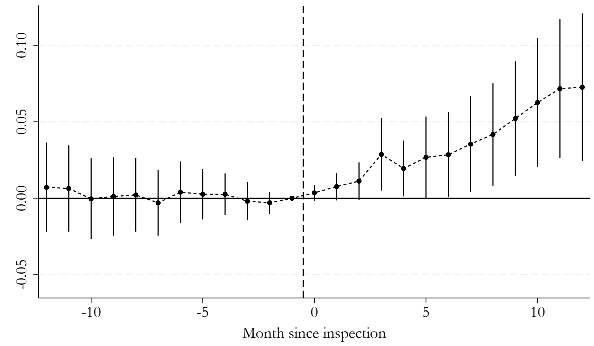
(a) Log Total Workers



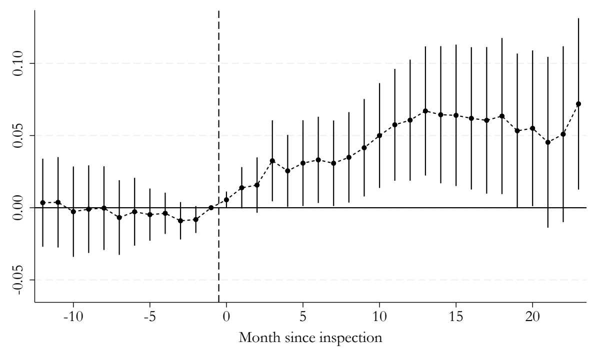
(b) Log Total Production Workers

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the quarterly level for $h \in [-4:8]$. In panel (a), the outcome variable is total workers. In panel (b), the outcome is the total number of blue-collar workers. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

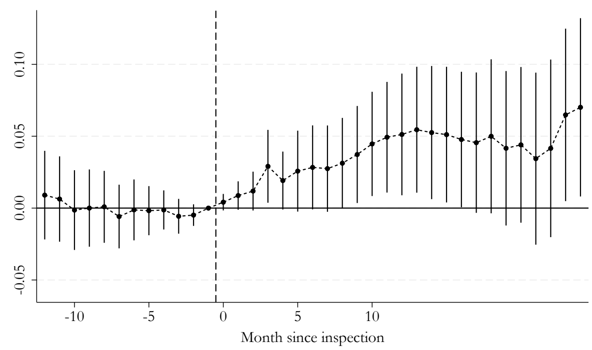
Figure A.12: Effect on Log Workers - Robustness



(a) Log Total Workers of Firm x State



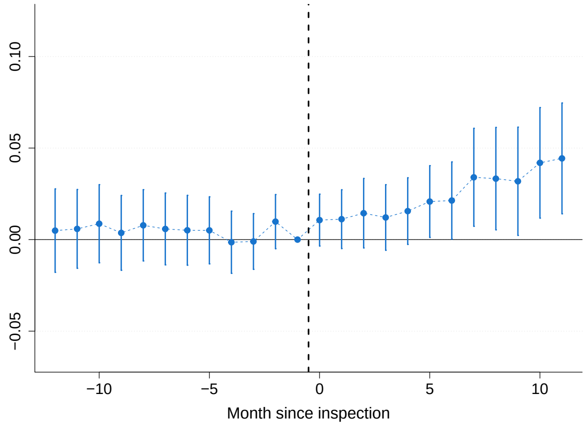
(b) Log Total Workers of Establishment (Long-Run)



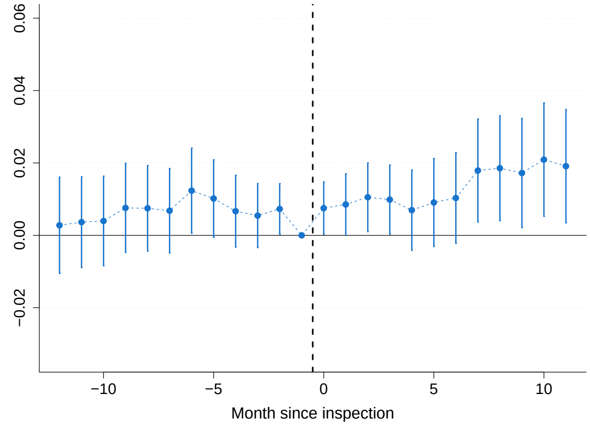
(c) Log Total Workers of Firm x State (Long-Run)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 12]$ in panel (a) and for $h \in [-12 : 23]$ in panel (b) and (c). In panels (a) and (c), the outcome variable is log total workers on the establishment level. In panel (b), the outcome is the log of total workers at the firm-by-state level. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

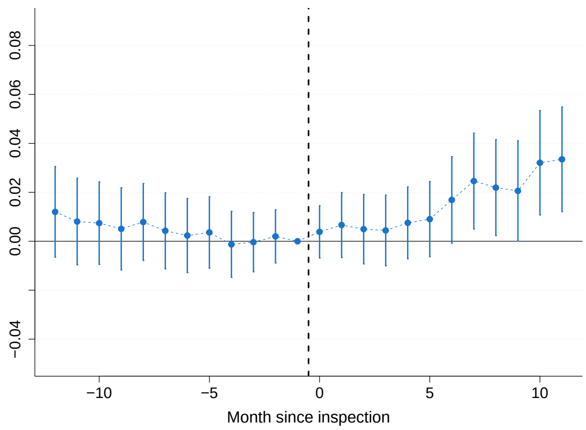
Figure A.13: Effect on Log Total Production Workers - Robustness



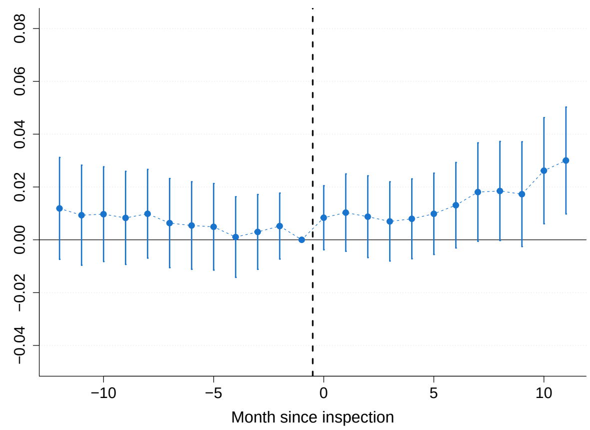
(a) Only Firms with One Establ. in each State



(b) Years 2017 - 2019



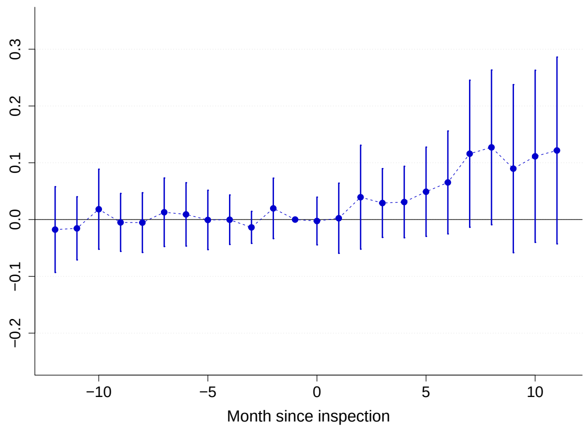
(c) Random. Grp. with broader Sector x Date FEs



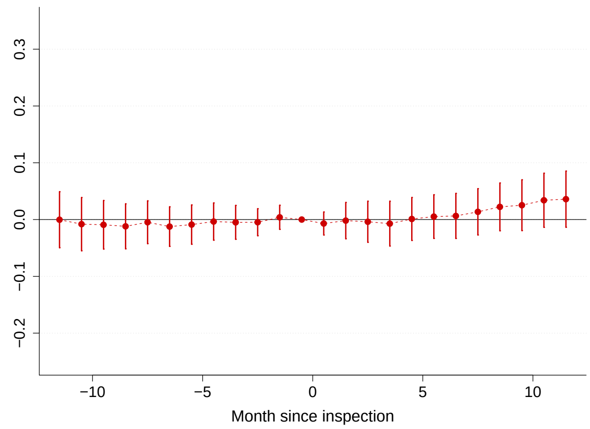
(d) Propensity Score Quantile x Date Fixed Effects

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. The outcome is total blue-collar workers. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019 in panels (a), (c), and (d). Control firms are those untreated for the whole period. Standard errors are clustered at the firm level. Panel (a) is based on a sample with single-establishment firms per state. Panel (b) captures treated firms receiving the first ordinary inspection between 2017 and 2019. Panel (c) is based on a broader definition of sector x date fixed effects. Panel (d) uses propensity score quantile x date fixed effects. [Back to Section 6.3.1]

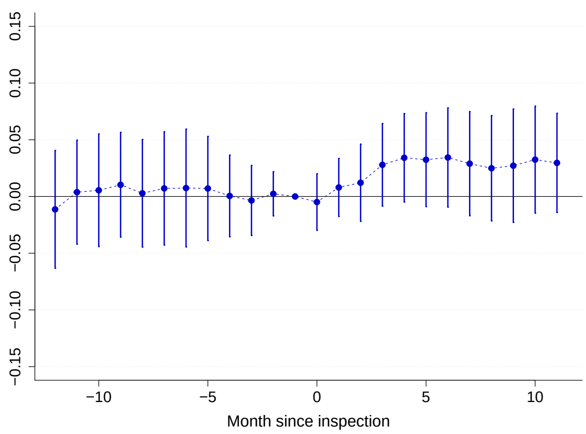
Figure A.14: Effect on Log Total Production Workers - Heterogeneity by HHI and Market Share



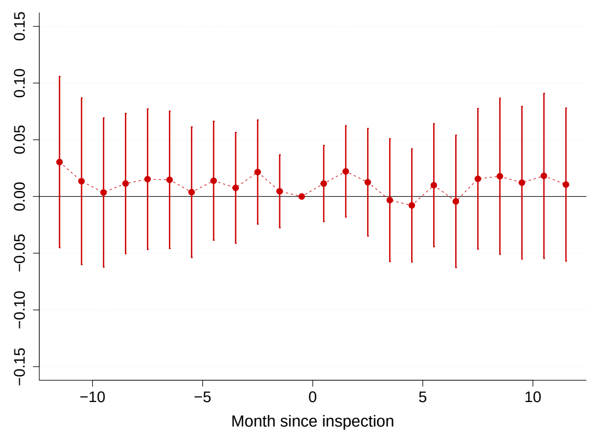
(a) High LLM Concentration



(b) Low LLM Concentration



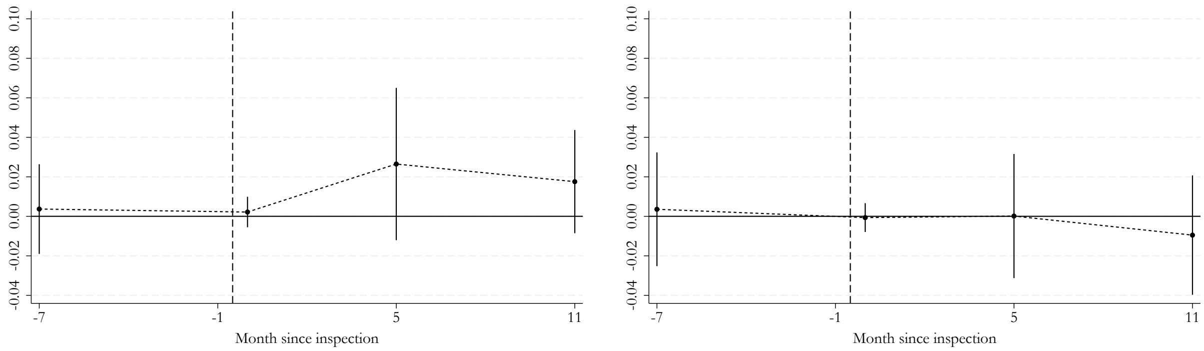
(c) High Market Share in LLM



(d) Low Market Share in LLM

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. The outcome is total blue-collar workers. Regressions are estimated based on sub-samples of a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those not untreated for the whole period. Panel (a) restricts the sample to firms in local labor markets with an HHI index above the 75th percentile of the HHI distribution in our sample. Panel (b) restricts the sample to firms in local labor markets with an HHI index below the 25th percentile. Panel (c) restricts the sample to firms with a labor market share above the 75th percentile. Panel (d) restricts the sample to firms with a labor market share below the 25th percentile. HHI and market share are measured using data from the Economic Census. Local labor markets are defined as municipality x 3-digits NAICS sector. [Back to Section 6.3.1]

Figure A.15: Effects on Hiring and Separation Rates

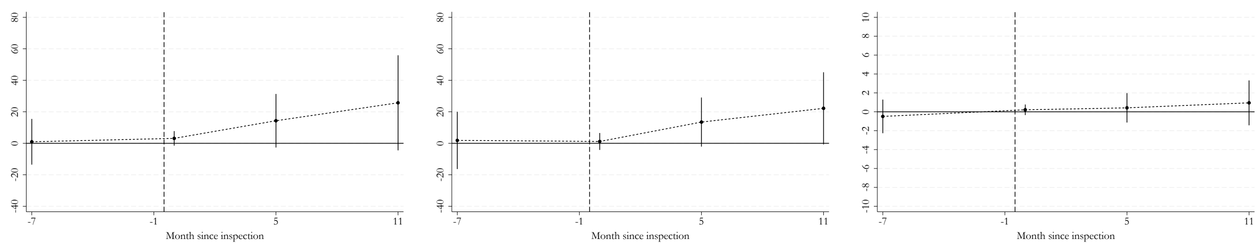


(a) Hiring Rate past 6 Months (Monthly Admin Data)

(b) Separation Rate past 6 Months (Monthly Admin Data))

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-7, 5, 11]$. In panel (a) the outcome variables is the hiring rate defined as the sum of hires in past 6 months over the current firm size. In panel (b) the outcome variable is the separation rate defined as the sum of separations in past 6 months over the current firm size. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those untreated for the whole period. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

