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

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Abstract

This paper studies the characteristics of large formal firms violating labor regulations and analyzes the effects of enforcement on both firms and workers. A stylized model of monopsonistic firms—where employers set both wages and working conditions—shows that high levels labor market power can lead to poor working conditions, and enforcement of minimum conditions can raise firm employment through an expansion of labor supply. We leverage administrative records of inspections enforcing labor regulations—such as workplace safety and health, and mandatory training—linked to detailed survey and administrative employer-employee data for large manufacturing firms in Mexico to verify the model predictions. We find that firms are more likely to be found violating if they invest less in worker training, have lower productivity, employ relatively fewer women, and hold a greater labor market share. We show that stratified random 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 causally estimate that these stratified random inspections increase total firm employment by around 4–7% within one year. Average firm wages decrease by less than 2%, driven by compositional changes rather than wage setting. Our results indicate that enforcing labor regulation compliance in large manufacturing firms can be an effective policy tool to improve working conditions, mitigate labor market power, and simultaneously increase firm employment.

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1 Introduction

Labor 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 beyond the limits of the labor law (Ulyssea et al., 2023). Emerging evidence suggests that also large formal firms in these environments frequently fail to meet 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 firm, and the consequences of different 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 non-wage working conditions, such as inadequate safety measures, particularly in environments where the enforcement of minimum standards is weak. In this context, the enforcement of labor 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 labor regulations, and in shaping the effects of regulatory enforcement on large firms and their workers. We study both aspects theoretically and empirically, leveraging microdata on stratified random labor 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 curve 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 (minimum working conditions), which is imperfectly enforced. Firms providing amenities below this mandatory level are considered non-compliant and 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 more inelastic with respect to non-wage amenities, i.e., firms with greater market power over

[1] Policies such as responsible sourcing standards (ILO, 2016), labor provisions in trade agreements (ILAB, 2024), or labor inspections.

amenities, 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 downwards 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 first model prediction by analyzing firm characteristics associated with being found violating labor regulations during inspections. We leverage inspection 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 enables us to control for 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 uni-variate and multivariate regressions. Specifically, firms with lower productivity, lower share of female workforce (a proxy for a workforce with a lower relative valuation of non-wage conditions (Maestas et al., 2018)), higher labor market share and in more concentrated labor markets (proxies for higher labor market power (Azar et al., 2022)) are more likely to be found violating. Low worker training, likely a direct proxy for violation of labor 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 violation.

Third, we test the second prediction of the model by studying the impact of enforcing minimum working conditions via stratified random labor inspections on a rich set of firm and worker characteristics. Our estimation is based on a staggered difference-in-differences design (Dube et al., 2023), comparing first inspected to not yet and never inspected firms within the same randomization group. As a first step, we present multiple forms of evidence consistent with firms increasing compliance with labor regulations following inspections. First, inspected firms increase investment in employee training, a direct and indirect proxy for compliance with labor regulations.[3] Second, inspected firms are less likely to be found violating in subsequent stratified random inspections, indicating lasting improvements in compliance. Third, we find a negative association between inspections and workplace accidents at the sector-municipality level, suggesting that inspections promote adherence to minimum working conditions and improve workplace safety.

We next examine how firms respond to enforcement of labor regulations induced by the inspections. Consistent with the second model prediction, inspections lead to an increase in firm employment by a 4–7%, primarily driven by blue-collar and permanent workers. These effects are more pronounced for firms found in violation during the inspection, likely because these firms are most affected by the enforcement of labor regulations. The increase in firm size is mostly driven by a rise in hires rather than a reduction in separations. The majority of new workers are hired from other formal firms. The average firm wage decreases only modestly by less than 2%), primarily due to compositional workforce changes rather than changes in wage setting. A worker-level analysis for

[2]This mechanism is analogous to the standard monopsony model, where 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).

incumbent and new workers provides a precise null effect of inspections on their respective wages.

Overall, the empirical results align with our theoretical model in which firms use their market power to offer sub-minimum working conditions. When enforcement improves working conditions and wages are downwards-rigid, employment rises through a movement along the labor supply curve (if we consider labor supply as a function of working conditions) which increases overall firm size.[4] Finally, we present suggestive survey evidence that workplace conditions are a relevant factor for workers in Mexico when choosing to quit or accept a new job. In addition, 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 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 explain the magnitude of the observed effects, in particular on employment, or they operate within a framework where firms exert monopsony power over working conditions, thereby reinforcing our main mechanism.

Literature – This paper contributes to three strands of the literature. First, we contribute to 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 focuses on multinational corporations and global supply chains, producing mixed findings. For example, Boudreau (2024) shows that occupational safety and health (OSH) committees among multinational apparel buyers can 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. While this body of work centers on firms integrated into global markets, much of the enforcement literature in low- and middle-income countries instead emphasizes its interaction with informality. For instance, Almeida and Carneiro (2012) show that stricter enforcement can push workers into informality by raising formal employment costs. Brotherhood et al. (2024) present evidence that formal firms caught with informal workers during inspections in Brazil face an increase in formal employment in the short run but a decrease in the long run. Closest to our study, Samaniego de la Parra and Fernández Bujanda (2024) use Mexican firm inspection data to show that enforced labor regulations temporarily increases formalization rates but leads to persistently lower overall formal employment. We contribute to this literature in two ways. First, unlike the research focusing on the enforcement-informality link, our paper focuses on the role of labor market power in explaining violation of labor regulations, and the implications for enforcement. This approach motivates our focus on large firms in the manufacturing sector, where informality is less prevalent and monopsonistic forces are likely to play a more prominent role.[5] Second, we evaluate the effects of labor inspections, a policy tool which can be implemented

[4]This mechanism parallels the effects of a minimum wage on the employment level of a monopsonistic firm.

[5]Manufacturing has the lowest informality rates in Mexico yet remains the primary contributor to workplace accidents (see Appendix Figures A.1 A.3).

at the national level and also targets firms that are not connected to international supply chains. To our knowledge, we provide novel evidence on the causal effects of stratified random firm inspections on a broad range of firm outcomes, including costs, revenues, and profits.

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. Additionally, this literature has expanded to examine 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 labor 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 for the industry by commuting zone level in the U.S. that higher compliance with labor regulations is associated with higher wages, lower labor market concentration and higher union coverage. 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 while holding firm-specific enforcement probabilities constant. Additionally, our study differentiates 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 independently of other economic choices, such as labor supply or production processes. Instead, in our context, workers may react to the level of non-compliance, affecting firm incentives to violate labor regulations.

Third, this paper contributes to the growing literature, which studies monopsonistic power among employers in both developed (Manning, 2004; Berger et al., 2022a) 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.[6] 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, 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 wage-amenity complementarity. Lagos (2024) estimates 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 that fail to account for non-wage attributes may be biased. This literature studies different policies to counter labor market power, such as the minimum wage (Engbom and Moser, 2022; Berger et al., 2022b) and collective bargaining (Lagos, 2024), which

[6] 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.

usually affect several firms, sectors, or geographic areas simultaneously. We contribute to this literature by leveraging exogenous variation in working conditions to single firms, allowing us to isolate firm-specific effects. Additionally, we study the interaction between labor market power and the provision of legally mandated minimum working conditions, including safety and health standards or mandatory training provision. These non-wage amenities have potentially immediate impact on worker safety, especially in hazardous workplaces. Finally, we provide evidence that workplace inspections can serve as an effective policy tool to constrain labor market power, particularly 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 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.

2 Theoretical Framework

In this section, we present a simple theoretical framework to assess the factors influencing firms' decisions to violate labor regulations and how wages and firm employment respond to enforced compliance. In the model, a representative firm j with productivity z_j produces output $f(n_j)$ with total 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, where the latter refers to working conditions. The government imposes 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 expected probability p, it must pay a fine ϕ. Based on the institutional setting in Mexico (described in detail in Section 3) the fine imposed increases with the firms' total employment n_j and the extent of per worker non-compliance $(a_{min} - a_j)$ (STPS, 2014).[7] We assume firms are risk-neutral. Firm profits depend on the decision whether to provide amenities above or below the mandated minimum level a_{min} and are given by:

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

where $p \cdot \phi < k < \phi$.[8] If $a_j < a_{min}$, the firm provides a level of amenities below the mandatory level and is thus violating labor regulations; otherwise, it is compliant. Following (Lagos, 2024), we assume the firm faces the following labor supply curve, which is increasing in both components of total compensation.[9]

[7]We assume linearity in firm size and non-compliance for model tractability.

[8]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 and per amenity fine ϕ.

[9]We micro-found 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.,

$$n^s(w_j, a_j) = \left(w_j^{\beta_w} \cdot a_j^{\beta_a}\right)^{\theta_j} \tag{1}$$

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 labor regulations. *If the firm faces a firm-specific labor supply curve that is upward sloping in amenities ($\beta_a > 0$), the optimal decision on whether to violate labor 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 labor regulations. Second, all else equal, 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 labor regulations.

In Section C.4 we derive the optimal wages and amenities provided by firms in each case. In Section C.5.1 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 labor 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 $\hat{p} = 1$.*

2018).