Figure A.16: Effects on Type of Hire



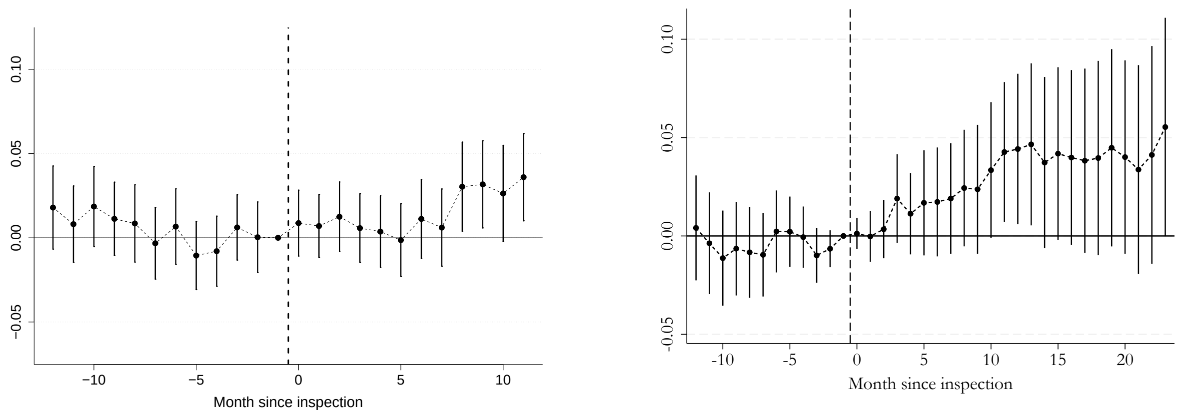
(a) Hires from other Firm

(b) Hires, from outside IMSS (previously in IMSS)

(c) Hires from outside IMSS (first time in IMSS)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-7, 5, 11]$. In panel (a) the outcome variables is the sum of hires from other formal firms in past 6 months. In panel (b) the outcome variables is the sum of hires from outside IMSS in past 6 months. Oustide IMSS refers to not being formally employed in the past month. In panel (c) the outcome variables is the sum of first time hires into IMSS in past 6 months. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those untreated for the whole period. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

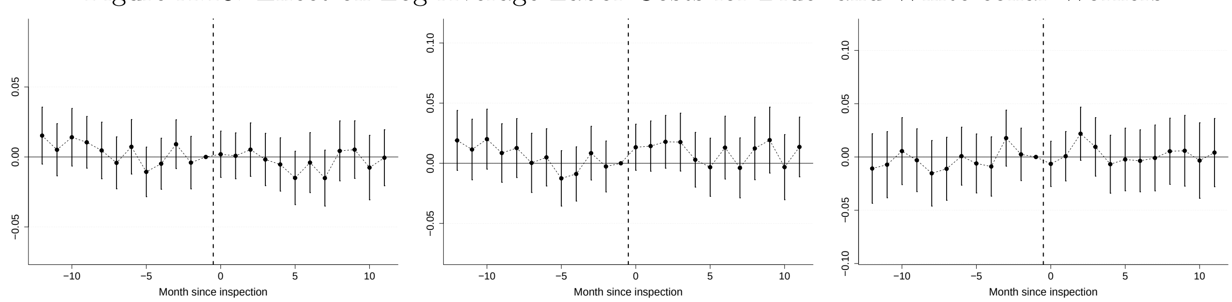
Figure A.17: Effect of Inspection on Total Labor Costs



(a) Log Total Labor Costs (EMIM) (b) Log Total Salaries (Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$ in panel (a) and $h \in [-12 : 24]$ in panel (b). The outcome variable is the log of total labor costs in panel (a), and log total salaries in panel (b). Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020 or IMSS from 2017 to 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2019 in EMIM or between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

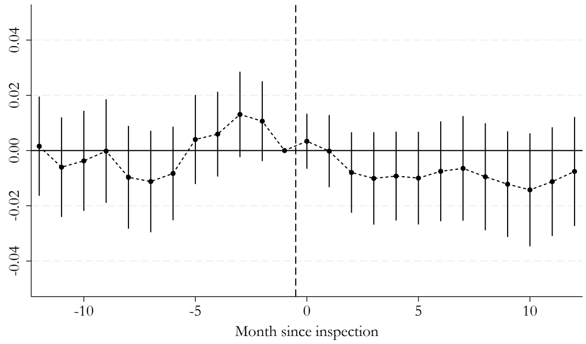
Figure A.18: Effect on Log Average Labor Costs for Blue- and White-collar Workers



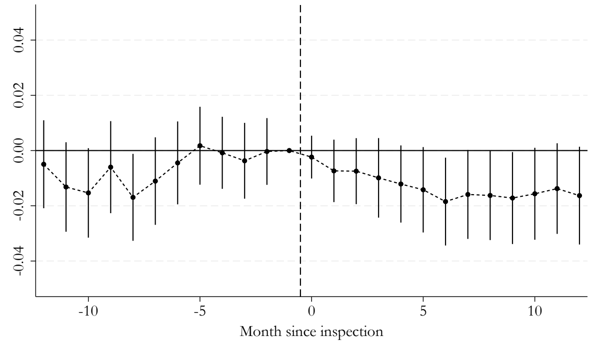
(a) Log Labor Costs per Worker (b) Log average Wage Blue-collar (c) Log average Wage White-collar

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variable is the log of average labor costs. Labor costs encompass wages, social security contributions, and expenses associated with outsourced workers. In panel (b), the outcome variable is log average labor costs for blue-collar workers at the firm level. In panel (c), the outcome is log average labor costs for white-collar workers at the firm level. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

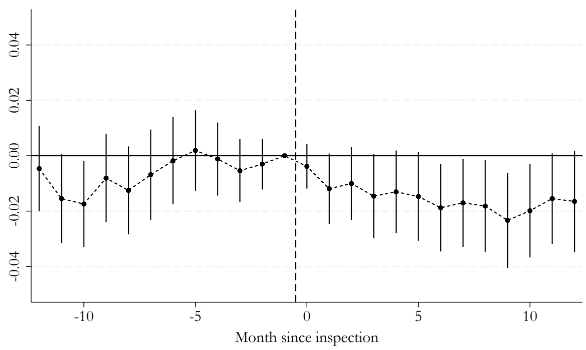
Figure A.19: Effect on Firm Wage Percentiles



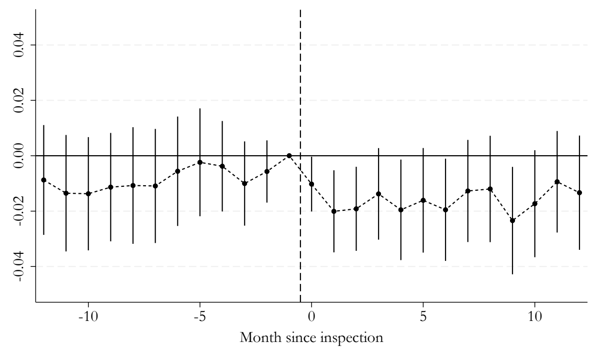
(a) Log Wage P10



(b) Log Wage P25



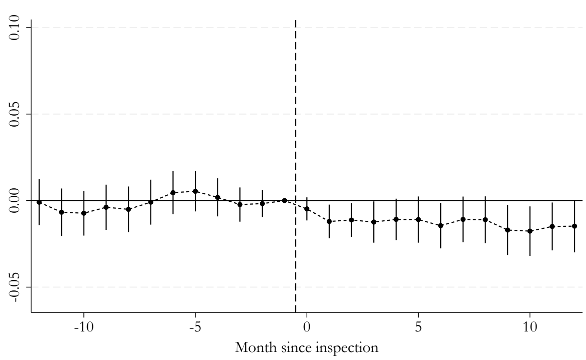
(c) Log Wage P50



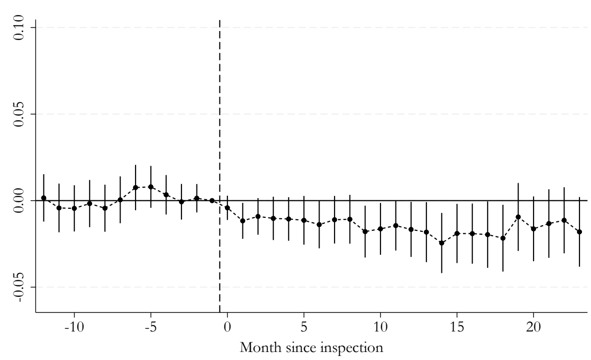
(d) Log Wage P90

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), (b), (c), (d) the outcome variable is the firm's 10th, 25th, 50th, 90th wage percentile, respectively. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those untreated for the whole period. Standard errors are clustered at the firm level.

Figure A.20: Effect on Average Firm Wage - Robustness



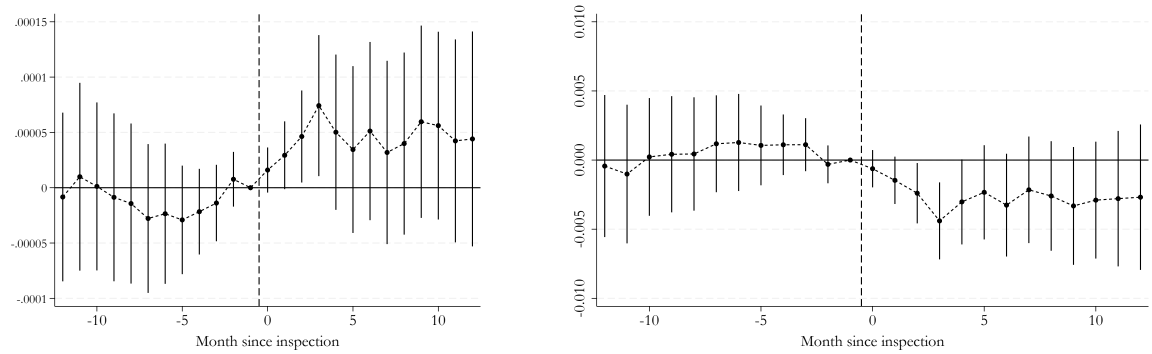
(a) Log Average Firm x State Wage
(Monthly Admin Data)



(b) Log Average Establishment Wage
(Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$ in panel (a) on the firm x state level and for $h \in [-12 : 23]$ on the establishment level in panel (b). Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those untreated for the whole period. Standard errors are clustered at the firm x state level in panel (a) and on the establishment level in panel (b).

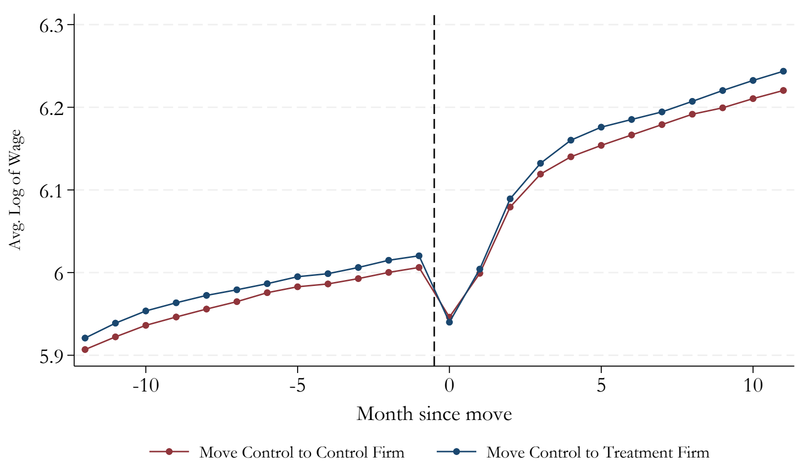
Figure A.21: Effect on Log Average Worker Age and Log Year of IMSS Registration



(a) Average Year of Worker Registration in IMSS (b) Average Worker Age

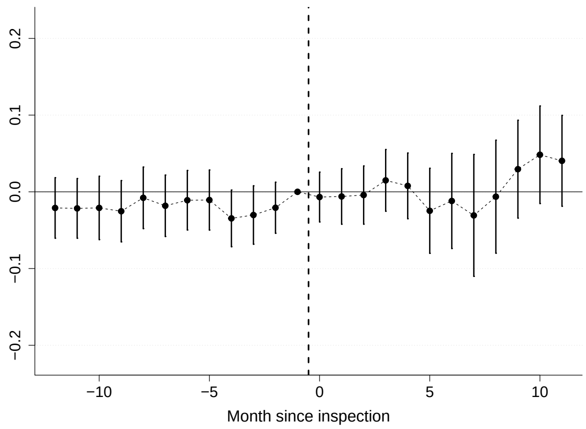
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome variable is the log average year of worker registration in IMSS at the firm level. In panel (b), the outcome is log average worker age at the firm level. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2021. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

Figure A.22: Log Wage of New Workers

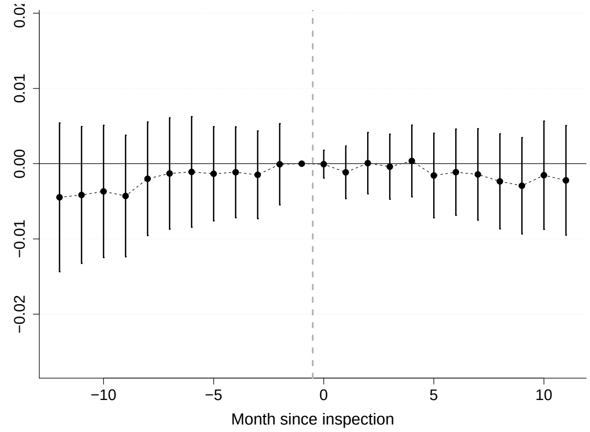


Notes: This Figure the log wage of new workers (hires) who were employed for at least 12 months at firms that had not yet been, or never would be, inspected, and who subsequently switch to either an already (blue line) or another not yet or never inspected (red line) firm, remaining in the new firm for at least 12 months. Job Moves are based on a balanced sample of workers from IMSS between 2017 and 2021 using the month of the job change as the event time. Treated firms are those receiving the first ordinary inspection between 2018 and 2020 in IMSS. Control firms are those untreated for the whole period.