This shift forces non-compliant firms into constrained compliance.[10] *For the newly complying firms:*
Prediction 2a. *Under no downwards wage rigidity, wages will adjust downwards.*

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

$$\Delta\% w_j = \frac{z_j - k a_{min}}{z_j - p\phi a_{min}} \left(1 + \frac{\beta_a}{\frac{1}{\theta_j} + \beta_w}\right) \tag{2}$$

Which is lower than 1 for all previously non-compliant firms.[11]

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.*

The proportional change in employment resulting from an inspection is characterized by the expression in Equation 3 and may be positive if the reduction in wages is insufficient to offset the increase in the value of non-wage benefits. The intuition is that when the firm-specific labor supply curve 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 curve (see Equation 1), 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).[12]

$$\Delta\% n_j = \left(\underbrace{\frac{a_{min}\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p\phi)}{z_j - p\phi a_{min}}}_{>1} \right)^{\theta_j \beta_a} \left(\underbrace{\Delta\% w_j}_{<1} \right)^{\theta_j \beta_w} \lesseqgtr 1 \tag{3}$$

In Section C.5 we solve the model under alternative scenarios: First, in Section C.5.1 under the assumption that labor supply responds only on wages ($\beta_a = 0$), as in a standard monopsony model (Cardoso et al., 2018). Second, in Section C.5.2, assuming labor 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. Additionally, in Section C.5.3 we show that under the assumption that firms take wages as given and possess only amenity setting power, enforcement can also lead to positive effects on employment. Therefore, observing a zero or positive employment effect from enforcement empirically suggests that firms have labor market power over amenities.

In Section 5 we empirically test Prediction 1 by analyzing the firm level characteristics that correlate with being found violating labor regulations, In Section 6.3 we empirically assess Prediction

[10] By modeling enforcement in this manner, we assume that after receiving an inspection, a firm that was previously not complying remains compliant from the date of the inspection onward.

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

[12] 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.

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 Labor Inspections in Mexico

The Mexican Secretariat of Labor and Social Welfare (STPS) conducts firm inspections to monitor and enforce compliance with labor regulations (STPS, 2014). Most inspections are carried out by state-level branches of the STPS and generally focus on compliance in three main areas: health and safety regulations, training obligations[13], and general working conditions[14].

Labor 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 take place after an initial visit and are intended to verify that identified violations have been corrected and that the employer has complied with any mandated remedial actions within a specified timeframe.

This study focuses on ordinary inspections, where 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).[15] This randomized approach provides a quasi-experimental setting for analyzing the effects of labor 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 noncompliance issues and provide necessary documentation. Extensions are allowed once for an equal period if requested in writing before the deadline, provided worker safety is not compromised. In cases in which the violation implies imminent danger

[13] In Mexico, firms are obliged to provide training to their workers (LFT, 2021).

[14] General working conditions includes issues such as compliance with the minimum wage, maximum working hours, mandatory breaks during the workday, payment of extra hours, compliance with the 13th salary (aguinaldo) and mandatory profit-sharing, compliance with mandatory vacation days (STPS, 2013).

[15] 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. Risk classification is determined by the firm's sector.

for workers, the inspector should require immediate action to correct the violation, or partial or total suspension of activities or restricting 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 fined. 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).[16] 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 labor inspections carried out 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 also 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. Although our main analysis concentrates 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 represent about 23% of all inspections, with first-time ordinary inspections making up 50% of this subset. Inconsistencies found 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,[17] wage bills, production, revenues, and costs. 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 survey captures a threshold level of national revenue—ranging from 60% to 85%, depending on the industry. The survey functions effectively as a monthly census of large, productive manufacturing establishments in Mexico, resulting in a balanced panel of around 9000 observations in our study.[18] We report summary statistics in Appendix Table A.1.

[16] 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.

[17] Total employment measured in this survey includes both formal and informal workers.

[18] 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.

Additional Establishment Surveys: To complement the EMIM, we also 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[19]), and social security coverage.

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, 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).[20] Earnings are bottom-coded at the minimum wage and top-coded at 25 UMA's (Units of Measure and Update).[21] 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.

Accident Data: We also leverage 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 accident reports, with an average of more than 5,000 accidents per month (see Appendix Figure A.2a). 67% 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 labor 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, 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

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

[20] 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.

[21] https://en.www.inegi.org.mx/temas/uma/

knowledge, this is the first study to link administrative data on labor inspections with detailed establishment survey data. To carry out this merge, we first match inspection logs to 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 id used by INEGI (CLEE), which enables the link to the establishment data. We aggregate establishments at the firm-level in each state, our level of observing labor inspections. For the remainder of this paper, the term *firm* in the context of EMIM refers to the firm-state level.[22]

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. We explain the lower matching rate with DENUE than with DNE with inspections being sampled from the DNE, which includes some establishments that may 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. In Table B.1, we 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.

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	0.23	0.23
Manuf. Survey EMIM	9 109	4 853	2 558	23 788	0.26	0.21
Manuf. Admin IMSS	8 930	3 280	1 598	13 594	0.24	0.22

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). Inspection data cover the years 2017-2022. The economic census includes all economic units active in 2018. The manufacturing survey includes all establishments active at some point between 2017 and 2022.

[22]We derive similar results when using establishment-level data or restricting the sample to firms with a single establishment per state (Appendix Figure A.12).

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.[23] Appendix Figure A.5 displays the distribution of inspection likelihood across randomization groups between 2018 to 2021 for all firms in the Economic Census.[24] 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 some subsectors within the manufacturing sector exhibit particularly high inspection rates, though there is significant variation in inspection probabilities within manufacturing. 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 higher 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 regression:

$$y_{ig} = \beta \cdot inspection_{it} + \phi_{xt} + \epsilon_{igt},$$

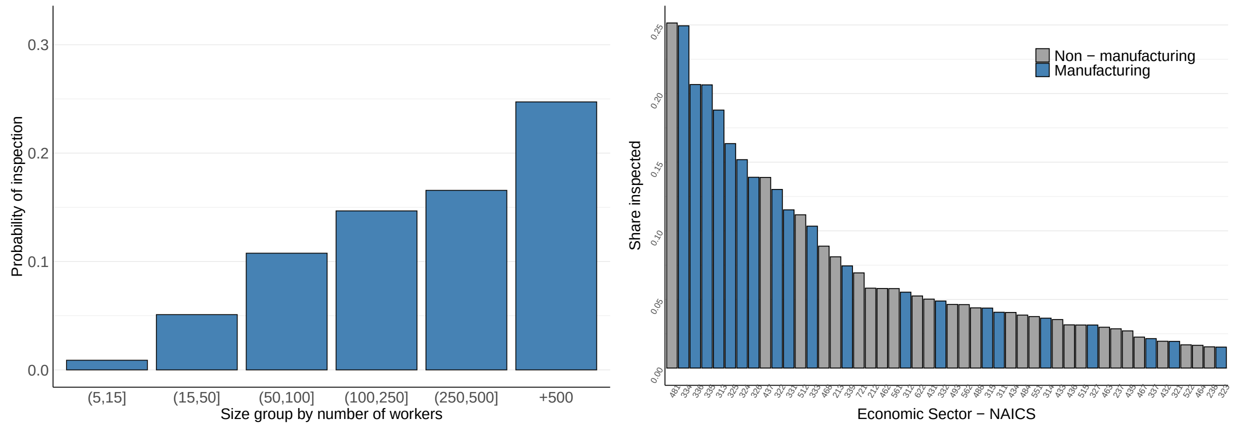
where y_{ig} corresponds to a pre-determined characteristic of firm i in group g (where the group is defined in month $t-1$), and $inspection_{it}$ is an indicator variable equal to 1 if the firm receives an ordinary inspection at date t (month-year). We carry out two different specifications, where ϕ_{xt} is either a state-by-date fixed effect, or a stratified randomization group-by-date fixed effect. We compare the coefficient β in both specifications to assess the validity of the randomization process. This test is comparable to a means test in a classic randomized controlled trial but accounts for the staggered treatment in our setting.[25]

[23] We calculate the probability of inspection in a group as the likelihood of being inspected at least once $= 1 - \frac{N_g - 1}{N_g}^{n\ inspections_g}$. Where N_g is the number of firms in the randomization group and $n\ inspections_g$ is the number of inspections carried out to firms in the randomization group. This accounts for being inspected more than once.

[24] The estimated propensity score is strongly correlated across time. The correlation between $\hat{p}$ in 2018 and 2017 is 0.76, and 2018 and 2019 is 0.6.

[25] This test of stratified randomization is stronger than conditional parallel trends, our necessary assumption in the

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 different 3-digit NAIC Economic Sectors. Figures where built using 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.[26] 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.

causal analysis of Section 6.3.

[26]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 hs 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 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 firm level. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

5 Characteristics of Violating Firms

In this section, we examine manufacturing firms found to be non-compliant with labor 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. We then test Prediction 1 from Section 2 relating non-compliance to productivity, labor market power, worker preferences, and inspection probability. We first take a structured approach by examining whether the empirical counterparts to the variables in Prediction 1 are correlated with observed violations. We then perform 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 1 out of every 4 (ordinary) inspected firms in the manufacturing survey were 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. Next, we examine the correlation between firm characteristics and violations. This 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 relationship between firm characteristics and violation rates (Heckman, 1979). Second, as shown in Section 2 a firm's decision to violate labor regulations depends not only on its own economic characteristics such as productivity and labor market power, but also directly on its (expected) inspection probability. Consequently, when analyzing the determinants of non-compliance, it is important to account for the fact that certain characteristics can affect firm's decision to violate directly and indirectly via the probability of inspection.[27] 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.

We correlate a rich set of given firm characteristics from the Economic Census 2018 with the probability of being found violating during an ordinary inspection conducted subsequent to the Census. The Economics Census 2018 provides three key advantages over EMIM and IMSS for this analysis. First, it covers the universe of formal and informal establishments, allowing for a better measurement of local labor market characteristics. Second, it provides a much richer set of firm characteristics. Third, it allows for mild restrictions, allowing us to focus on the manufacturing sector but all relatively large establishments above firm size 20. We report the results of univariate regressions in Appendix Table A.6, where the dependent variable is an indicator for whether the firm was found violating labor regulations and the independent variable is a firm characteristic. We include specifications with and without controlling for the estimated probability of inspection. We categorize each explanatory variable depending on whether it is related to a firm-specific characteristic, to the firm's workforce, or to the firm's local labor market, defined as municipality by 2-digit NAICS sector cells. We find that firms providing less workers with training 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 working conditions. 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 noncompliance. Furthermore, firms that rely more 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 (Estefan et al., 2024).[28] This also relates to Prediction 1 that firms with higher labor market power are more likely to be non-compliant with labor 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., 2018), interpreted as a higher $\frac{\beta_a}{\beta_w}$ in our model. 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 higher wages (Sorkin, 2018; Bell, 2023). While it is not a primary prediction of our model, this relationship indicates some complementarity between wages and bad working conditions, as monopsonistic firms offering lower wages are also more likely to violate labor 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 labor regulations. These results align with the findings in Marinescu et al. (2021) who show that local industries with higher violation rates pay less wages and exhibit more labor market concentration. The results also align with our theoretical predictions in Section 2 suggesting that firms with greater labor market power

[27]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 less inspections, prompting firms to violate due to low enforcement.

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

are more likely to violate labor 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, along with a positive correlation with average hours worked. Columns 6 and 7 confirm that the results hold when restricting the outcome to firms that were both found violating and subsequently sanctioned.

We do not find a statistically significant direct effect of inspection probability on the likelihood of a firm being found in violation of labor regulations. Our measure of inspection probability is derived from the number of realized inspections per randomization group over the past year. This 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 labor regulations.

Given the large set of potentially correlated firm-level characteristics, we adopt 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 such variables that serve as proxies for the characteristics identified in Prediction Prediction 1 as determinants of labor 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.6 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.[29] All regressions use heteroskedasticity-robust standard errors, following Belloni et al. (2014).[30] The coefficients from this multivariate OLS regression can be seen in Figure 2b. In line with our results from Appendix Table A.6 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 be found violating labor regulations. The coefficients related to worker training become insignificant in multivariate regressions, further suggesting that low worker training is a direct proxy for bad working conditions, likely to be strongly correlated with other firm characteristics that predict violations. While the effect of higher wages is mostly not significantly

[29]The OLS post-Lasso estimator performs at least as well as Lasso in terms of the rate of convergence, while offering the advantage of a smaller bias (Belloni et al., 2010).

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

associated with violations, this coefficient turns significantly negative in multivariate regressions for severe violations including a fine. Neither profits nor value added per worker, two proxies for firm productivity, survive the lasso regularization. This may be explained by the effect of wages, which positively correlate with 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 a violation (blue) or a violation with a fine (sanction) (red). The explanatory variables is a subset of firm characteristics from the 2018 Economic Census. Coefficients are estimated on sample of manufacturing firms with over 20 workers which receive 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 regressions control for estimated probability of inspection. All explanatory variables are standardized such that the mean is 0 and standard deviation is 1.

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

6 Effect of Labor 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 on the firm's group in the month before inspection (g_{t-1}). We apply a local projection difference-in-differences approach (LP-DID) (Dube et al., 2023), which allows us to control for the randomization group in the pre-treatment month.[31]

[31] As mentioned in Dube et al. (2023), A distinctive feature of the LP-DiD approach is that it allows to control for pre-treatment values of time-varying covariates. This is exactly the case for randomization groups, as for instance years since last inspection and age can change over time.

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

$$y_{i,t+h} - y_{i,t-1} = \beta^h \Delta D_{it} + \gamma^h_{gt-1} + \epsilon^h_{it}, \tag{4}$$

where $y_{i,t+h}$ is the outcome of firm i at month x 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 $t+h$. γ^h_{gt-1} is a randomization group x date fixed effect.[32] Following (Dube et al., 2023), 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. (2023), section 4.1.). We cluster standard errors at the firm-level. We apply this LP-DID approach to the monthly manufacturing sample (EMIM) and the comparable monthly administrative employer-employee data (IMSS).

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

6.2 Effect of Labor Inspections on Compliance

The Mexican Labor and Social Security Secretariat (STPS) conducts labor 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 labor regulations is generally challenging. Much of the existing literature has relied on survey-based estimates of informality—a specific form of noncompliance—and a few studies focus on field experiments involving small samples of firms. While we cannot measure compliance directly, we present four types of evidence that firms increase compliance after being inspected.

First, we analyze follow-up inspections, which are a specific type of inspection aimed to check compliance with regulations after a violation were previously found. Approximately 40% of inspections that resulted in a violation of labor standards and provided firms with additional time to fix their insufficient working conditions are followed-up by such an inspection[34]. 82% of these follow-up

[32] 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.

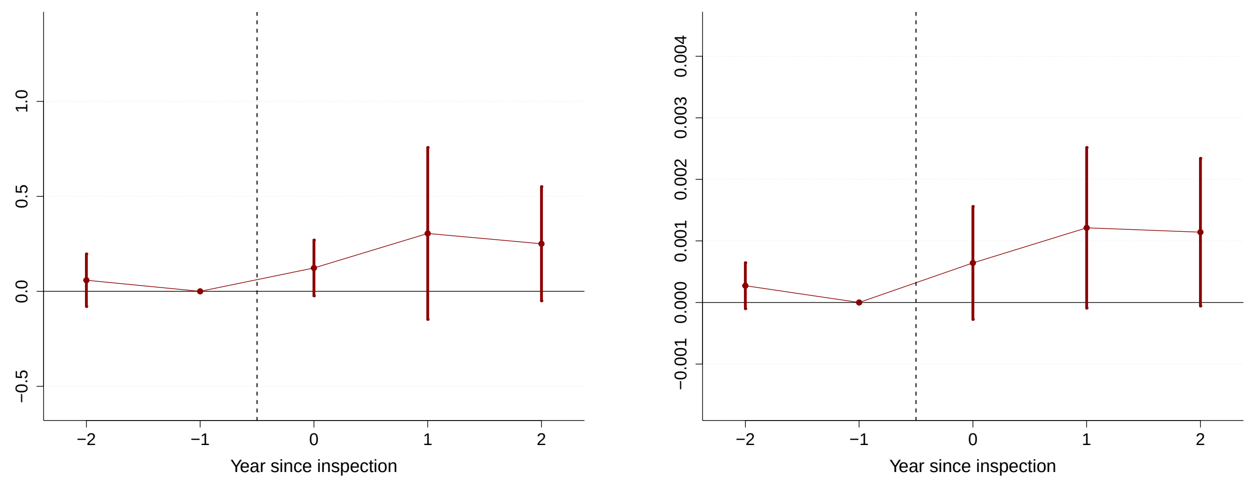
[33] We find similar but noisier results for considering only 2018-2019 in IMSS. EMIM results are robust to considering first ordinary inspections from 2018-2020.

[34] In contrast, only around 6% of inspections where no violation is found are followed by a follow-up inspection

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.

Second, we assess the impact of inspections on worker training expenses, a direct and indirect proxy for compliance with labor regulations. It is a direct measure as firms in Mexico are obliged by the Federal Labor Law (LFT, 2021) to provide annual training to their workers, and certain labor inspections specifically check for compliance with this mandate. It is also an indirect proxy for compliance with labor regulations as better trained workers should be more able to securely operate machines in the manufacturing sector. We observe worker training expenses on the yearly level and apply our main empirical approach 4 on the yearly-level. Figure 3 depicts the effect of labor 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 is in line with firms improving working conditions rather than just learning how to conceal bad working conditions.

Figure 3: Effect of Inspection on Training



(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 4 at the yearly level for $h \in [-2 : 2]$. The outcome variables are training costs per worker and training costs over 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.

Third, We find that stratified random inspections are linked to fewer accidents at the municipality level. While firm-level accident data is unavailable, we use monthly-municipality-level records on accident counts, as well as accident types or sectors. Out of 2,466 municipalities, we retain 1,588 with at least one recorded accident, of which 1,003 had at least one inspection between January 2017 and June 2022. Similar to our main specification 4, we apply a local projections difference-in-differences approach by Dube et al. (2023).[35] We estimate separate regressions for each month since

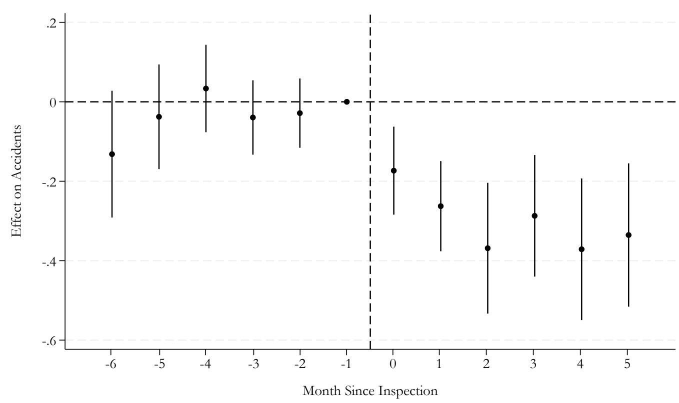
[35] Compared to our firm-level-analysis, we cannot control for the inspection probability via the randomization groups.

inspection $h \in [-6, -2] \cup [0 : 5]$

$$WorkAccidents_{m,t+h} - WorkAccidents_{m,t-1} = \beta^h RandomInspections_{m,t} + \gamma_t^h + \delta_m^h + \epsilon_{it}^h, \quad (5)$$

where γ_t^h and δ_m^h capture monthly and municipality fixed effects, respectively. β^h can be interpreted as the effect of an inspection in t on the the number of accidents in $t+h$. The regressions are based on a balanced sample, considering ordinary inspections from January 2018 until June 2019.[36] We keep only clean controls, i.e. municipality-month observations without additional inspections until $t+5$.[37] 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 in comparison to the pre-inspection month. The effect is statistically significant at the 5% level for all post-inspection periods, while pre-trends are not significantly different from 0. The effect size gradually decreases during the first 3 months after an inspections and stays constant afterwards. In Appendix A.9, we show that our results are robust to different changes in treatment, outcome, or considered time period.