Figure A.23: Effect on Log Revenue per Worker and Outsourcing



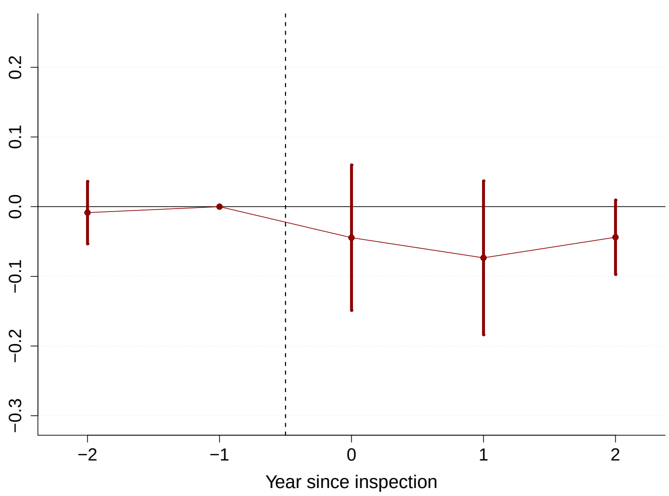
(a) Log revenue per worker



(b) Share of workers outsourced

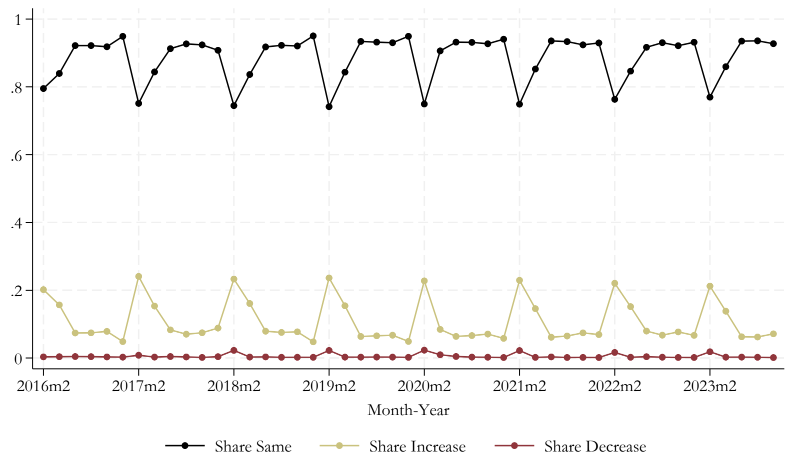
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. The outcome variable is log monthly revenue per worker in panel a and share of workers outsourced in panel b. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

Figure A.24: Effect on Profits over Value Added



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the yearly level for $h \in [-2 : 2]$. The outcome variable is yearly profits divided by yearly value-added. Regressions are estimated based on a balanced sample of firms from EAIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those untreated until period h. Standard errors are clustered at the firm level.

Figure A.25: Share Wage Changes



Notes: This figure presents the shares of same, increases, and decreases in wages among workers across even-numbered months from February 2016 to October 2023. We focus on employment spells lasting at least four months, excluding the first and last month of each spell, as wages in those months may be affected by non-standard start or end dates. Only even-numbered months are considered, as they reflect base wages, whereas odd-numbered months also include variable components such as overtime. or commissions. The analysis is limited to employees working between 2018 and 2019 in firms included in the IMSS manufacturing sample.

A.2 Tables Appendix

Table A.1: Summary Statistics: EMIM Survey and IMSS Admin Data

Panel A: EMIM		**Panel B: IMSS**	
Variable	Mean (SD)	Variable	Mean (SD)
Total Workers	373	Total Workers	275.79
	(614)		(698.18)
Avg. Monthly Labor Costs	17.03	Mean Salary	298.24
	(12.38)		(254.16)
Share Blue-Collar Workers	0.77	Total Female Workers	97.49
	(0.17)		(301.83)
Share of Workers Outsourced	0.21	Total Permanent Workers	236.74
	(0.39)		(637.62)
Yearly Profits	156,616	Total Hires since Last Month	14.36
	(1,016,691)		(58.17)
Firm Age	24.24	Total Separations since Last Month	12.97
	(15.71)		(45.62)
Multinational	0.29	Northern Zone Region	0.11
	(0.45)		(0.31)
Firms	8,793	Establishments	8,930
Observations	105,516	Observations	107,160

Notes: This table reports the mean and standard deviation (in parentheses) for the main EMIM (Panel A) and IMSS (Panel B) firm characteristics in 2017. Variables in Panel A are in thousands of Mexican Pesos. Mean Salary in Panel B is represented by the worker's daily taxable income (*salario base de cotización*) in Mexican Pesos, which includes various forms of compensation such as overtime, bonuses, commissions, the 13th salary (*aguinaldo*), and the mandatory vacation bonus (*prima vacacional*). However, it excludes profit-sharing benefits (PTU).

Table A.2: Correlation Between Survey Firm Characteristics (EMIM) and Extraordinary Inspection

	Total workers	Inhouse workers	VA per worker	Revenue	Revenue per worker	Share blue collar	Share outsourced	Avg wage	Avg hs worked
Panel A: Without randomization group fixed effects									
	126.9***	150.7***	-8.662	13,478.4***	-63.61*	0.0053*	-0.0916***	-0.5560	-0.0116***
	(16.92)	(15.59)	(7.355)	(3511)	(32.84)	(0.0031)	(0.0058)	(0.5838)	(0.001)
N	201,267	200,336	200,336	200,070	200,006	164,448	200,336	200,336	198,813
Panel B: With randomization group fixed effects									
	70.40***	55.89***	-3.898	8,758.2**	-42.04	0.0020	0.0028	0.2317	-0.0017**
	(16.52)	(15)	(4.848)	(3352.2)	(32.35)	(0.0027)	(0.0027)	(0.2693)	(6e-04)
N	181,900	180,935	180,935	180,769	180,705	148,391	180,935	180,935	179,487

Notes: This Table shows the results from estimating a regression using the sample of firms from EMIM 2018 and 2019, where we regress a firm-level outcome measured in month $t-1$ indicated in the different columns on a binary variable indicating an extraordinary inspection in month t. The regressions in panel B include state-by-date fixed effects. The regressions in panel B include randomization-group-by-date fixed effects. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

Table A.3: Correlation Between Survey Firm Characteristics (EMIM) and First Ordinary Inspection

	Total workers	Inhouse workers	VA per worker	Revenue	Revenue per worker	Share blue collar	Share outsourced	Avg wage	Avg hs worked
Panel A: No randomization group fixed effects									
	55.42***	88.31***	23.46	6,419.5	68.19	-0.0054	-0.0553***	0.6663	-0.0096***
	(18.77)	(17.55)	(25.86)	(4086.9)	(102.5)	(0.0051)	(0.0106)	(1.423)	(0.0013)
N	201,267	200,336	200,336	200,070	200,006	164,448	200,336	200,336	198,813
Panel B: With randomization group fixed effects									
	25.19	34.27*	33.47	4,190.0	100.7	-0.0062	0.0029	-0.4061	-0.0018
	(20.58)	(18.37)	(25.91)	(4114.4)	(121.1)	(0.0054)	(0.0049)	(0.7836)	(0.0013)
N	181,900	180,935	180,935	180,769	180,705	148,391	180,935	180,935	179,487

Notes: This Table shows the results from estimating a regression using the sample of firms from EMIM in 2018 and 2019, where we regress a firm-level outcome measured in month $t-1$ indicated in the different columns on a binary variable indicating a firm's *first* ordinary inspection in month t. The regressions in panel A include state-by-date fixed effects. The regressions in panel B include randomization-group-by-date fixed effects. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

Table A.4: Correlation Between Administrative Firm Characteristics (IMSS) and First Ordinary Inspection

	Total workers	Share women	Share temp. workers	Separations	Hires	Worker age	Mean salary	Median salary	Market share	Share min. wage
Panel A: Without randomization group fixed effects										
	251.4***	0.000763	0.0218**	13.12***	14.47***	-1.075***	73.84***	53.65***	0.0344***	-0.0717***
	(35.82)	(0.00821)	(0.00948)	(2.998)	(3.379)	(0.162)	(10.62)	(10.46)	(0.00744)	(0.00428)
N	189,920	189,920	189,920	189,920	189,920	189,920	189,920	189,920	189,920	189,920
Panel B: With randomization group fixed effects										
	81.18*	0.00412	-0.0114	0.865	5.963	-0.0434	15.09**	14.58*	0.00243	-0.00754*
	(47.49)	(0.00805)	(0.0111)	(4.036)	(4.396)	(0.168)	(8.813)	(8.608)	(0.00825)	(0.00405)
N	164,837	164,837	164,837	164,837	164,837	164,837	164,837	164,837	164,837	164,837

Notes: This Table shows the results from estimating a regression using the sample of establishments from IMSS in 2018 and 2019 where we regress a establishment-level outcome measured in month t-1 indicated in the different columns on a binary variable indicating a first ordinary inspection in month t. The regressions from panel A include state x year x month fixed effects. The regressions from panel B include randomization group x year x month fixed effects. Standard errors are clustered at the establishment level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.5: Correlation between firm characteristics and violation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Dependent variable				
	Violation					Violation & sanction		
	No controls	Control for propensity score			Prop score weighting	No controls	Control for prop score	Prop score weighting
Regressions with firm specific characteristics								
Total workers	0.0077	0.0072	0.0057	0.0312	0.0069	-0.0016	0.0222	-0.0565
	(0.0285)	(0.0285)	(0.0286)	(0.0306)	(0.0469)	(0.0272)	(0.0293)	(0.0423)
Revenue	-0.0051	-0.0051	-0.0054	-0.0035	-0.0051**	-0.0019	-0.0024	-0.0052*
	(0.0038)	(0.0038)	(0.0038)	(0.004)	(0.0026)	(0.0039)	(0.004)	(0.0027)
Profit	-0.0089***	-0.0089***	-0.0091***	-0.0084**	-0.0156*	-0.0058*	-0.0071**	-0.0133*
	(0.0034)	(0.0034)	(0.0034)	(0.0041)	(0.0082)	(0.003)	(0.0036)	(0.007)
Profit per worker	-0.2091	-0.2081	-0.214	-0.312	-2.143***	-0.1464	-0.2467*	-2.22***
	(0.1538)	(0.1539)	(0.1556)	(0.1928)	(0.6494)	(0.1223)	(0.1461)	(0.6259)
Value added per worker	-0.2095	-0.21	-0.2183	-0.3548*	-1.982***	-0.164	-0.2828*	-1.962***
	(0.1646)	(0.1648)	(0.1664)	(0.1994)	(0.5766)	(0.1306)	(0.151)	(0.559)
Total investment	0.0108	0.011	0.0109	0.0162	-0.0139	0.0233	0.0269	-0.0093
	(0.0216)	(0.0217)	(0.0217)	(0.0221)	(0.0119)	(0.0211)	(0.0209)	(0.009)
Total value of machinery	0.0112	0.0113	0.0109	0.0178	0.0056	0.0191	0.02	-0.0417*
	(0.0207)	(0.0207)	(0.0208)	(0.0211)	(0.0413)	(0.0202)	(0.0202)	(0.0251)
Multinational firm	-7e-04	-9e-04	-0.0014	0.006	0.0085	0.0015	0.0085	0.0108
	(0.0069)	(0.0069)	(0.0069)	(0.008)	(0.0175)	(0.0063)	(0.0075)	(0.0183)
Share of revenue exported	-4e-04	-4e-04	-4e-04	0.0049	0.0068	-0.0016	0.0012	0.001
	(0.006)	(0.006)	(0.006)	(0.0068)	(0.0132)	(0.0054)	(0.0063)	(0.0124)
Firm age	0.0051	0.0046	0.0045	-0.001	-0.0191*	-6e-04	-1e-04	-0.0193**
	(0.0096)	(0.0097)	(0.0097)	(0.0102)	(0.0109)	(0.009)	(0.0099)	(0.0093)
Regressions with worker characteristics								
Avg wage	-0.0059	-0.0058	-0.0055	-0.0116	-0.0515***	-0.0127	-0.0181**	-0.0389**
	(0.01)	(0.01)	(0.01)	(0.0111)	(0.0185)	(0.0081)	(0.0089)	(0.0176)
Avg hs worked	-0.0048	-0.0049	-0.0053	-0.0069	0.053	-0.0039	-0.0041	0.0741
	(0.0225)	(0.0225)	(0.0225)	(0.0234)	(0.0527)	(0.0207)	(0.0215)	(0.0481)
Share of workers receiving training	-0.027***	-0.0279***	-0.0281***	-0.0302***	-0.0176	-0.0291***	-0.0281***	-0.0099
	(0.0094)	(0.0094)	(0.0094)	(0.0095)	(0.0199)	(0.0084)	(0.0086)	(0.0191)
Training costs per worker	-0.006	-0.0062	-0.0067	-0.0046	-0.0063	-0.0069*	-0.0063	-0.0076
	(0.0053)	(0.0053)	(0.0054)	(0.0051)	(0.0047)	(0.0036)	(0.0042)	(0.0055)
Average worker age	0.0109	0.0106	0.0107	0.0029	0.0107	0.0057	6e-04	-0.0052
	(0.0123)	(0.0123)	(0.0123)	(0.0129)	(0.0281)	(0.0111)	(0.0118)	(0.0254)
Share of workers outsourced	0.0425***	0.0448***	0.0449***	0.0536***	0.0193	0.0476***	0.0487***	0.0212
	(0.0111)	(0.0113)	(0.0112)	(0.011)	(0.0202)	(0.0109)	(0.0108)	(0.0206)
Share of workers with college degree	0.0056	0.0059	0.0058	0.0073	-0.0066	0.013	0.0128	0.0166
	(0.0123)	(0.0123)	(0.0123)	(0.0128)	(0.0253)	(0.0111)	(0.0118)	(0.0218)
Share workers women	-0.0314***	-0.0319***	-0.0321***	-0.0254**	-0.0134	-0.0273***	-0.0214*	-0.0089
	(0.0104)	(0.0104)	(0.0104)	(0.0121)	(0.0288)	(0.0095)	(0.0111)	(0.0257)
Regressions with local labor market characteristics								
Labor market concentration in LLM (HHI)	0.0181*	0.0193**	0.0193**	0.0139	0.0255	0.0239***	0.0173	0.0382
	(0.0094)	(0.0095)	(0.0095)	(0.011)	(0.0273)	(0.0092)	(0.0106)	(0.0257)
Labor market share in the LLM	0.0173**	0.0184**	0.0185**	0.0134	0.0099	0.0228***	0.0171*	0.0188
	(0.0086)	(0.0087)	(0.0086)	(0.0098)	(0.0178)	(0.0085)	(0.0095)	(0.0186)
Estimated inspection probability	-	-0.0238	-	-0.0189	-	-	-0.0212	-
	-	(0.0529)	-	(0.0567)	-	-	(0.052)	-
Control for $\hat{p}$	No	Yes	No	Yes	No	No	Yes	No
$\hat{p}$ quintile FE	No	No	Yes	No	No	No	No	No
Probability re-weighting	No	No	No	No	Yes	No	No	Yes
State and Sector FE	No	No	No	Yes	Yes	No	Yes	Yes