Figure 4: Effect of Inspections on Work Accidents



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 5 at the monthly level for $h \in [-6 : 5]$. The outcome variable is number of work accidents on 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.

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

$$Violation_{itT} = \sum_{t=1}^{t=T} \beta_{tT} D_{itT}$$

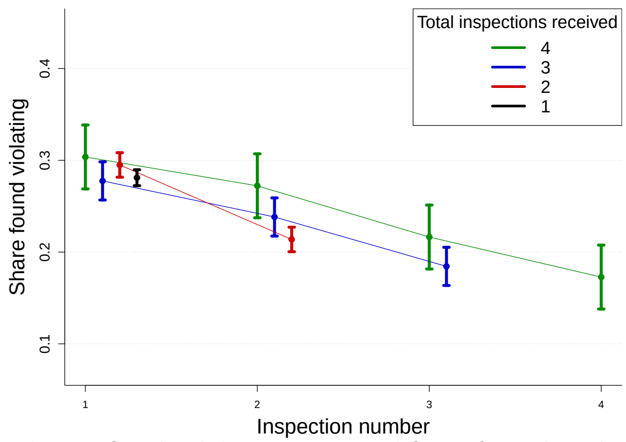
for each group of total inspection $T \in [1, 4]$. $Violation_{itT}$ is a binary variable equal to 1 if firm i, which in our data receives T random inspections in total, is found violating during inspection number t. Thus, the coefficient β_{tT} corresponds to the average violation rate during inspection t of firms

[36] In contrast to our main analysis, we do not consider inspections after June 2019 as we would otherwise include accidents during covid.

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

with total inspections T. Standard errors are clustered at the firm-level. The results are presented in Figure 5, which shows the proportion of firms with a violation in the firm's 1st, 2nd, 3rd, and 4th inspection (t). We plot these proportions separately for firms that have received a total of 1, 2, 3, or 4 inspections (T). Thus, the sample to estimate each coefficient stays the same within each group of total inspections, represented by different colors. 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. The figure suggests that a firm's probability to be found violating labor regulations decreases in the number of inspections they have already received.[38]

Figure 5: Share of Firms Violating, by Inspection Number



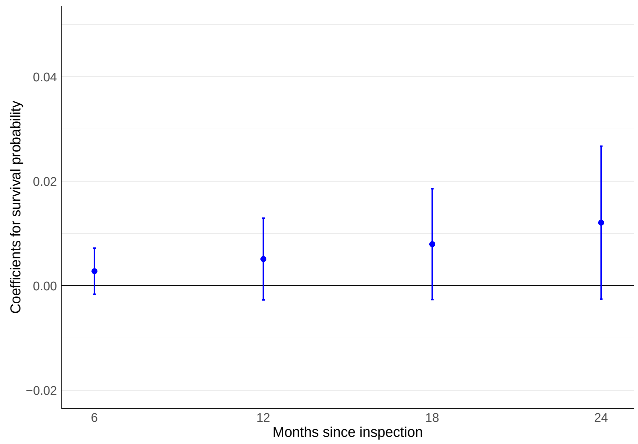
Notes: This figure is constructed using firm level data on inspected firms, from the subsample of inspections merged to DENUE. It shows the coefficients and 95% confidence intervals from regressing a binary variable indicating violation during inspection t on a variable indicating how many ordinary inspections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

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 labor 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.[39] These results corroborate our earlier institutional analysis, indicating that inspections aim to promote compliance rather than generate revenue through penalties or induce firm closures.

[38] In comparison, we report in Appendix Figure A.10 that the firm's probability to be found violating labor regulations does not decrease in the number of extraordinary inspections.

[39] It is important to note that our sample comprises large manufacturing firms with substantial workforces and that results may differ for other firm categories.

Figure 6: Effect of Inspection on Firm Survival



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 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 sample of firms from EMIM which have not yet exited until time t. Standard errors are clustered at the firm level.

6.3 Effect of Inspections on Firm Outcomes

6.3.1 Employment

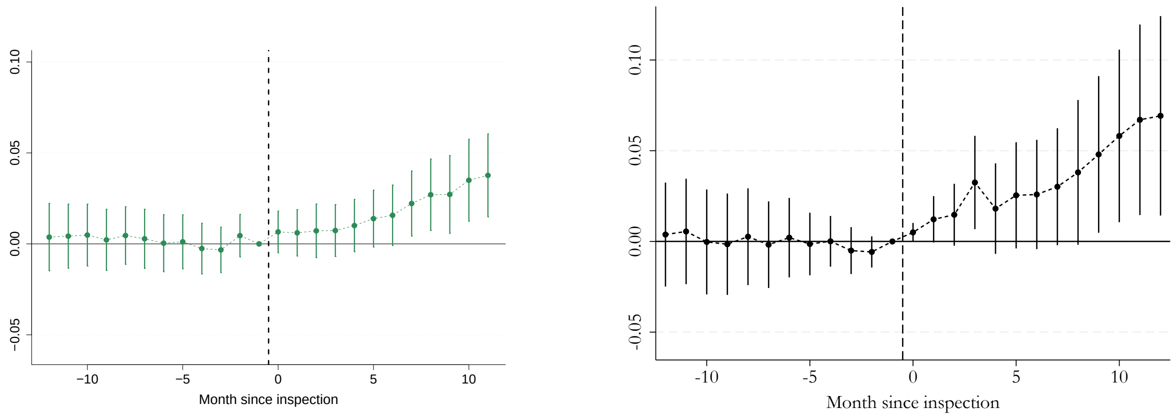
Given our earlier findings that inspections tend to improve compliance, we now turn to evaluating Prediction 2 from Section 2 by estimating the impact of inspections on firm-level outcomes, beginning with total employment. Figure 7 displays the estimated coefficients from a local projection difference-in-differences (LP-DID) model for log total employment, over a 12-month pre- and post-inspection horizon. Panel (a) shows that in the EMIM sample, inspections are associated with a statistically significant increase in firm employment of approximately 4% one year after the inspection.[40] We observe qualitatively similar results in the IMSS data in Panel (b), where firm employment rises by about 7% one year following an inspection.[41] Appendix Figure A.11 demonstrates that this positive effect on employment persists in subsequent years post inspection.

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, consistent with the focus of labor regulation enforcement on areas such as safety, health, training, and general working conditions, 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, rather than temporary ones.

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

[41] This corresponds to an increase of approximately 42 workers, given an average firm size of 600 workers prior to the inspection.

Figure 7: Effect of Inspection on Employment

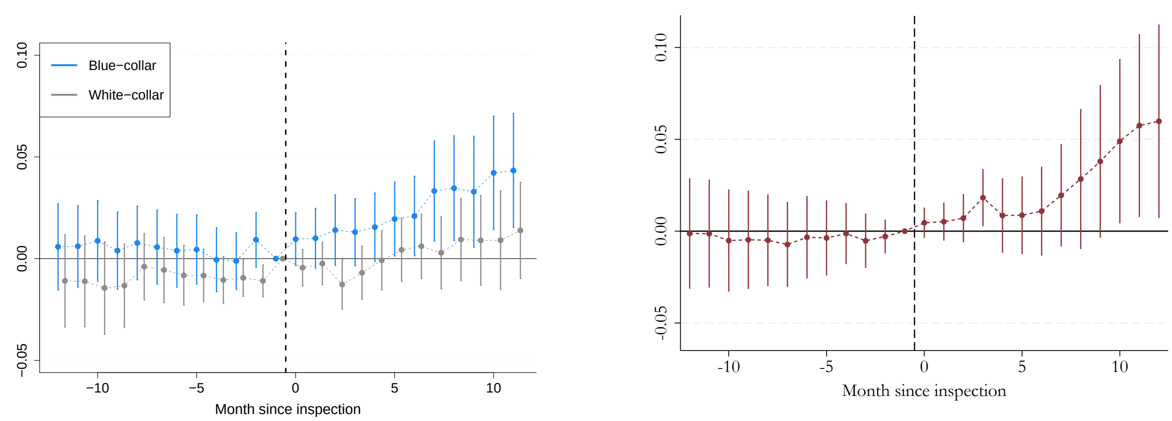


(a) Log Firm Employment (Monthly Survey)

(b) Log Firm Employment (Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 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 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 untreated for the whole period. Standard errors are clustered at the firm level.

Figure 8: Effect on Type of Employment



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

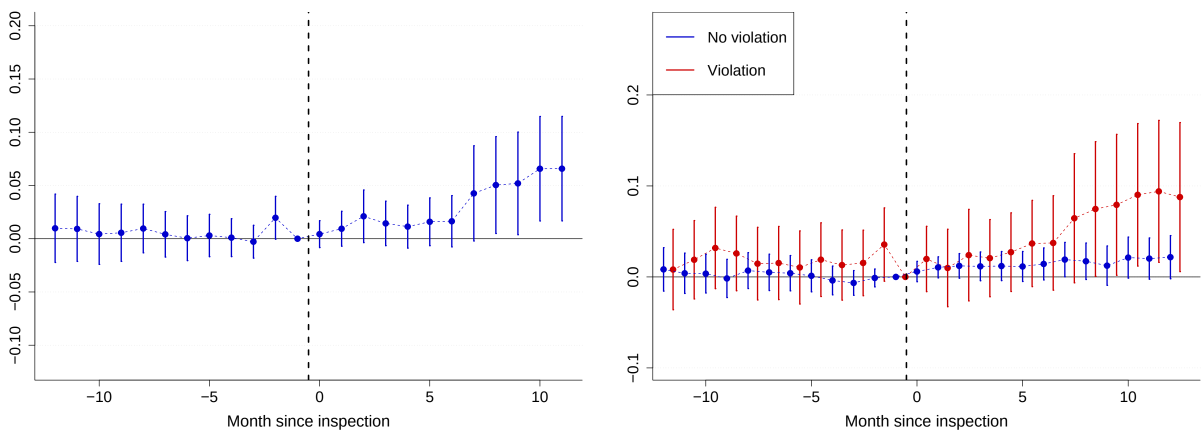
(b) Log Permanent Workers (Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variables are total blue-collar workers and total white-collars workers at the firm in EMIM. In panel (b) the outcome variable is total 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 in IMSS. 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 untreated for the whole 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.3. 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. We also estimate the effects of inspections on the number of blue-collar workers separately for firms found to be in violation of labor regulations and for those that are not. As shown in Figure 9b, the positive employment effects are concentrated among non-compliant firms, which likely undertake regulation-related adjustments in response to inspection findings. These results are consistent with our earlier 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.12 demonstrates that these results are robust to several alternative specifications: 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 are in line 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



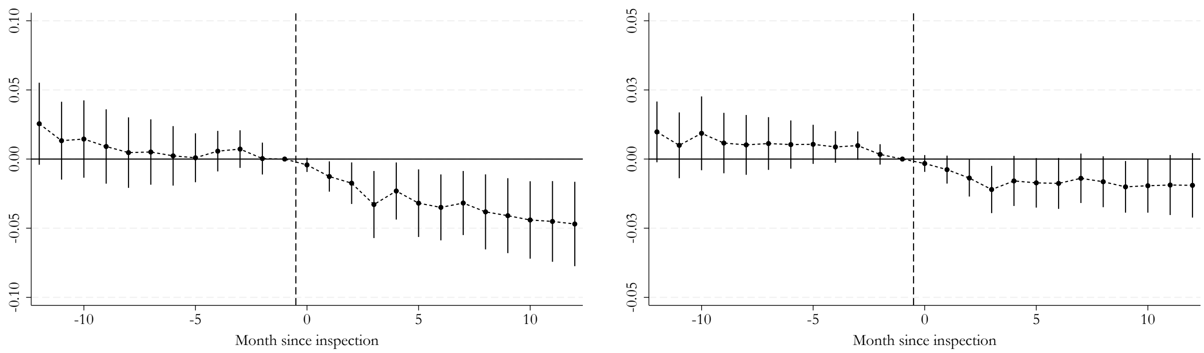
(a) Safety and Health Inspections

(b) Log Blue-collar Workers - Violating vs Not

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variables is total blue-collar employment for safety and health inspections in EMIM. In panel (b) the outcome variable is blue-collar workers, and we divide the treated firms into those that were found violating during the inspection and those that weren't 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 untreated for the whole 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 1% and in labor market experience by around 5%. 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.19), 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 10: Effect on Workforce Experience and Tenure



(a) Log Average LM Experience

(b) Log Average Firm Tenure

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variables is average formal labor market experience. In panel (b) the outcome variable is average firm tenure. 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.

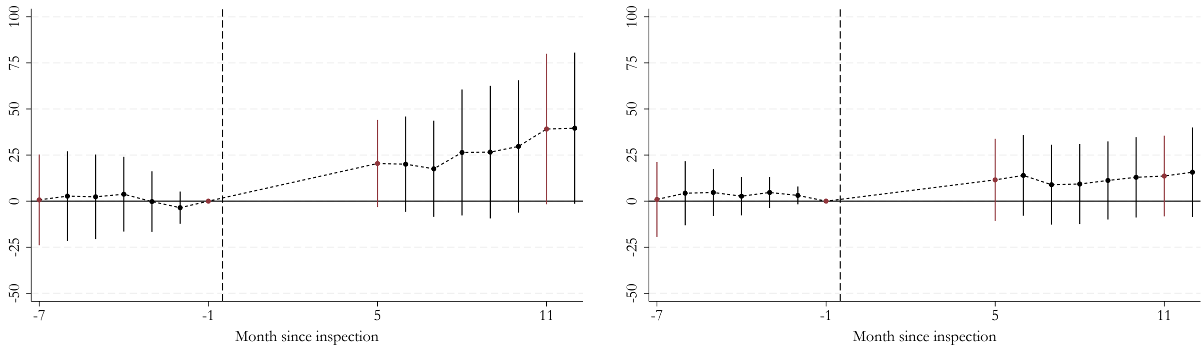
We present the estimated effects of first-time ordinary inspections on cumulative hires and separations in Figure 11a and Figure 11b, respectively.[42] While we cannot fully rule out that inspections caused a change in separations, this evidence suggests that a rise in hires helps explain the post-inspection increase in firm employment.[43] In Appendix Figure A.13, 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 separation rate remains close to zero and statistically insignificant.

Appendix Figure A.14 shows that this increase in hires is driven by both firm-to-firm moves and by individuals who were not registered in the IMSS system in the prior month (e.g., unemployed or out of the labor force) but who had 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 D, these results suggest that the increase in firm employment is unlikely to be driven by formalization alone.

[42] 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.

[43] 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.

Figure 11: Effect on Hires and Separations



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

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

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variables is sum of hires in past 6 months. In panel (b) the outcome variable is sum of separations in 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 untreated for the whole period. Standard errors are clustered at the firm level.

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. Since 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. Additionally, 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 following an exogenous 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.

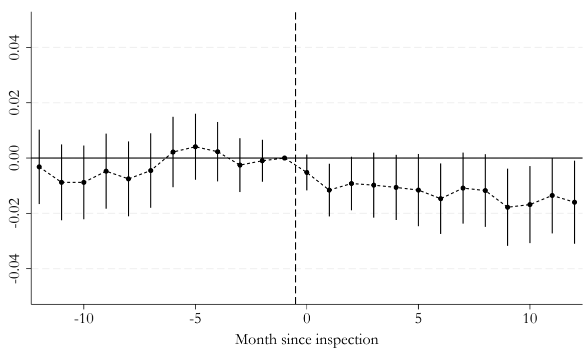
Consistent with an increase in firm employment, we observe a mechanical increase in total labor costs of around 3% one year after an inspection using the monthly survey EMIM in Figure A.15.[44]

[44] Survey data form EMIM include benefits in total labor costs such as social security and profit-sharing on a monthly-level. We obtain similar positive effects on total labor costs using both yearly and quarterly estimation windows, as shown in Appendix Figure A.16.

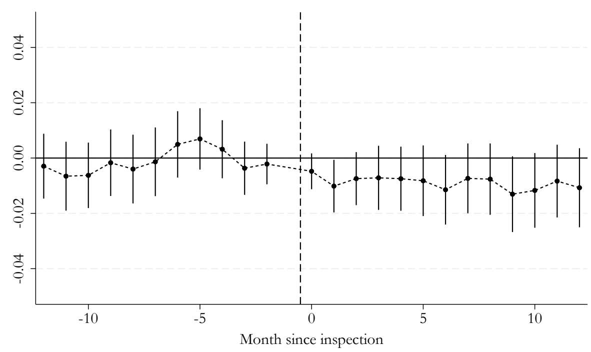
However, we do not observe a significant change in the average labor costs per worker. This result may reflect two key factors: (i) labor benefits typically constitute a relatively small share of total firm costs and are measured with more noise than variables such as total employment; and (ii) changes in workforce composition following inspections—specifically, an increase in the share of blue-collar workers—may affect labor costs, depending on how their wages and benefits compare to the firm average. To further explore this, we separately estimate the effect of inspections on average labor costs for blue- and white-collar workers. As shown in Appendix Figure A.17, we find no statistically significant effects for either group.

To gain additional insights into wage dynamics, we turn to administrative employer-employee data from IMSS, which offers more precise measures of base wages and allows us to distinguish between incumbent and newly hired workers. Figure 12a presents evidence of a statistically significant decline in the average firm wage—at the 5% level—one year after an inspection. The estimated effect is less than -2% , with a lower bound of approximately -3%.[45]

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 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variables is average firm wage. In panel (b) the outcome variable is average firm wage, controlling for average workforce year of entry into IMSS, log number of men, age, tenure, 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 untreated for the whole period. Standard errors are clustered at the firm level.

As previously discussed, this decline is likely driven by compositional changes in the workforce. While IMSS does not include explicit indicators for worker type (e.g., blue- vs. white-collar), we 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.[46] 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.

[45] We find similar effects for median firm wage and report the effect on different wages percentiles in Appendix Figure A.18.

[46] We report the effect on average firm age and year of worker registration in IMSS in Appendix Figure A.19.