Notes: This table shows the coefficients of different regressions of a binary variable indicating whether an inspected firm was found violating on a firm characteristic. Each coefficient is estimated in a separate regression. In Columns 1 to 5, the outcome variable is whether the firm was found in violation. In Columns 6 to 8, the outcome is 1 if the firm was found violating and a sanction procedure was initiated (firms violating without a sanction procedure are excluded from this estimation). The regressions are estimated using a sample of manufacturing firms from the 2018 Economic Census with more than 20 workers that received an ordinary inspection between 2019 and 2021. Firm characteristics correspond to the values measured during the 2018 Economic Census. Columns 5 and 8 estimate the regression using the inverse of the estimated probability of inspection as a weight (Hirano et al., 2003). *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$. Back to Section 5

Table A.6: Summary Statistics of Survey Responses by Salary Category

	Monthly salary		
	Below 10k	Above 10k	All
When choosing current job, considered:			
Salary	0.61	0.81	0.74
Vacation	0.2	0.36	0.31
Personal growth opportunities	0.35	0.51	0.45
Pension contributions	0.086	0.13	0.11
Profit-sharing	0.1	0.21	0.17
On the job training	0.31	0.36	0.34
Working conditions	0.17	0.14	0.14
N	188	417	638

Notes: This table shows responses for self-conducted survey to Mexican workers via Prolific (N=638). Each row displays the share of respondents selecting each job attribute in response to the question: *What job characteristics did you take into consideration when making the decision to choose your current job?*.

B Appendix: Matching Process

Matching Inspections and establishment data. Our original inspections data includes information on the firm and the state in which the inspection took place, but does not identify the specific establishment inspected. As a result, for multi-establishment firms operating within the same state, we cannot determine which establishment was visited. In contrast, the establishment survey data is at the establishment level. To merge these datasets, we aggregate the establishment survey to the firm-by-state level. All regressions based on EMIM are conducted at this level of analysis. Under this approach, a firm classified as inspected should be interpreted as having had at least one of its establishments in a given state inspected. Over 90% of firms in our sample have only one establishment per state. Therefore, this firm-by-state aggregation does not strongly affect the interpretation or the results. One potential concern of this approach is that tagging all establishments of a firm within a state as inspected could cause the estimated effect to capture both direct and spillover effects across these establishments. However, we find no evidence on spillover effects when analyzing the impact of an inspection on another establishment located in a different state but belonging to the same firm. We demonstrate that our main results are robust when focusing exclusively on single-establishment firms in Appendix Figure A.13.

Table B.1: Comparison between Original and Matched Datasets

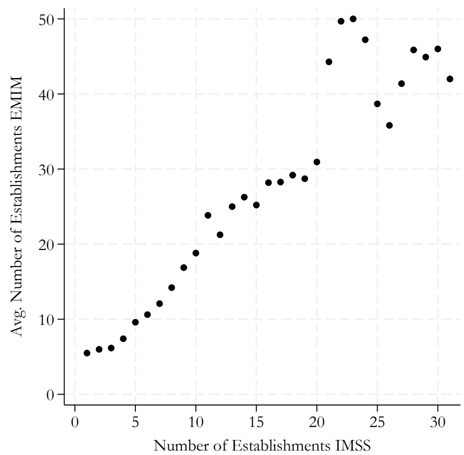
	(1)	(2)	(3)	(4)	(5)
	Inspections	Inspections matched w. DENUE		Inspections matched with DNE	
	Share	Share	$\|(2)-(1)\|$	Share	$\|(4)-(1)\|$
Inspection Type					
extraordinary	0.712	0.694	0.018	0.705	0.007
ordinary	0.241	0.258	0.017	0.248	0.007
follow - up	0.035	0.038	0.003	0.036	0.001
Inspection Topic					
gral labor cond	0.340	0.348	0.009	0.348	0.008
security & hygiene	0.371	0.389	0.018	0.377	0.006
training	0.117	0.123	0.006	0.120	0.003
Inspection Year					
2017	0.287	0.284	0.002	0.286	0.001
2018	0.290	0.280	0.010	0.286	0.004
2019	0.130	0.135	0.005	0.133	0.002
2020	0.107	0.114	0.007	0.109	0.002
2021	0.125	0.125	0.000	0.125	0.000
N inspections	206 749	159 581		188 515	

Notes: This table shows shares of inspections by type, topic, and date for all inspections (column 1), inspections matched with DENUE (column 2), and inspections matched with DNE (column 4). Columns (3) and (5) show the difference in shares between the original data and the matched datasets. [Back to Section 3.2]

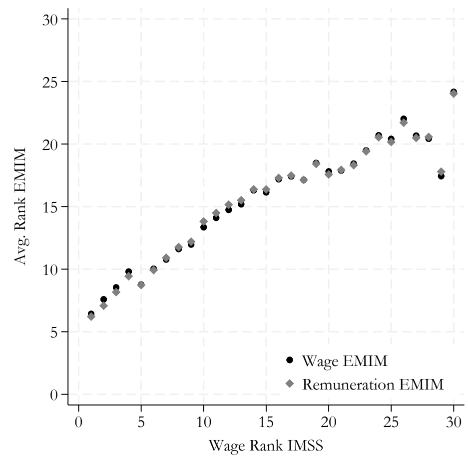
Sample construction with social security data (IMSS). For the results with the establishment data (EMIM) and social security data (IMSS) to be comparable, it is necessary to work with a sample from the social security data that is comparable to the EMIM data. We construct this sample in IMSS by replicating the EMIM distribution across months, 3-digit manufacturing sectors, establishment sizes (five categories), and cities. Within each cell, we select the same number of IMSS and EMIM establishments, ordered by average wage (a proxy for productivity). Although EMIM includes both formal and informal workers, and wage concepts may differ, we find in Appendix Figure B.1 strong correlations in the number of establishments or average wage ranks, respectively, between both datasets. Our final IMSS sample is a balanced panel of approximately 9,000 establishments.

Figure B.1: Sample Comparability EMIM - IMSS

(a) Number of Establishments



(b) Wages



Notes: This figure compares data from the EMIM and IMSS samples across the number of establishments and wages, aggregated by month, firm size (5 groups), 3-digit manufacturing sector, and city. Panel (a) plots the average number of establishments in the EMIM sample against the number of establishments in the IMSS sample. Panel (b) compares wage rankings between the EMIM and IMSS samples. The plot shows the average wage rank in EMIM (for both wage and remuneration measures) against the wage rank in IMSS.

C Appendix: Model

In this section, we derive the solution to the model presented in Section 2. We begin by microfounding the labor supply function 2 using a static discrete choice framework, as is common in the monopsony literature (Card et al., 2018; Berger et al., 2022). The indirect utility of worker i for working in firm j is:

$$U_i(w_j, a_j) = \beta_w \cdot log(w_j) + \beta_a \cdot log(a_j) + \epsilon_{ij}, \tag{12}$$

where ϵ_{ij} is an idiosyncratic shock of working at firm j, due to commuting costs, heterogeneous preferences, or heterogeneous information frictions, among others. We assume ϵ_{ij} follows a GEV Type I extreme value distribution. Therefore, each firm will face a firm-specific labor supply function of the following form:

$$n^s(w_j, a_j) = \left(w_j^{\beta_w} \cdot a_j^{\beta_a}\right)^{\theta_j}, \tag{13}$$

where the elasticity of labor supply with respect to the wage is captured by $\eta_{j;n,w} \equiv \frac{\partial n_j}{\partial w_j}\frac{w_j}{n_j} = \beta_w \theta_j$ and the elasticity with respect to the non-wage benefit by $\eta_{j;n,a} \equiv \frac{\partial n_j}{\partial a_j}\frac{a_j}{n_j} = \beta_a \theta_j$. While we do not explicitly model θ_j in this framework, Berger et al. (2022) show that in general equilibrium the labor supply elasticity faced by the firm, θ_j, will depend negatively on its labor market share. This result motivates using labor market share, or labor market concentration in the empirical analysis as the relevant proxy for a low firm-level labor supply elasticity.[50]

Firms produce output with labor n_j and firm-specific productivity z_j. Firms pay firm-specific wages w_j and offer non-wage benefits a_j. There exists a minimum level of mandatory labor benefits a_{min} imposed by the government. Enforcement of this minimum level is imperfect. Thus, a firm can decide to provide a level of a_j below a_{min}. If this is the case and the firm is inspected, which occurs with probability p, it must pay a fine which is linearly increasing in its total employment n_j and in the difference between the minimum level of mandatory benefits and the level of benefits it provides. Thus, expected firm profits are given by the following expression:

$$\Pi(w_j, a_j) = \begin{cases} z_j n_j - w_j n_j - k a_j n_j & \text{if } \ a_j \geq a_{min}, \\ z_j n_j - w_j n_j - k a_j n_j - p \cdot \phi n_j (a_{min} - a_j) & \text{if } a_j < a_{min}, \end{cases}$$

We assume that marginal provision costs of labor benefits are larger than expected marginal non-provision costs (fines): $k > p \cdot \phi$. If the firm provides $a_j < a_{min}$, we state the firm is violating workplace regulations; if $a_j \geq a_{min}$, we state that the firm is in compliance with workplace regulations.

C.1 Optimal firm decision before enforcement

C.1.1 Scenario where firm violates workplace regulations $a_j < a_{min}$

The first order conditions (FOC) with respect to the firm-specific wage and labor benefit levels are:

[50] This result holds under the assumption that ϵ_{ij} follows a nested GEV where the nests correspond to local labor markets, and that there is strategic complementarity across firms in a local labor market.

$$(w_j) : z_j = w_j \left(1 + \frac{1}{\eta_{j;n,w}}\right) + ka_j + p \cdot \phi(a_{min} - a_j), \tag{14}$$

$$(a_j) : z_j = ka_j \left(1 + \frac{1}{\eta_{j;n,a}} \left(1 - p \cdot \frac{\phi}{k}\right)\right) + w_j + p \cdot \phi(a_{min} - a_j). \tag{15}$$

If we join both FOCs, we get:

$$\frac{w_j}{\eta_{j;n,w}} = \frac{ka_j}{\eta_{j;n,a}} \left(1 - p \cdot \frac{\phi}{k}\right). \tag{16}$$

Using (15) and (16) we obtain the following expressions for a_j and w_j:

$$a_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p \cdot \phi)}, \tag{17}$$

$$w_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)}. \tag{18}$$

Using (17) and (18) we obtain the following expression for total employment:

$$n_j^v = \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)}\right)^{\theta_j \beta_w} \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p \cdot \phi)}\right)^{\theta_j \beta_a}. \tag{19}$$

Profits will be given by:

$$\Pi^v = \frac{1}{\eta_{j;n,w}} \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}\right)^{\theta_j \beta_w + 1} \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)\left(1 - p \cdot \frac{\phi}{k}\right)}\right)^{\theta_j \beta_a}. \tag{20}$$

Finally, note that the firm will only be in this scenario if the expression of a_j in Equation 17 is lower than a_{min}. This simplifies to:

$$z_j < \underbrace{ka_{min}\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right) - p \cdot \phi \cdot a_{min}\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a}\right)}_{\Omega_0}. \tag{21}$$

C.1.2 Scenario where firm complies with workplace regulations $a_j > a_{min}$

We refer to the case when the firm provides a value of a_j strictly above a_{min} as *unconstrained compliance*. In this scenario, the FOCs are:

$$(w_j) : z_j = w_j \left(1 + \frac{1}{\eta_{n,w}}\right) + ka_j, \tag{22}$$

$$(a_j) : z_j = ka_j \left(1 + \frac{1}{\eta_{n,a}}\right) + w_j. \tag{23}$$

If we join both FOC we get:

$$\frac{w_j}{ka_j} = \frac{\eta_{n,w}}{\eta_{n,a}}, \tag{24}$$

where the ratio of marginal costs (LHS) is equal to the ratio of labor supply elasticities (RHS). The optimal a_j and w_j are:

$$a_j^{uc} = \frac{z_j}{k\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}, \tag{25}$$

$$w_j^{uc} = \frac{z_j}{\left(\frac{1}{\theta_j\beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}. \tag{26}$$

Employment and profits are given by:

$$n_j^{uc} = \left(\frac{z_j}{\left(\frac{1}{\theta_j\beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}\right)^{\theta_j\beta_w} \left(\frac{z_j}{k\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}\right)^{\theta_j\beta_a}, \tag{27}$$

$$\Pi^{uc} = \frac{1}{\eta_{n,w}} \left(\frac{z_j}{\left(\frac{1}{\theta_j\beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}\right)^{\theta_j\beta_w+1} \left(\frac{z_j}{k\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}\right)^{\theta_j\beta_a}, \tag{28}$$

Finally, note that the firm will only be in this scenario if the expression of a_j in Equation (25) is greater than a_{min}. This simplifies to:

$$z_j > \underbrace{ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}_{\Omega_1}. \tag{29}$$

C.1.3 Scenario where firm complies with workplace regulations $a_j = a_{min}$

We refer to the case when the firm provides a value of $a_j^{cc} = a_{min}$ as *constrained compliance*. The marginal cost of labor in this scenario is:

$$w_j\left(1 + \frac{1}{\theta_j\beta_w}\right) + ka_{min}. \tag{30}$$

The resulting wage, labor, and profits are:

$$w_j^{cc} = \frac{z_j - ka_{min}}{1 + \frac{1}{\theta_j\beta_w}}, \tag{31}$$

$$n_j^{cc} = \left(\frac{z_j - ka_{min}}{1 + \frac{1}{\theta_j\beta_w}}\right)^{\beta_w\theta_j} (a_{min})^{\beta_a\theta_j}, \tag{32}$$

$$\Pi^{cc} = \frac{1}{\eta_{n,w}} \left(\frac{z_j - ka_{min}}{1 + \frac{1}{\theta_j\beta_w}}\right)^{\theta_j\beta_w+1} (a_{min})^{\theta_j\beta_a}. \tag{33}$$