We can further disentangle composition and wage setting effects by assessing the effect of inspections on wages for new and incumbent workers, respectively. First, we carry out a worker level regression using a balanced panel of approximately 800,000 workers continuously employed at the same firm (treated or control) for at least 24 consecutive months. Similar to our firm-level analysis, we estimate the effect of first-ordinary inspections on the worker-level through separate regressions for $h \in [-12 : -2] \cup [0 : 11]$:

$$y_{i,j,t+h} - y_{i,j,t-1} = \beta^h \Delta D_{jt} + \gamma^h_{gt-1} + \epsilon^h_{ijt}, \tag{6}$$

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 with full severance benefits (LFT, 2021).

Second, we estimate the effect of first ordinary inspection on new hires' wages. We defined treated workers as those who switch from a not yet or never inspected firm to and already inspected firm. Control hires are those switching between not yet or never inspected firm. We restrict treated and control workers to be employed in the firm 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 not yet enforced better working conditions to one with potentially enforced better working conditions affects workers' wages. We estimate

$$Y_{jt} = \alpha_j + \phi_y + \theta \text{Treat}_j \cdot \text{Post}_t + \epsilon_{jt}, \tag{7}$$

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 for individual (α_j) and year of hire (ϕ_y) fixed effects.[47] Figure 13b shows no negative effect on workers' wages when being hired by an already inspected to a not yet or never inspected firm. We further do not observe differential pre-trends, indicating that workers have parallel wage growth before being hired by another firm. This suggests that firms do not reduce wages when hiring into improved working conditions.[48] Given the null wage results for 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).

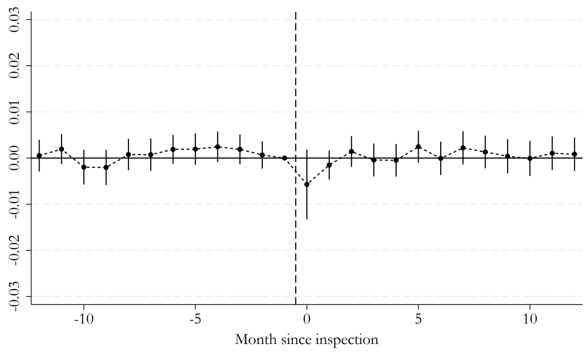
Overall, we observe a mild decrease in the average firm wage, which is likely driven by compositional changes rather than active wage setting. The absence of downward wage adjustments despite improvements in working conditions can be attributed to downward nominal wage rigidity in the

[47] The results are robust to including "origin" firm randomization group x date fixed effects prior to the job change.

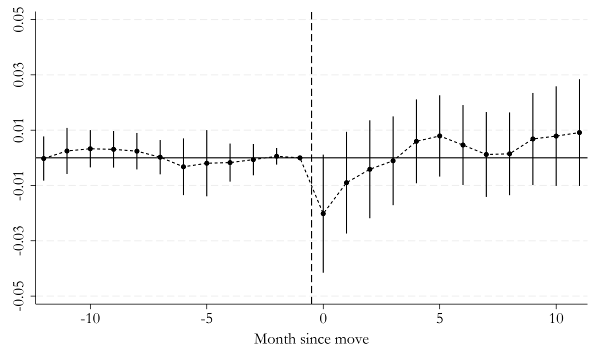
[48] We report basic difference-in-differences graph including parallel pre-trends for new workers in Appendix Figure A.20.

given institutional setting. Notably there 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.23 presents descriptive month-to-month wage comparisons, showing that nominal wages almost never decrease.

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 on first ordinary inspections on wage changes for incumbent and new workers, respectively. Panel (a) reports β^h and 95% confidence intervals from estimating Equation 6 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 variables is 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 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 7. 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.

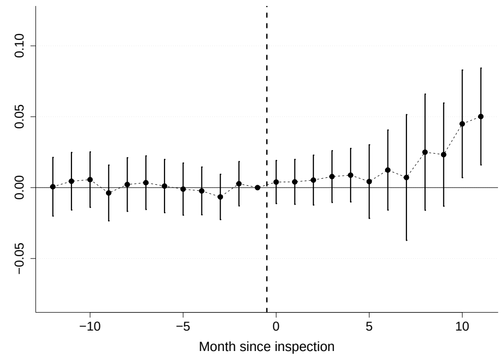
6.3.3 Production Process, Revenues, and Profits

The increase in firm employment is accompanied by significant rises in total hours worked, plant capacity utilization, total material costs, and total revenue as reported in Figure 14. These real effects on the firm's production process may partly reflect a mechanical consequence of higher employment levels. However, 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 Section D.

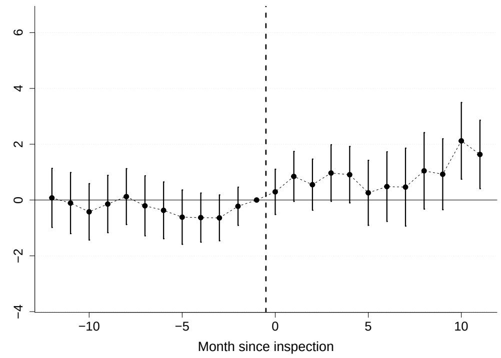
We find no significant change in revenue per worker, as shown in Appendix Figure A.21. While firm profits over value added tend to decline, the estimates are not statistically distinguishable from zero at the 95% confidence level, reported in Appendix Figure A.22.[49] The estimates indicate a negative, though noisy and statistically insignificant, effect. This 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 A.19a), potentially leading to a modest decline in firm profitability.

[49] We use profits over value added as the outcome variable, following Nimier-David et al. (2023).

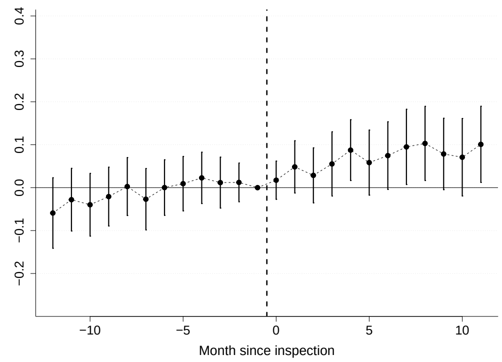
Figure 14: Effect on Production Process and Revenues



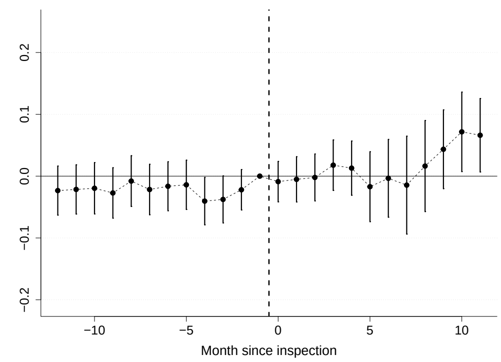
(a) Log Total Hours Worked



(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 4 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 untreated for the whole period. Standard errors are clustered at the firm level.

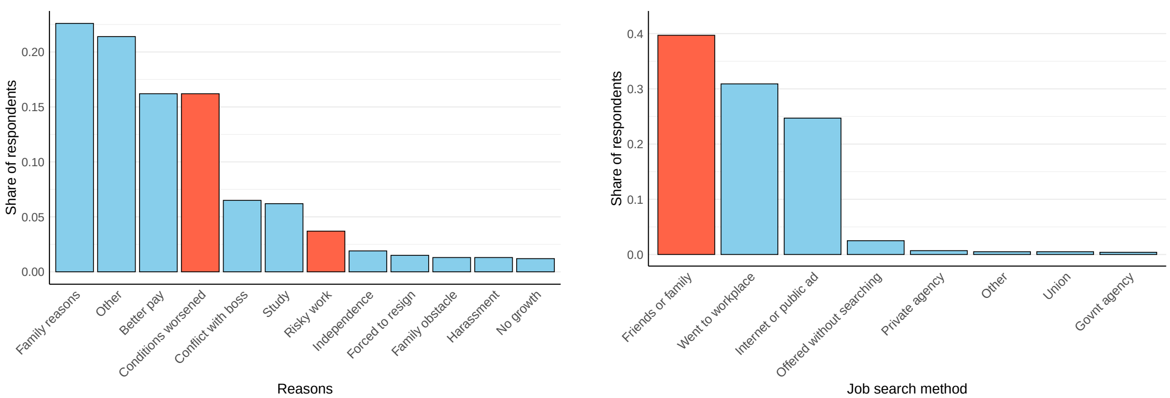
7 Mechanisms

Working conditions and labor supply. The results in Sections 5 and 6.3 are consistent with a monopsonistic labor market, where labor supply depends positively on both wages and non-wage labor conditions. In such settings, employers facing a firm-specific labor supply curve can maximize profits by offerings substandard working conditions without perfectly offsetting them with higher wages, unlike in a perfectly competitive labor market. When better working conditions are mandated without corresponding wage reductions, the effective labor supply curve shifts outward. 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 know about a firm's working conditions when deciding to work for a firm, implying that the increase in firm size driven by an increases 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. Panel (a) of Figure 15 illustrates the distribution of these responses for the 2018–2020 period.[50] 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 = 3681). As shown, poor working conditions are a key reason for quits, alongside family considerations and higher pay. Hazardous work environments—directly aligned with our empirical findings—also play a significant role, with 3% of workers explicitly citing workplace risks as their main reason for quitting.

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 and whose previous job was at a formal firm in the private sector (N = 3681). 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 using responses from the first trimester of years 2018-2020.

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.[51] 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.

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

[51]The sample for this question is restricted to workers employed in privately-owned manufacturing firms with more than 20 employees (N = 46 779).

To further explore the salience of working conditions in workers' job selection, we conducted a complementary survey on Prolific with a sample of 134 Mexican workers. Respondents were asked to indicate the job characteristics they considered when choosing their current employment. Table A.5 presents summary statistics of these responses, disaggregated by income group. Table A.5 reports summary statistics, disaggregated by income group. Approximately 10% of respondents cited working conditions as a salient factor in their employment choice, while nearly 40% highlighted training opportunities. Importantly, the emphasis on training was especially pronounced among lower-income workers (earning less than 10,000 MXN, or approximately 500 USD per month)

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

Alternative mechanisms In Appendix D, 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 explain the magnitude of the observed effects, in particular on employment, or they operate within a framework where firms exert monopsony power over working conditions, thereby reinforcing our main mechanism.

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 workers. 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—for example, overestimating the costs associated with dismissal regulations. However, for enforcement to induce an increase in employment under such circumstances, firms must still face an upward-sloping labor supply curve with respect to working conditions. Hence, even this mechanism relies on the presence of monopsonistic features in the labor market.

8 Conclusion

Ensuring compliance with minimum working conditions—such as safety, health, training, and general labor standards—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 labor regulations, and shapes the consequences of enforcement for firms and workers. We study this question combining stratified random inspection records with rich administrative and survey data covering large manufacturing firms in Mexico. We find that firms which 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 regulations during inspections. We show that enforcement via these inspections increases compliance, proxied by greater investment in training, reductions in workplace accidents, and a lower probability of future violations. An inspection causally leads to a 4–7% increase in firm-level employment within one year. Average firm wages decrease only modestly (less than 2%), 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 sub-minimum working conditions. When enforcement improves working conditions and wages are rigid, employment rises through labor supply responses, as workers value improved working conditions.

These results have important implications especially in contexts with limited enforcement capacity. The results imply that enforcement of minimum working conditions in large formal firms can serve as a policy to mitigate labor market power, raise compliance, and increase firm employment. These results also have important implications for the optimal design of inspection targeting policies, as governments may aim not only to maximize detection rates but also to prioritize inspections where enforcement is likely to yield greater positive impact not only on worker well being but also on efficiency. This represents a promising direction for future research. Moreover, we remain agnostic about the underlying determinants of labor market power over working conditions—whether they stem for example from heterogeneous worker preferences or differential salience of job attributes. This remains another interesting avenue for research. A deeper understanding of what drives labor market power can shed light on the effects of enforcement across different settings and inform the design of complementary policies to curb market power in the labor market.

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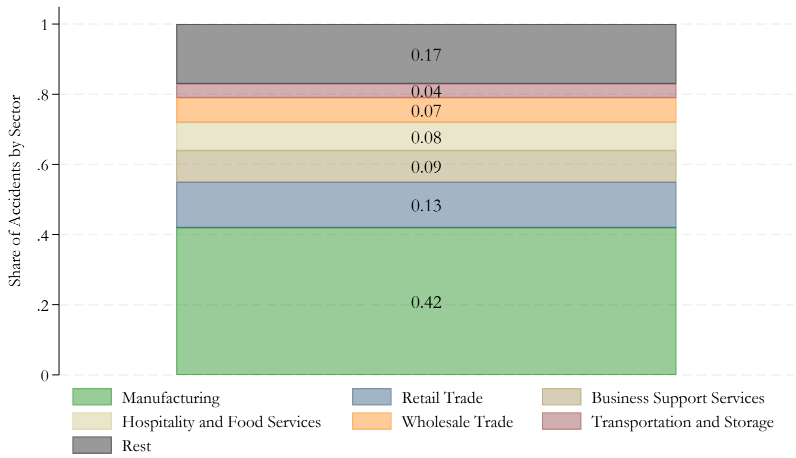
A Appendix: Additional Tables and Figures

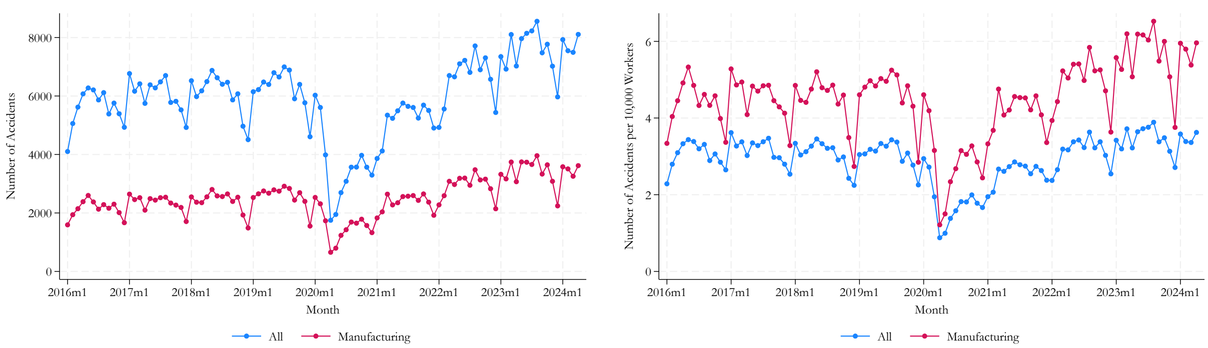
A.1 Figures Appendix

Figure A.1: Share of Accidents by Sector



Notes: This Figure shows the share of total accidents by sector. The Figure was built using all accident data from January 2016 until April 2024. 15 sectors with the smallest shares were aggregated as *Rest*.

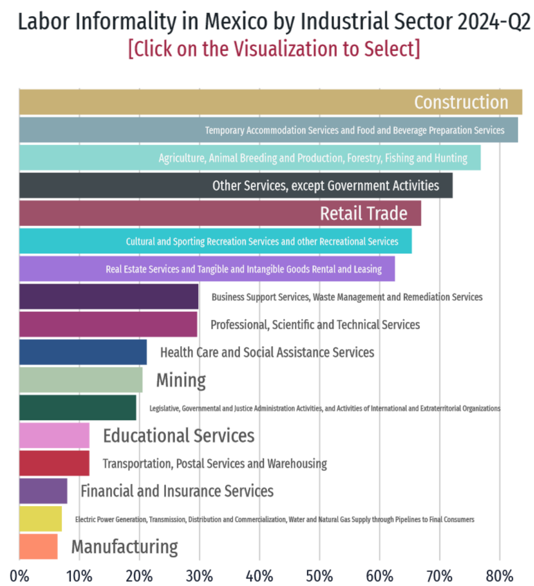
Figure A.2: Accidents over time



(a) Accidents over Time (b) Accidents over Time per 10,000 Workers

Notes: This Figure shows accidents on a monthly basis. 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 Figure was built using all accident data from January 2016 until April 2024. Number of workers per month and sector were retrieved from administrative employer-employee data (IMSS).

Figure A.3: Share of Informality by Sector



www.economia.gob.mx/datamexico/en/profile/geo/mexico?occupationMetrics=workforceOption

Notes: This Figure shows the share of informal workers by sector in the second quarter of 2024. Source: `www.economia.gob.mx/datamexico/en/profile/geo/mexico?occupationMetrics=workforceOption` (INEGI). Last accessed: February 22, 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:

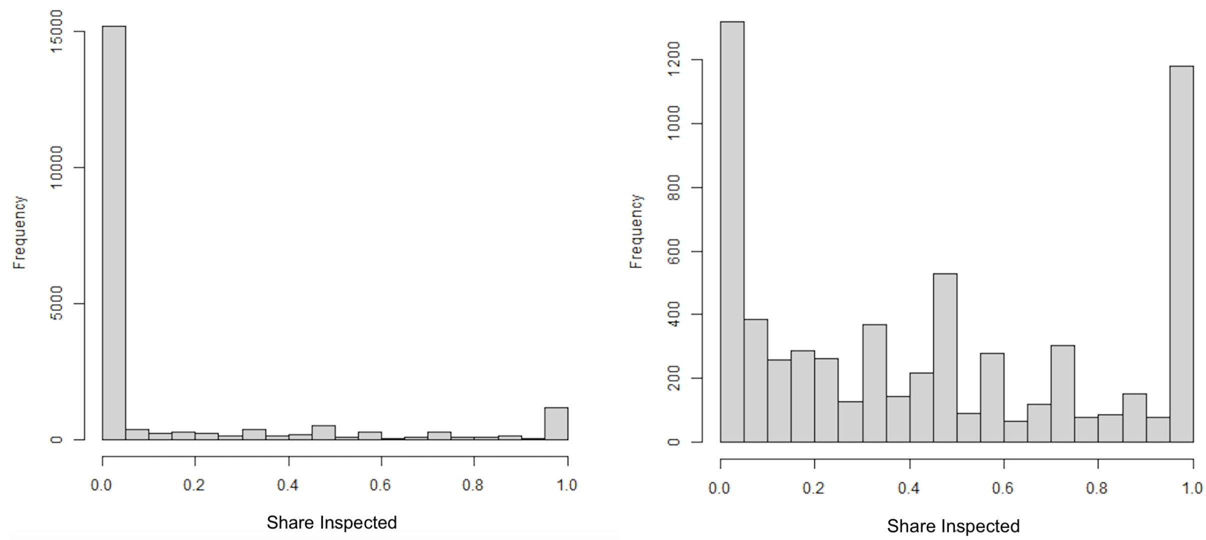
- **Fecha de Alta en el Directorio Nacional de Empresas:** % Menor a 1 año - % De 1 a 3 años - % Mayor a 3 años.
- **Tamaño de la Empresa:** Mínimo 15 trabajadores – Máximo sin límite de trabajadores.
- **Porcentaje de la Clase de Riesgo de IMSS:** %Clase I - %Clase II - %Clase III - %Clase IV - %Clase 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.