C.1.4 When do firms violate regulations

Using Equations (21) and (29), we arrive at the following expressions for when the firm will decide to violate:

Non-compliance (violation). If $z_j < \Omega_0$ the optimal decision of the firm will be to violate and provide $a_j < a_{min}$. The optimal a_j and w_j will be:

$$a_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)\left(1 - p \cdot \frac{\phi}{k}\right)}, \qquad w_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)}.$$

Constrained compliance. If $z \in [\Omega_0 : \Omega_1]$ the optimal decision of the firm will be to not violate (comply), and provide $a_j = a_{min}$. The optimal a_j and w_j will be:

$$a_j^{cc} = a_{min}, \qquad w_j^{cc} = \frac{z_j - k a_{min}}{1 + \frac{1}{\theta_j \beta_w}}.$$

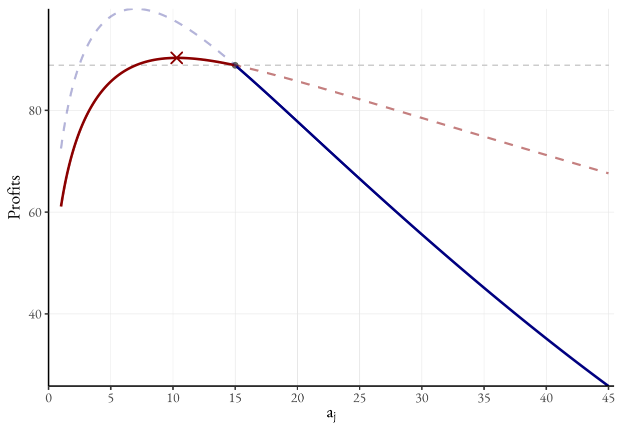
Unconstrained compliance. If $z_j > \Omega_1$, the optimal decision of the firm will be to comply with workplace regulations and provide $a_j > a_{min}$. The optimal a_j and w_j will be:

$$a_j^{uc} = \frac{z_j}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}, \qquad w_j^{uc} = \frac{z_j}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)}.$$

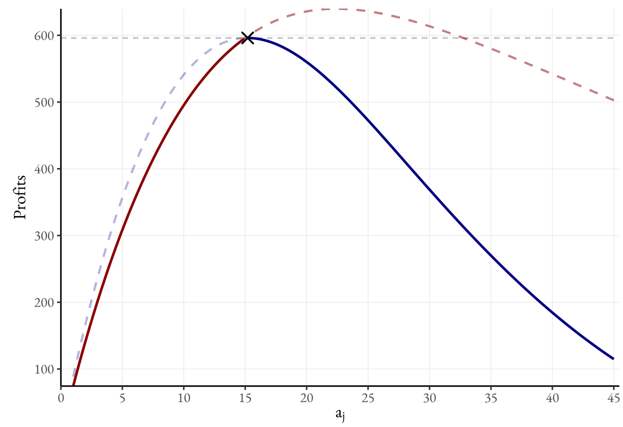
All else equal, firms with higher productivity, z_j, are more likely to comply with workplace regulations. Second, when $k > p \cdot \phi$, firms with lower θ_j that possess greater labor market power are more inclined to violate regulations. This occurs because workers in these firms are less responsive to unfavorable working conditions, allowing employers to reduce benefits without this impacting their capacity to hire and retain workers. Third, the prevalence of non-compliance decreases as the probability of inspection, p, and the penalty severity, ϕ, increase, as stronger enforcement raises the cost of violating workplace regulations.

In Figure B.2 we report illustrative examples of firm profits as a function of working conditions a_j for different values of θ_j, our main parameter of interest, keeping all other parameters constant. We show the three cases: Non-compliance, constrained compliance, and compliance. In general, this figure illustrates that the degree of labor market power θ_j, can shift otherwise similar firms from non-compliance to constrained compliance to compliance.

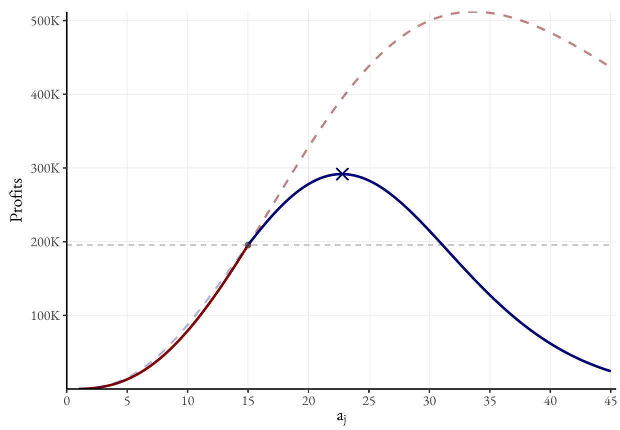
Figure B.2: Illustrative examples of firm profits as a function of a_j for different levels of θ_j



(a) Non-compliance is optimal



(b) Constrained compliance is optimal



(c) Compliance is optimal

Notes: These figures plot firm profits as a function of a_j for different values of θ_j, capturing varying degrees of monopsony power. The black dot indicates profits at $a_{\min} = 15$, with the corresponding dashed horizontal line. Optimal choices via profit maximizing are marked by "x". In Panel (a), the firm's optimal choice is non-compliance; in Panel (b), it is constrained compliance ($a_j = a_{\min}$); and in Panel (c), it is compliance ($a_j > a_{\min}$). Dashed blue lines show profits without expected penalties and are infeasible. Dashed red lines represent outcomes with both penalties for non-compliance and subsidies for over-compliance, which are also infeasible. Parameter values: $z_j = 70$, $k = 1.1$, $p = 0.5$, $\phi = 1.5$, $\{\beta_w, \beta_a\} = \{0.7, 0.3\}$. Panel (a) $\theta_j = 0.3$, Panel (b) $\theta_j = 1$, Panel (c) $\theta_j = 3$.

C.2 Effect of enforcing compliance

In this section, we examine the impact of enforcing compliance on a firm that would otherwise choose $a_j < a_{min}$. Notably, for a firm whose optimum before enforcement is non-compliance, enforcement will always move it to the constrained compliance scenario. This occurs because a non-compliant firm's optimal choice without enforcement satisfies $z_j < \Omega_0$ in Equation (21). Since $\Omega_0 < \Omega_1$, condition (29) cannot be satisfied, and the optimal a_j under enforced compliance will never exceed a_{min}. Consequently, the firm will provide $a_j = a_{min}$. Furthermore, firm profits will weakly decrease under enforced compliance, since the firm's newly required bundle was already feasible before enforcement, but the firm had previously chosen a different bundle to maximize profits.

The proportional change in wages is given by Equation (34), which is the ratio of Equation (31) to Equation (18)—that is, the optimal wage under constrained compliance relative to the optimal wage under violation:

$$\Delta\%w_j = \frac{z_j - ka_{min}}{z_j - p\phi a_{min}} \left(1 + \frac{\beta_a}{\frac{1}{\theta_j} + \beta_w}\right), \tag{34}$$

which is lower than 1 for all previously non-compliant firms.[51]

The proportional change in employment is given by Equation (35), which is the ratio of Equation (32) to Equation (19)—that is, firm size under constrained compliance relative to firm size under violation:

$$\Delta\%n_j = \left(\underbrace{\frac{a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p\phi)}{z_j - p\phi a_{min}}}_{>1}\right)^{\theta_j\beta_a} \left(\underbrace{\Delta\%w_j}_{<1}\right)^{\theta_j\beta_w} \lesseqgtr 1 \tag{35}$$

C.3 Model solution with alternative labor supply

In this section, we solve the model under three specific cases of the labor supply function in 13. First, we analyze a setting where the firm faces an upward-sloping labor supply curve that depends solely on the wage ($\beta_a = 0$), as in conventional monopsony models (Card et al., 2018). Second, we assume that firms set w_j and a_j in a perfectly competitive labor market, where firms take the value of a job as given ($\theta \to \infty$) (Dube et al., 2022; Mas, 2024). Third, we analyze the case where the firm faces an upward-sloping labor supply curve that depends solely on the non-wage conditions ($\beta_w = 0$).

We obtain two key results. First, in the alternative scenario where $\beta_a = 0$ such that firms do not have amenity setting power, if the price per unit of amenity k and the fine per worker ϕ are constant across firms, the only factor driving heterogeneity in a firm's decision to violate regulations is the probability of inspection p. Second, in both alternative scenarios considered, enforcing a minimum level of amenities $a_{\min}$ on a non-compliant firm always leads to a reduction in employment. The intuition behind this result is as follows. In the scenario where the labor supply curve is upward sloping in the wage, the increase in amenities due to enforcement raises the marginal cost of labor but does not increase labor supply, resulting in a decline in employment. In the competitive environment, firms minimize marginal cost of labor $w_j + a_j$ subject to providing a level of worker utility they take as given. An increase in enforcement causes firms to deviate from this cost-minimizing allocation, causing a decrease in employment.

C.3.1 Labor supply depending only on w_j

Now we consider a setting in which labor supply depends solely on the wage:

[51] The expression is < 1 when $z_j < ka_{min} + (k - p\phi)a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a}\right)$, which is exactly the condition for firms to choose to be non-compliant.

$$n^s = w^{\theta_j \beta_w}. \tag{36}$$

As there is no benefit for the firm of providing a_j (the derivative of the profit function wrt a_j is strictly negative) the firm will provide the lowest value of benefits possible. A non-compliant firm will choose $a_j = 0$. As in the benchmark case we set $f(n_j) = n_j$. Thus, the expressions for labor and profits are:

$$n_j^{violate} = (z_j - p\phi a_{min})^{\theta_j \beta_w} \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w}, \tag{37}$$

$$\Pi_j^{violate} = \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w} \left(\frac{1}{1 + \theta_j \beta_w} \right) (z_j - p\phi a_{min})^{1+\beta_w \theta_j}. \tag{38}$$

If the firm complies, it will never choose $a_j > a_{min}$. In this case, labor and profits are given by:

$$n_j^{comply} = (z_j - k a_{min})^{\theta_j \beta_w} \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w}, \tag{39}$$

$$\Pi_j^{comply} = \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w} \left(\frac{1}{1 + \theta_j \beta_w} \right) (z_j - k a_{min})^{1+\beta_w \theta_j}. \tag{40}$$

Comparing the two profit expressions, the firm will choose to violate iff $k > p\phi$. Therefore, assuming constant k and ϕ across firms, no firm characteristics should predict violation after controlling for p. Additionally, by comparing Equations (37) and (39), we can see that in the case that a firm does not comply with a_{min} ($k > p\phi$), then enforcement of a_{min} will reduce employment n_j.

C.3.2 Perfectly competitive labor market

In a perfectly competitive labor market, the elasticity of the labor supply function in Equation (13), $\theta_j \to \infty$. The utility of total compensation $\bar{U}$, is determined in the competitive market and taken as given by the firm (Mas, 2024).

$$w_j^{\beta_w} \cdot a_j^{\beta_a} = \bar{U}. \tag{41}$$

Using Equation 41, we can express amenities as a function of wages: $a_j = \bar{U}^{\frac{1}{\beta_a}} \cdot w_j^{-\frac{\beta_w}{\beta_a}}$. Substituting this into the firm's profit function yields:

$$\Pi(w_j, a_j) = \begin{cases} z_j f(n_j) - w_j n_j - k\bar{U}^{\frac{1}{\beta_a}} w_j^{-\frac{\beta_w}{\beta_a}} n_j & \text{if } \; a_j \geq a_{min}, \\ z_j f(n_j) - w_j n_j - k\bar{U}^{\frac{1}{\beta_a}} w_j^{-\frac{\beta_w}{\beta_a}} n_j - p \cdot \phi n_j (a_{min} - \bar{U}^{\frac{1}{\beta_a}} w_j^{-\frac{\beta_w}{\beta_a}}) & \text{if } a_j < a_{min}. \end{cases}$$

To ensure that the firm's maximization problem yields a finite solution for employment, we assume that the production function exhibits diminishing marginal returns to labor, i.e., $f''(n_j) < 0$.[52]

[52] If instead we use a linear production function, $f(n_j) = n_j$, as in the benchmark case, the optimal firm size would be either zero or unbounded, depending on the parameter values.

When the firm does not comply with minimum amenities requirements ($a_j < a_{min}$), the first-order conditions with respect to w_j and n_j are:

$$(w_j): \quad w_j = \left((k - p\phi)\bar{U}^{\frac{1}{\beta_a}} \frac{\beta_w}{\beta_a}\right)^{\frac{\beta_a}{\beta_a+\beta_w}}, \tag{42}$$

$$(n_j): \quad z_j f'(n_j) = w_j + (k - p\phi) \underbrace{\bar{U}^{\frac{1}{\beta_a}} w_j^{-\frac{\beta_w}{\beta_a}}}_{a_j} + p \cdot \phi a_{min}. \tag{43}$$

The firm will only be in the non-compliance scenario if $a_j < a_{min}$. Substituting the expression for a_j into 42, this implies:

$$\bar{U}^{\frac{1}{\beta_a}} \cdot \left((k - p\phi)\bar{U}^{\frac{1}{\beta_a}} \frac{\beta_w}{\beta_a}\right)^{\frac{-\beta_w}{\beta_a+\beta_w}} < a_{min}. \tag{44}$$

When the firm complies with workplace regulations by providing exactly $a_j = a_{min}$ (constrained compliance), we can use Equation (41) and the FOC of the profit function with respect to n_j to derive the expressions for wages and labor:

$$w_j = \left(\frac{\bar{U}}{a_{min}^{\beta_a}}\right)^{\frac{1}{\beta_w}}, \tag{45}$$

$$z_j f'(n_j) = \left(\frac{\bar{U}}{a_{min}^{\beta_a}}\right)^{\frac{1}{\beta_w}} + k a_{min}. \tag{46}$$

In this setting, enforcement of a_{min} among non-compliant firms will unambiguously reduce employment. This result follows from the fact that in a perfectly competitive labor market, the firm problem can be expressed as a cost minimization problem where the firm chooses w_j and a_j to minimize $w_j + a_j$, subject to the utility constraint (41) (Mas, 2024). The marginal cost resulting from this optimization problem determines the optimal level of labor. For non-compliant firms, this optimal level of employment is defined by Equation 43, where the right-hand side of the equality corresponds to the (minimized) marginal cost of labor. When compliance is enforced, the firm is constrained to offer the bundle $\{a_{\min}, w_j\}$, which deviates from the cost-minimization bundle, resulting in an increase in the marginal cost of labor. This increase in the marginal cost of employment causes the amount of labor hired, defined by Equation (46), to decrease. This negative effect on employment is amplified in the presence of downward nominal wage rigidity, as wages cannot adjust downward, leading to a sharper increase in the marginal cost of labor.

C.3.3 Labor supply depending only on a_j

Now we consider a setting in which the firm takes wages as given ($w = \tilde{w}$) and faces a firm-specific labor supply curve that depends only on the level of amenities offered:

$$n^s = a_j^{\theta_j \beta_a}. \tag{47}$$

Firm profits are given by the following expression:

$$\Pi(a_j) = \begin{cases} z_j a_j^{\theta_j \beta_a} - \tilde{w} a_j^{\theta_j \beta_a} - k a_j^{\theta_j \beta_a + 1} & \text{if } \ a_j \geq a_{min}, \\ z_j a_j^{\theta_j \beta_a} - \tilde{w} a_j^{\theta_j \beta_a} - k a_j^{\theta_j \beta_a + 1} - p \cdot \phi a_j^{\theta_j \beta_a} (a_{min} - a_j) & \text{if } a_j < a_{min}. \end{cases}$$

In this scenario, the optimal level of amenities if the firm violates and provides $a_j < a_{min}$ is:

$$a_j = \frac{z_j - \tilde{w} - p\phi a_{min}}{\left(1 + \frac{1}{\theta_j \beta_a}\right)(k - p\phi)}, \tag{48}$$

while labor is given by:

$$n_j = \left(\frac{z_j - \tilde{w} - p\phi a_{min}}{\left(1 + \frac{1}{\theta_j \beta_a}\right)(k - p\phi)} \right)^{\theta_j \beta_a}. \tag{49}$$

The firm will be in the non-compliance scenario if the expression in Equation (48) is lower than a_{min}. This translates into:

$$z_j - \tilde{w} < k a_{min} \left(\frac{1}{\theta_j \beta_a} + 1 \right) - p \cdot \phi \cdot a_{min} \frac{1}{\theta_j \beta_a}. \tag{50}$$

If the firm complies with a_{min}, as long as $a_{min} < \frac{z_j - w}{k}$, such that profits are weakly positive, the optimal level of labor will be determined by the labor supply function (47) and given by:

$$a_{min}^{\theta_j \beta_a}. \tag{51}$$

Notably, in this scenario, Predictions 1 and 2 from the benchmark model are confirmed. In contrast to the alternative case where labor supply depends solely on wages, Equation (50) highlights that the firm's decision to comply or not is influenced by firm-specific and labor market characteristics, namely z_j, θ_j, and β_a. Furthermore, comparing firm-level employment under non-compliance and compliance, as shown in Equations (49) and (51), reveals that as long as $a_{min} < \frac{z_j}{k}$, enforcement of minimum amenity standards leads to an increase in employment. This occurs through a positive adjustment along the labor supply curve.

D Model with more general CES labor supply function

In this section, we present a version of the model introduced in Section 2 that incorporates a more general labor supply function, allowing for varying degrees of complementarity between wages and amenities. Following Dube et al. (2022), the value of a job for a worker i at firm j is $V_j \cdot e_{ij}$, where e_{ij} follows a Fréchet distribution with shape parameter $\frac{1}{\theta_j}$. V_j is a constant elasticity of substitution (CES) aggregate of wages w_j and amenities a_j:

$$V_j = \left(\beta_w w_j^{\frac{\sigma - 1}{\sigma}} + \beta_a a_j^{\frac{\sigma - 1}{\sigma}} \right)^{\frac{\sigma}{\sigma - 1}}, \tag{52}$$

where σ denotes the elasticity of substitution between w and a. Note that $\sigma = 1$ corresponds to the labor supply function in Section C. β_w and β_a are parameters governing the relative importance of wages and amenities in the utility function with $\beta_w + \beta_a = 1$. Therefore, the firm-specific labor supply elasticity is:

$$n^s(w_j, a_j) = V_j^{\theta_j} \tag{53}$$

where θ_j is the labor supply elasticity with respect to the value of the job V_j.

D.1 Scenario where the firm violates workplace regulations $a_j < a_{min}$

We refer to the case when the firm provides a value of a_j that is strictly below a_{min} as *non-compliant*. We derive the first-order conditions (FOCs) with respect to the firm-specific wage w_j and labor benefit level a_j. For simplicity, we abbreviate V_j as V. In general, V_w and V_a denote the partial derivatives of V with respect to w_j and a_j, respectively.

Using the fact that $n_j = V^{\theta_j}$, the FOC with respect to w_j is:

$$(w_j): \quad \Pi_w = [z_j - w_j - ka_j - p\phi(a_{min} - a_j)]\, \theta_j V^{\theta_j - 1} V_w - V^{\theta_j} = 0 \tag{54}$$

Dividing through by $V^{\theta_j - 1}$ and substituting $V_w = \beta_w \left(\frac{V}{w_j}\right)^{1/\sigma}$ gives a simplified form:

$$(w_j): \quad [z_j - w_j - ka_j - p\phi(a_{min} - a_j)]\, \theta_j V^{1/\sigma} - \frac{w_j^{1/\sigma}}{\beta_w} V = 0 \tag{55}$$

Analogously, the FOC with respect to a_j is:

$$(a_j): \quad \Pi_a = [z_j - w_j - ka_j - p\phi(a_{min} - a_j)]\, \theta_j V^{\theta_j - 1} V_a - (k - p\phi) V^{\theta_j} = 0 \tag{56}$$

Using $V_a = \beta_a \left(\frac{V}{a_j}\right)^{1/\sigma}$ and simplifying:

$$(a_j): \quad [z_j - w_j - ka_j - p\phi(a_{min} - a_j)]\, \theta_j V^{1/\sigma} - (k - p\phi) \frac{a_j^{1/\sigma}}{\beta_a} V = 0 \tag{57}$$

Combining equations (55) and (57), we obtain the relationship between optimal w_j and a_j:

$$\boldsymbol{\frac{w_j}{a_j} = \left(\frac{\beta_w}{\beta_a}(k - p\phi)\right)^{\sigma}} \tag{58}$$

We use (58) and (55) to obtain the following expression for optimal w_j, a_j and n_j:

$$\boldsymbol{w_j = \frac{(z_j - p\phi a_{min})\theta_j}{(1 + \theta_j)\left(1 + \left(\frac{\beta_a}{\beta_w}\right)^{\sigma} (k - p\phi)^{1-\sigma}\right)}} \tag{59}$$

$$\boldsymbol{a_j = \frac{(z_j - p\phi a_{min})\theta_j}{(1 + \theta_j)(k - p\phi)\left(1 + \left(\frac{\beta_w}{\beta_a}\right)^{\sigma} (k - p\phi)^{\sigma-1}\right)}} \tag{60}$$

$$\boldsymbol{n_j} = \left[\frac{(z_j - p\phi a_{min})\theta_j}{(1+\theta_j)} \left[\beta_w \left(\frac{1}{1+\left(\frac{\beta_w}{\beta_a}\right)^{\sigma} (k-p\phi)^{1-\sigma}} \right)^{\frac{\sigma-1}{\sigma}} + \beta_a \left(\frac{1}{(k-p\phi)+\left(\frac{\beta_w}{\beta_a}\right)^{\sigma} (k-p\phi)^{\sigma}} \right)^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}} \right]^{\theta_j} \tag{61}$$

Finally, note that the firm will only be in this scenario if the expression of a_j in Equation (60) is lower than a_{min}. This simplifies to:

$$z_j < \underbrace{k a_{min} \frac{(1+\theta_j)}{\theta_j} \left(1 + \left(\frac{\beta_w}{\beta_a}\right)^{\sigma} (k-p\phi)^{\sigma-1} \right) - p\phi a_{min} \frac{(1+\theta_j)}{\theta_j} \left[\left(\frac{\beta_w}{\beta_a}\right)^{\sigma} (k-p\phi)^{\sigma-1} + \frac{1}{\theta_j} \right]}_{\Omega_0} \tag{62}$$

D.2 Scenario where firm complies with workplace regulations $a_j = a_{min}$

We refer to the case when the firm provides a value of $a_j = a_{min}$ as *constrained compliance.*

The FOC with respect to the wage is:

$$(w_j): \quad [z_j - w_j - k a_{min}]\theta_j V^{\frac{1}{\sigma}} - \frac{w_j^{\frac{1}{\sigma}}}{\beta_w} V = 0 \tag{63}$$

Rearranging terms, we obtain the following expression for the FOC:

$$(w_j): \quad [z_j - w_j - k a_{min}]\theta_j - \frac{w_j^{\frac{1}{\sigma}}}{\beta_w} V^{\frac{\sigma-1}{\sigma}} = 0 \tag{64}$$

$$(w_j): \quad [z_j - w_j - k a_{min}]\theta_j = w_j + \frac{\beta_a}{\beta_w} a_{min}^{\frac{\sigma-1}{\sigma}} w_j^{\frac{1}{\sigma}} \tag{65}$$

While there is no general, analytical closed-form solution, we provide such solutions for three specific cases:

Case 1: Perfect substitutes, $\sigma \to \infty$:

$$\boldsymbol{w_j} = \frac{(z_j - k a_{min})\theta_j - \frac{\beta_a}{\beta_w} a_{min}}{1+\theta_j} \tag{66}$$

$$\boldsymbol{n_j} = \left[\frac{\theta_j \left(\beta_w (z_j - k a_{min}) + \beta_a a_{min} \right)}{1+\theta_j} \right]^{\theta_j} \tag{67}$$

Case 2: Cobb Douglas, $\sigma \to 1$:

$$\boldsymbol{w_j} = \frac{(z_j - k a_{min})\theta_j}{1 + \theta_j + \frac{\beta_a}{\beta_w}} \tag{68}$$

$$\boldsymbol{n_j} = \left[\left(\frac{(z_j - k a_{min})\theta_j}{1 + \theta_j + \frac{\beta_a}{\beta_w}} \right)^{\beta_w} a_{min}^{\beta_a} \right]^{\theta_j} \tag{69}$$

Case 3: Leontief, $\sigma \to 0$:
We can rewrite

$$(w_j): \quad [z_j - w_j - ka_{min}]\theta_j = w_j + \frac{\beta_a}{\beta_w} a_{min} (\frac{w_j}{a_{min}})^{\frac{1}{\sigma}} \tag{70}$$

and distinguish between $\frac{w_j}{a_{min}} < 1$ and $\frac{w_j}{a_{min}} \geq 1$:

$$\boldsymbol{w_j(\sigma \to 0) = \min\left(\frac{(z_j \ - k\, a_{min})\theta_j}{1+\theta_j},\ a_{min}\right)} \tag{71}$$

$$\boldsymbol{n_j = \left[\min\left(\frac{(z_j - ka_{min})\theta_j}{1+\theta_j},\, a_{min}\right)\right]^{\theta_j}} \tag{72}$$

Comparing all three cases, we see that wages **decrease in Case 1** with the relative importance of working conditions $\left(\frac{\beta_a}{\beta_w}\right)$, since wages and working conditions are perfect substitutes. This effect **still holds in Case 2** but is weaker, because the Cobb–Douglas specification implies a lower degree of substitution between w_j and a_j. In **Case 3**, wages are **bounded by the minimum working conditions**, as both job components are perfect complements.

D.3 Scenario where firm complies with workplace regulations $a_j > a_{min}$

We refer to the case when the firm provides a value of a_j strictly above a_{min} as *unconstrained compliance*. Using the fact that $n_j = V_j^{\theta}$, the FOC with respect to w_j is:

$$(w_j): \quad [z_j - w_j - ka_j]\theta_j V^{\frac{1}{\sigma}} - \frac{w_j^{\frac{1}{\sigma}}}{\beta_w} V = 0 \tag{73}$$

$$(a_j): \quad [z_j - w_j - ka_j]\theta_j V^{\frac{1}{\sigma}} - k\frac{a_j^{\frac{1}{\sigma}}}{\beta_a} V = 0 \tag{74}$$

We combine equalities in (73) and (74) to arrive at the following expression determining the relationship between optimal wages and amenities:

$$\frac{w_j}{a_j} = \left(\frac{\beta_w}{\beta_a} k\right)^{\sigma} \tag{75}$$

We use (75) and (73) to obtain the following expression for optimal w_j, a_j and n_j:

$$\boldsymbol{w_j = \frac{z_j \theta_j}{(1+\theta_j)\left(1 + \left(\frac{\beta_a}{\beta_w}\right)^{\sigma} k^{1-\sigma}\right)}} \tag{76}$$

$$\boldsymbol{a_j = \frac{z_j \theta_j}{(1+\theta_j) k \left(1 + \left(\frac{\beta_w}{\beta_a}\right)^{\sigma} k^{\sigma-1}\right)}} \tag{77}$$

$$\boldsymbol{n_j = \left[\frac{z_j\theta_j}{(1+\theta_j)}\left[\beta_w\left(\frac{1}{1+\left(\frac{\beta_w}{\beta_a}\right)^{\sigma}k^{1-\sigma}}\right)^{\frac{\sigma-1}{\sigma}}+\beta_a\left(\frac{1}{k+\left(\frac{\beta_w}{\beta_a}\right)^{\sigma}k^{\sigma}}\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}}\right]^{\theta_j}} \tag{78}$$

Note that the firm will only be in this scenario if the expression of a_j in Equation is greater than a_{min}. This simplifies to:

$$z_j > \underbrace{k a_{min}\frac{(1+\theta_j)}{\theta_j}\left(1+\left(\frac{\beta_w}{\beta_a}\right)^{\sigma}k^{\sigma-1}\right)}_{\Omega_1} \tag{79}$$

D.4 The decision to comply with minimum working conditions a_{min}

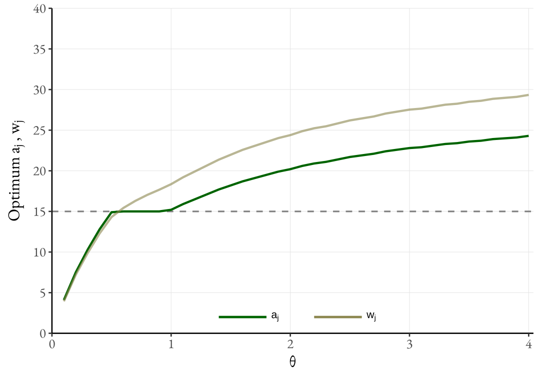
Based on the derivations in the previous sub-sections, the firm is ***non-compliant*** if $z_j < \Omega_0$, derived in inequality (62). The optimal decision of the firm will be to violate and provide $a_j < a_{min}$. If $z \in [\Omega_0, \Omega_1]$, defined in (62), (79), the firm will be ***constrained compliant***. The corresponding optimal decision of the firm will be to provide $a_j = a_{min}$. If $z_j > \Omega_1$, the firm will be ***compliant*** with $a_j > a_{min}$.

As in the main version of the model presented in Section 2, all else equal, firms with lower θ_j, meaning they possess greater labor market power, are more inclined to violate regulations. Second, firms with higher productivity, z_j, are more likely to comply with workplace regulations. This occurs because workers in these firms are less responsive to unfavorable working conditions, allowing employers to reduce benefits without this impacting their capacity to hire and retain workers. Third, firms are more likely to comply with workplace regulations if workers value wages (β_w) relatively less than working conditions (β_a). Fourth, the prevalence of non-compliance decreases as the probability of inspection, p, and the penalty severity, ϕ, increase, as stronger enforcement raises the cost of violating workplace regulations. We illustrate these analytical results with a numerical example in Figure B.3. In this scenario, firms exhibit relatively high market power ($\theta = 0.3$), have low productivity ($z_j = 70$), and face an inspection probability of $p = 0.5$. Workers place comparatively greater weight on wages than on working conditions. Importantly, wages and working conditions are relatively strong complements ($\sigma = 0.2$).

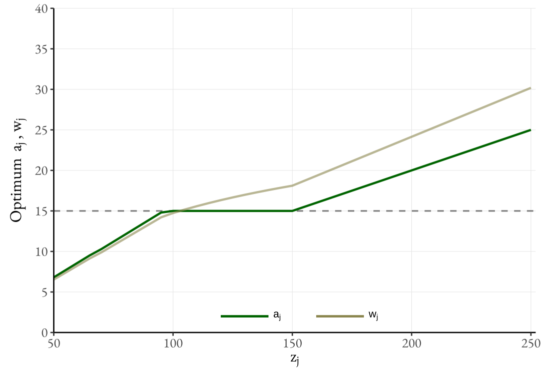
D.5 Effect of enforcing compliance

In this section, we examine the impact of enforcing compliance on a firm that would otherwise choose $a_j < a_{min}$. Notably, for a firm whose optimum before enforcement is non-compliance, enforcement will always move it to the constrained compliance scenario. This occurs because a non-compliant firm's optimal choice satisfies $z_j < \Omega_0$. Since $\Omega_0 < \Omega_1$, condition (79) cannot be satisfied, and the optimal a_j under enforced compliance will never exceed a_{min}. Consequently, the firm will provide $a_j = a_{min}$. Furthermore, firm profits will weakly decrease under enforced compliance, since the firm's newly required bundle was already feasible before enforcement, but the firm had previously chosen a different bundle to maximize profits.

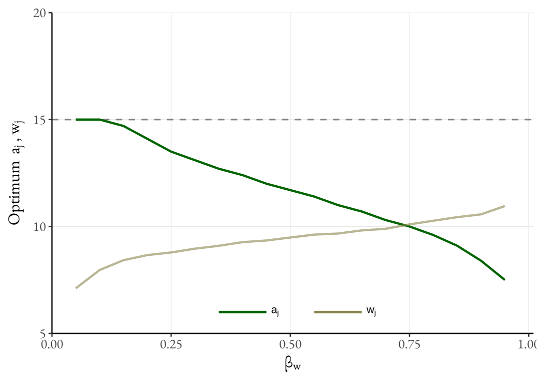
Figure B.3: Optimal a_j, w_j for different parameters



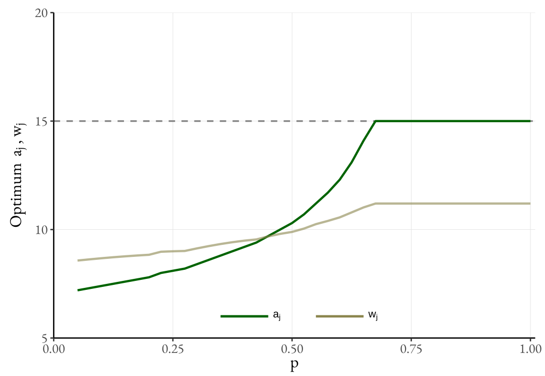
(a) Elasticity of Labor Supply Curve θ_j



(b) Firm Productivity z_j



(c) Relative Importance of Wages β_w



(d) Inspection Probability p

Notes: These figures show the optimal values of a_j in green and w_j in khaki for different parameter values. The black dotted horizontal line corresponds to a_{min}. Panel (a) varies the elasticity of the labor supply curve θ_j. Panel (b) varies firm productivity z_j. Panels (c) varies the relative importance of wages (β_w) in comparison to working conditions (β_a). Panel (d) varies the probability of inspection p. Fixed parameter values if not varied in a panel: $\theta_j = 0.3$, $z_j = 70$, $\{\beta_w, \beta_a\} = \{0.7, 0.3\}$, $p = 0.5$, $k = 1.1$, $\phi = 1.5$, $\sigma = 0.2$.

D.5.1 Wages

Next, we derive the effect on wages by comparing optimal wages in case of constrained compliance (w_j^{cc}) and non-compliance (w_j^{nc}). The wages in each of these scenarios are defined by the FOC with respect to the wage in each case:

$$(w_j^{cc}): \quad [z_j - w_j^{cc} - k a_{min}]\theta_j = w_j^{cc} + \frac{\beta_a}{\beta_w} a_{min}^{\frac{\sigma-1}{\sigma}} \left(w_j^{cc}\right)^{\frac{1}{\sigma}} \tag{80}$$

$$(w_j^{nc}): \quad [z_j - w_j^{nc} - k a_j^* - p\phi(a_{min} - a_j^*)]\theta_j = w_j^{nc} + \frac{\beta_a}{\beta_w} (a_j^*)^{\frac{\sigma-1}{\sigma}} \left(w_j^{nc}\right)^{\frac{1}{\sigma}} \tag{81}$$

Rearranging terms we can obtain the following equality

$$w_j^{cc}(1+\theta_j) + \frac{\beta_a}{\beta_w} a_{min}^{\frac{\sigma-1}{\sigma}} (w_j^{cc})^{\frac{1}{\sigma}} = w_j^{nc}(1+\theta_j) + \frac{\beta_a}{\beta_w} (a_j^*)^{\frac{\sigma-1}{\sigma}} (w_j^{nc})^{\frac{1}{\sigma}} - \theta_j(k - p\phi)(a_{min} - a_j^*) \tag{82}$$

Where $a_j^* < a_{min}$, corresponding to non-compliance before enforcement.

In order to compare the magnitude of w_j^{cc} and w_j^{nc}, we define:

$$f(w_j) \;=\; w_j(1+\theta_j) \;+\; \frac{\beta_a}{\beta_w}\, a_{\min}^{\frac{\sigma-1}{\sigma}}\, w_j^{\frac{1}{\sigma}}.$$

Then

$$f'(w_j) \;=\; (1+\theta_j) \;+\; \frac{1}{\sigma}\,\frac{\beta_a}{\beta_w}\, a_{\min}^{\frac{\sigma-1}{\sigma}}\, w_j^{\frac{1-\sigma}{\sigma}} \;>\; 0,$$

so f is strictly increasing.

Replacing the left-hand side of (82) by $f(w_j^{cc})$ gives

$$f(w_j^{cc}) = w_j^{nc}(1+\theta_j) + \frac{\beta_a}{\beta_w}(a_j^*)^{\frac{\sigma-1}{\sigma}}(w_j^{nc})^{\frac{1}{\sigma}} - \theta_j(k-p\phi)(a_{\min}-a_j^*)$$

and note that

$$f(w_j^{nc}) = w_j^{nc}(1+\theta_j) + \frac{\beta_a}{\beta_w}\, a_{\min}^{\frac{\sigma-1}{\sigma}}\, (w_j^{nc})^{\frac{1}{\sigma}}.$$

Subtracting both gives

$$f(w_j^{cc}) - f(w_j^{nc}) = \frac{\beta_a}{\beta_w}(w_j^{nc})^{\frac{1}{\sigma}}\left[(a_j^*)^{\frac{\sigma-1}{\sigma}} - a_{\min}^{\frac{\sigma-1}{\sigma}}\right] \;-\; \theta_j\,(k-p\phi)\,(a_{\min}-a_j^*). \tag{2}$$

Since f is strictly increasing, $w_j^{cc} = w_j^{nc}$ if and only if

$$\frac{\beta_a}{\beta_w}\,(w_j^{nc})^{\frac{1}{\sigma}}\left[(a_j^*)^{\frac{\sigma-1}{\sigma}} - a_{\min}^{\frac{\sigma-1}{\sigma}}\right] = \theta_j\,(k-p\phi)\,\big(a_{\min}-a_j^*\big),$$

which is equivalent to

$$w_j^{nc} = \left[\frac{\theta_j\,(k-p\phi)\,\big(a_{\min}-a_j^*\big)}{\frac{\beta_a}{\beta_w}\Big((a_j^*)^{\frac{\sigma-1}{\sigma}} - a_{\min}^{\frac{\sigma-1}{\sigma}}\Big)}\right]^{\sigma}.$$

The role of σ in wage effects

The effect of enforcement on wages crucially depends on σ, the parameter governing the degree of complementarity between wages and working conditions. Specifically:

- If $\sigma \geq 1$, then
$$(a_j^*)^{\frac{\sigma-1}{\sigma}} \leq a_{\min}^{\frac{\sigma-1}{\sigma}},$$
so the denominator is ≤ 0 while the numerator is > 0, implying a negative wage.
$$\Longrightarrow\; w_j^{cc} < w_j^{nc} \text{ always.}$$

- If $\sigma < 1$, then
$$(a_j^*)^{\frac{\sigma-1}{\sigma}} > a_{\min}^{\frac{\sigma-1}{\sigma}},$$

so equality $w_j^{cc} = w_j^{nc}$ is possible at

$$w_j^{nc} = \left[\frac{\theta_j \left(k - p\phi\right) \left(a_{\min} - a_j^*\right)}{\frac{\beta_a}{\beta_w} \left(\left(a_j^*\right)^{\frac{\sigma-1}{\sigma}} - a_{\min}^{\frac{\sigma-1}{\sigma}} \right)} \right]^{\sigma}.$$

The numerator reflects the *compliance cost*: the additional resources the firm must allocate to benefits, determined by the rise in employment (through θ_j) and the higher marginal cost of provision. The denominator represents the *relative utility gain from benefits*: it measures how much more valuable the mandated increase in a_j is compared to wages, as implied by the CES structure and the weight $\frac{\beta_a}{\beta_w}$. The resulting wage adjustment under constrained compliance depends on the balance between these two forces.

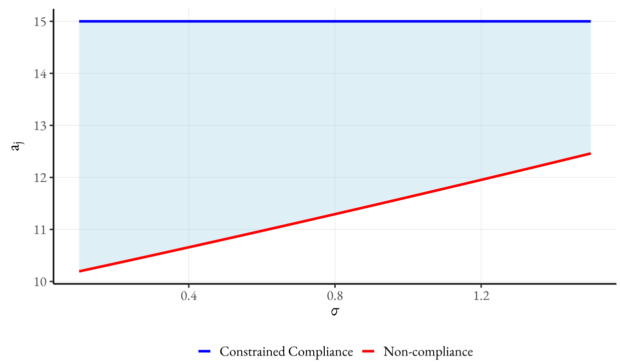
Importantly, enforcement can even lead to higher wages post-compliance ($w_j^{cc} > w_j^{nc}$). When σ is low enough, wages and working conditions act as strong complements. In this case, mandated improvements in a_j are costly for the firm, but reducing wages in response is even more costly, since this would further reduce the firm's capacity to attract workers who value both dimensions jointly. By maintaining or even raising wages, the firm can leverage the enforced improvement in working conditions to attract more workers.

We illustrate these findings in Figure B.4. Panel (a) shows that enforcement raises a_j from the non-compliant to the compliant level for all values of σ and given parameters. Under non-compliance, a_j increases in σ, since the firm substitutes away from w_j toward a_j whenever amenities are below the minimum and the firm faces costly fines. Panel (b) shows the opposite pattern for wages: under non-compliance, w_j decreases in σ. This decline is even stronger under constrained compliance, as firms offer larger wages for small σ, and much lower wages for high σ to offset the additional enforcement costs. For small σ, constrained-compliance wages exceed those under non-compliance, but once $\sigma \gtrsim 0.8$, the opposite holds. Panel (c) illustrates that firm size rises with enforcement, as workers value the higher amenities, reinforced by higher wages for small σ and not fully offset for large σ. Finally, panel (d) shows that firm profits fall under constrained compliance, especially for low σ, where firms provide both higher amenities and higher wages. This attracts more workers but raises costs disproportionately.

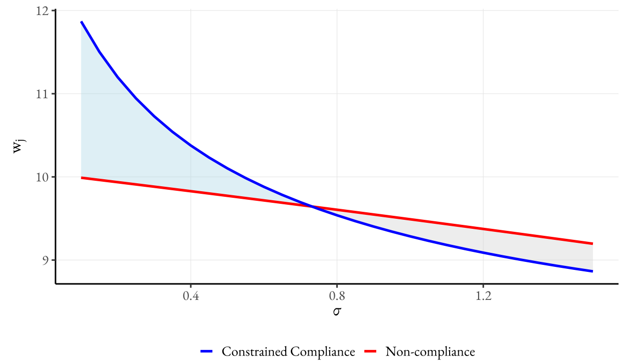
D.5.2 Employment

To assess the effect of enforcement on employment, which is equal to V_j^{θ}, we compare the value of the job V in each scenario. If both a and w increase due to enforcement (i.e. low enough σ), it is straightforward that V and hence labor supply and therefore firm size will increase. However, even if the wage decreases, V may increase, depending on how much workers value improved working conditions relative to wages. This can be seen in the simulation results in Panel (c) of Figure B.4. For these parameter values, the effect of enforcement on total employment is positive for every value of σ plotted, while the effect on wages is only positive for $\sigma \lesssim 0.8$.

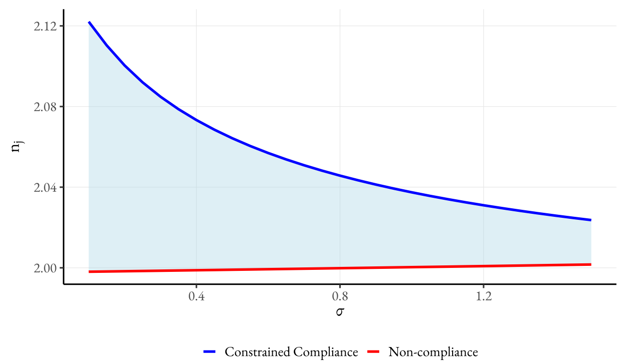
Figure B.4: Effect of Enforcement for Different σ



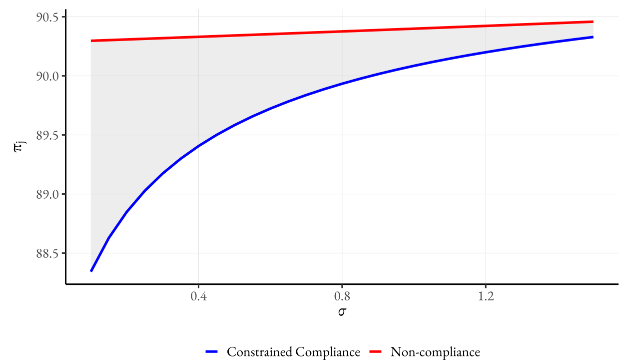
(a) Firm Working Conditions a_j



(b) Firm Wage w_j



(c) Firm Size n_j



(d) Firm Profits π_j

Notes: These figures depict the equilibrium values of w_j, a_j, n_j, and π_j under constrained compliance and non-compliance, plotted as a function of σ. For all σ values shown, profits are higher under non-compliance, making it the optimal strategy. Consequently, the firm will only be in the constrained compliance scenario once $a_{\min}$ is enforced. Parameter values are: $z_j = 70$, $k = 1.1$, $p = 0.5$, $\phi = 1.5$, $a_{\min} = 15$, $\theta_j = 0.3$, and $\{\beta_w, \beta_a\} = \{0.7, 0.3\}$. The shaded areas indicate the change in the variable following enforcement, with light blue representing a positive effect and grey indicating a negative effect.

E Appendix: Alternative mechanisms

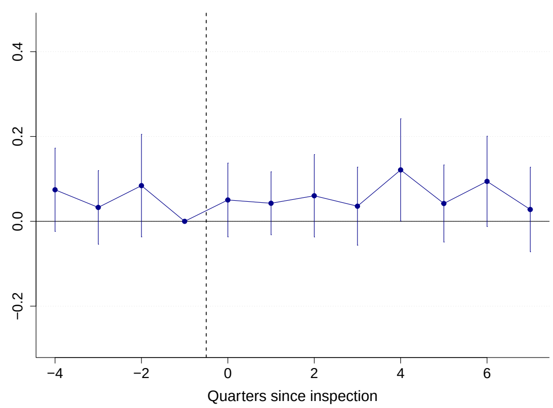
In this section, we consider three main alternative mechanisms that could account for the empirical patterns documented in Section 6, with a particular focus on the observed increase in firm size. We discuss (i) the formalization of previously informal workers, (ii) a mechanical labor increase required for compliance, and (iii) firm misinformation about potential benefits and costs of improving working conditions. We provide empirical evidence suggesting that these alternative explanations are unlikely to fully account for the observed result or operate as complementary channels that reinforce our main mechanism. This main mechanism relies on firms exerting monopsony power over working conditions, leading to an increase in employment following an inspection.

Worker formalization. The formalization of informal workers in formal firms following firm inspections that focus on labor regulations has often been emphasized in the literature (Brotherhood et al., 2024; Samaniego de la Parra and Fernández Bujanda, 2024). We provide four sets of evidence that —in our context of large and productive manufacturing firms— the increase in firm employment following inspections is unlikely to be driven by the formalization of previously informal workers.

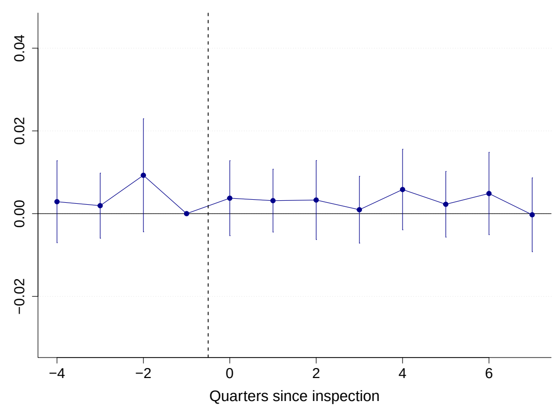
First, the monthly manufacturing survey EMIM is designed to capture information on the total workforce, including both formal and informal workers. Consequently, we should not expect to see an increase in total employment if firms only formalize informal workers.[53] Even if firms were inclined to misreport their informal workers, the firms in our sample are unlikely to have large shares of informal workers. The manufacturing sector has the lowest share of informal workers among all sectors in Mexico (see Figure A.3), and our sample focuses on the largest and most productive manufacturing firms, which are even less likely to employ informal workers.[54]

Second, if the observed increase in total employment was solely due to a shift from informal to formal employment, we would not expect to see an increase in production, reported in Figure 14 by more hours worked, more plant capacity used, increased total material costs, and higher revenue. Moreover, we would expect an increase in average social security contributions per worker. In Figure B.5, we find no significant effect of inspections on social security contributions per worker nor on the ratio of contributions to average wages (at the 5% level).

Figure B.5: Effect on Average Social Security Contributions



(a) Social Sec. Contrib. per Worker



(b) Social Sec. Contrib. over Average Wage

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 7 at the quarterly level for $h \in [-12 : 11]$. The outcome variables measured at the firm level are social security contributions per worker and average social security contributions per worker divided by the average wage. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those not treated for the entire period. Standard errors are clustered at the firm level.

Third, if the employment increase was primarily driven by the formalization of informal workers, we would expect the strongest effects to arise from inspections focused on general labor conditions, which primarily address topics such as worker registration and informality. However, our results in Figure B.6 show the opposite pattern. The most significant effects are observed in inspections related to workplace safety, hygiene, and training.

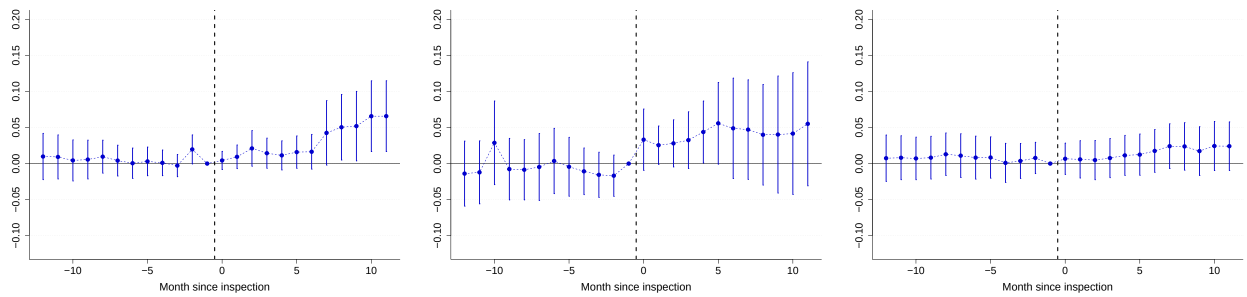
Fourth, while the increase in employment is primarily driven by additional hires, Figure A.16a shows that approximately half of these additional hires are workers previously employed at other firms

[53]The selection of firms included in this survey is highly confidential, as firms report detailed information on outsourcing, profits, the production process, and thus plausibly also on total workforce, including informality.

[54]Empirical evidence also shows that the increase in employment is not driven by changes in outsourcing practices, as we do not find an effect on the share of outsourced workers post-inspection.

registered in the social security system. This suggests that a substantial share of the employment gain reflects reallocation within the formal sector rather than net entry from informal employment or unemployment. Taken together, this evidence suggests that the employment response is not merely a shift from informal to formal employment but is instead likely to be driven by broader improvements in working conditions.

Figure B.6: Effect on Log Production Workers - Heterogeneity by Inspection Topic



(a) Safety and Hygiene (b) Training (c) General labor conditions

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 7 at the monthly level for $h \in [-12 : 11]$. The outcome variable is the total number of blue-collar workers. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020 for different inspection topics: Panel (a) for safety and hygiene, Panel (b) for training, Panel (c) for general labor conditions. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are not treated for the entire period. Standard errors are clustered at the firm level.

Mechanical increase in labor for compliance. One potential alternative explanation for the post-inspection increase in employment is that firms are mechanically required to hire additional personnel to comply with newly enforced regulations—for example, by employing workers specialized in safety or regulatory compliance. To assess the plausibility of this mechanism, we reviewed the complete set of inspection protocols, which detail the specific standards subject to enforcement during inspections.[55] The protocols do not contain explicit directives requiring firms to hire more workers as an automatic consequence of inspection. However, certain provisions may indirectly encourage firms to increase staffing levels to comply with regulations or correct deficiencies. For instance, provision 002-STPS-2010 stipulates that firms classified as having a high risk of fire must create fire response brigades with specific training and simulation requirements. If the existing workforce is insufficient to meet these requirements across all shifts, additional personnel may need to be hired. Similarly, provision 009-STPS-2011 states that if inspections reveal deficiencies in the handling of machinery, hazardous materials, or work at heights, the firm must assign authorized, trained personnel to operate and maintain the machinery. If existing staff are insufficient or unqualified, new hires may be necessary to comply.

Such adjustments apply only to a small subset of norms and would represent an indirect effect of enforcement. It is therefore unlikely that this channel alone accounts for the 4–7% increase in employment documented in Figure 7. Nevertheless, we can interpret this channel within the

[55]The inspection protocols can be found here: `https://www.stps.gob.mx/bp/secciones/conoce/quienes_somos/quienes_somos/inspeccion/ins/protocolos.html`, last accessed 07/2025

framework developed in Section 2. Suppose compliance requires hiring q extra specialized workers for every productive worker at the firm, then a_{min} can be expressed as $q \cdot \tilde{w}$ where $\tilde{w}$ is an exogenous wage paid to the specialized workers. Firm profits under compliance can be expressed as:

$$\Pi_j = z_j n_j - w_j \underbrace{n_j}_{\substack{\text{productive}\\\text{employment}}} - \tilde{w} \underbrace{q \cdot n_j}_{\substack{\text{specialized}\\\text{employment}}} \tag{83}$$

In this setting, total employment can be expressed as $n_j(1+q)$, implying that a mechanical increase in observed employment may stem from an increase in q. As explained in Section C.3, if the firm did not have labor market power over working conditions in this setting, an increase in q would lead to a decrease in n_j, which represents productive employment in this specific scenario. However, as noted in Figure 14, we find evidence of an increase in production driven by an increase in productive workers rather than an increase in productivity per worker, consistent with an increase, rather than a drop, in the number of productive workers. Additionally, Figure A.15 shows that inspections do not cause elevated separation rates. Thus, a mechanical increase in the labor force required for compliance with regulations without a corresponding decline in the number of productive workers is consistent with firms possessing market power over working conditions.

Firm misinformation. An alternative explanation for non-compliance is that firms may not be aware of labor regulations or their associated true costs and benefits. This can be the case if firms have a limited understanding of hiring and firing regulations and overestimate associated costs, as in Bertrand and Crépon (2021). Yet, Figure B.6 shows that the positive employment effect is stronger for inspections related to workplace safety, hygiene, and training, suggesting that this type of misinformation is unlikely to be the primary driver of our results.[56]

It is still possible that non-compliance is influenced by a lack of knowledge of labor regulations. Within our theoretical framework, this corresponds to a firm operating in constrained compliance, under a misperceived minimum mandatory level of benefits $\tilde{a}_{min}$ such that it provides $\tilde{a}_{min} < a_{min}$. However, for misinformed firms to consistently offer poor working conditions and still attract workers, it is necessary to assume some degree of monopsony power over these conditions. The underlying intuition is similar to Dube et al. (2025), who show that monopsonistic employers can mis-optimize wage setting without being driven out of the market, precisely because of their market power. Additionally, for the enforcement of the true a_{min} to lead to an increase in employment in this scenario, it is again necessary to assume that firms possess monopsony power over working conditions.

Alternatively, it is possible that firms misperceive the positive labor supply effects of providing good working conditions. In this scenario, firms forced to improve working conditions, through an inspection, may realize that doing so increases their attractiveness to workers. This mechanism is also consistent with a model of monopsony power, in which firms underestimate the labor supply elasticity with respect to amenities—i.e., they perceive $\tilde{\beta}_a < \beta_a$. As a result, firms provide sub-optimally low levels of non-wage amenities, marking down working conditions more than is optimal. During an inspection, firms may update their beliefs and voluntarily shift from non-compliance to compliance. In conclusion, even if inspections operate primarily by conveying information to firms, the observed increase in employment remains aligned with monopsonistic behavior.

[56] Hiring and firing regulations (including severance pay) are part of inspections on general labor conditions.