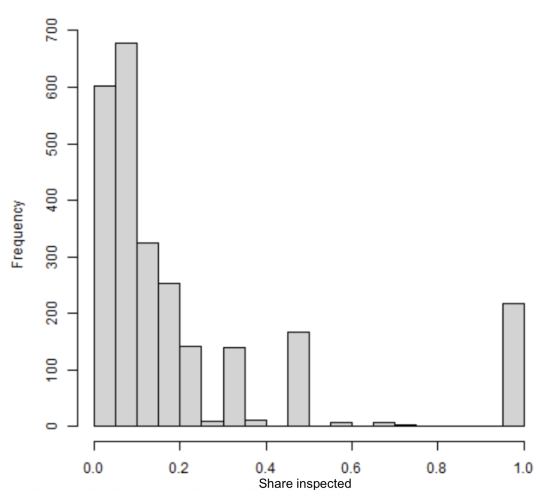
Figure A.5: Distribution in Probability of Inspection across Randomization Groups



(a) All groups (b) Groups with positive inspection probability

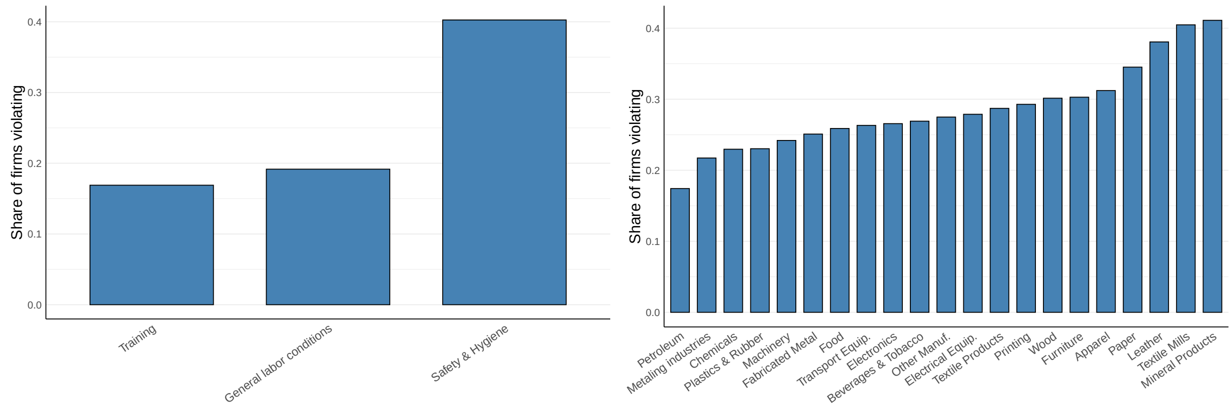
Notes: This figure shows the distribution of the estimated probability of inspection across randomization groups in the Economic Census. Each randomization group is a sector x age group x size group x years since last inspection group x state stratum. Probability of inspection for each group is calculated 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 Labor Regulations



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

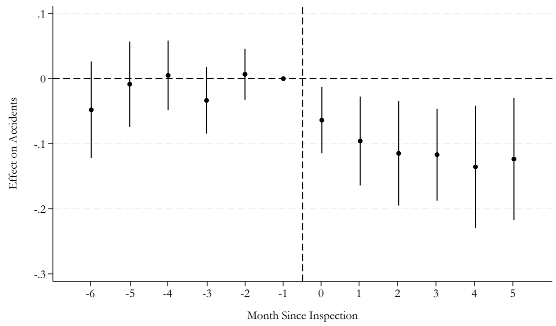
Notes: This Figure shows the share of firms that are found being violating labor regulations by inspection topic (Panel a) and by 21 manufacturing subsectors (Panel b). Figures where built using data from the 2019 Economic Census.

Figure A.8: Coefficients of Violation Regression - Robustness

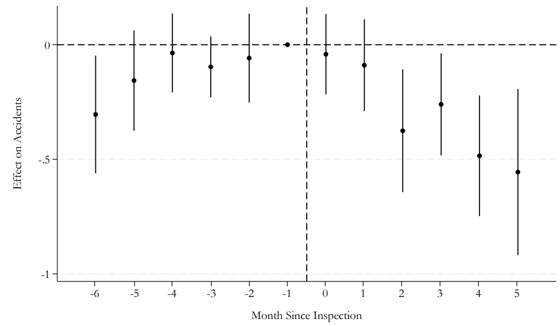
(a) Variables from Model Prediction (b) Variables from Lasso Regularization

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]

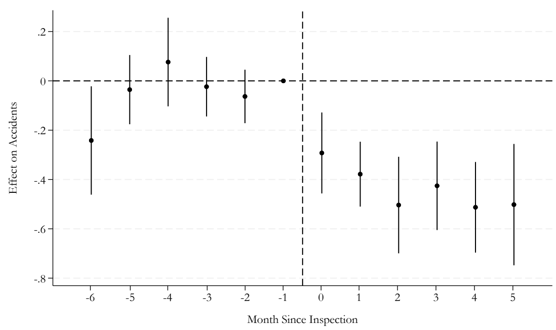
Figure A.9: Robustness Results Effect of Inspections on Accidents



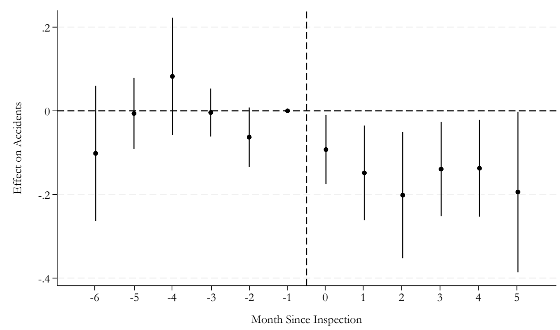
(a) Effect of Ordinary Inspections on Work Accidents - Extended Window



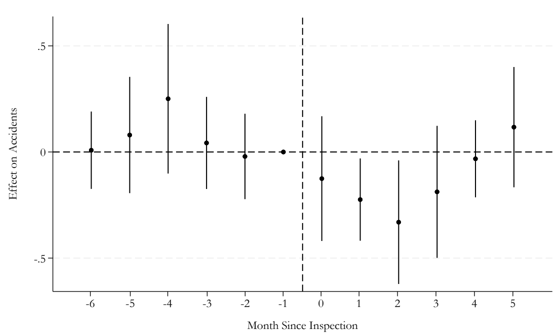
(b) Effect of First Ordinary Inspections on Work Accidents



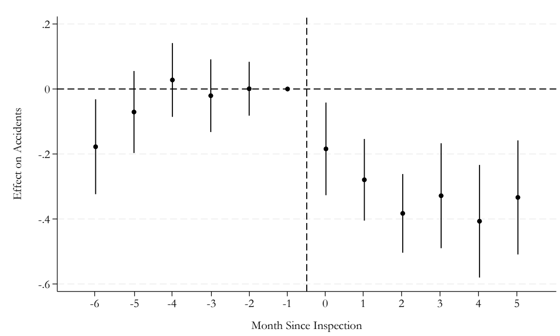
(c) Effect of Ordinary Inspections on Total Accidents



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



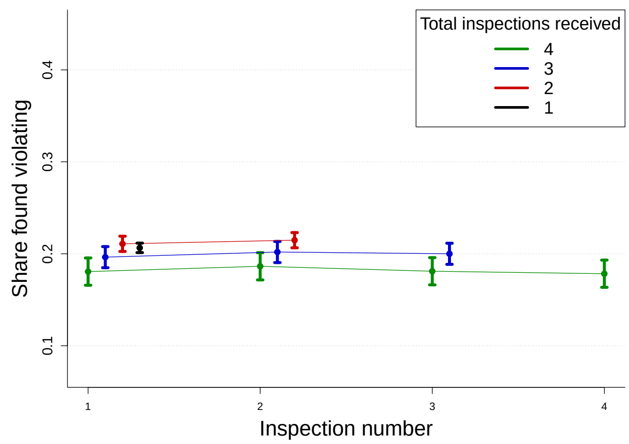
(e) Effect of Ordinary Inspections in Manuf. Sector on Total Accidents in the Manufacturing Sector



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

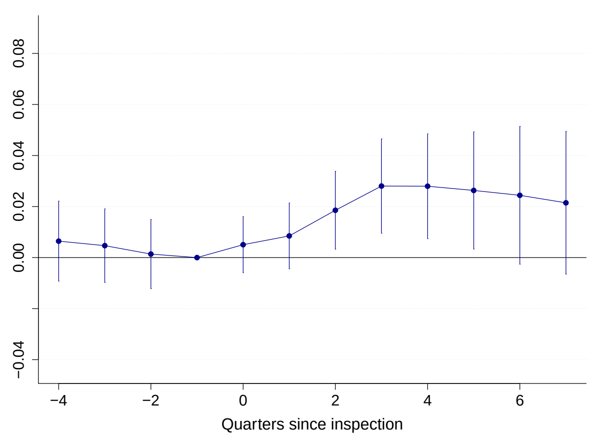
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 5 at the monthly level for $h \in [-6 : 5]$. Regressions are estimated based on a balanced sample of municipalities from the accident data. Treated municipalities are those receiving a stratified random inspection 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 5 but cluster on the municipality x (non-) manufacturing level.

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

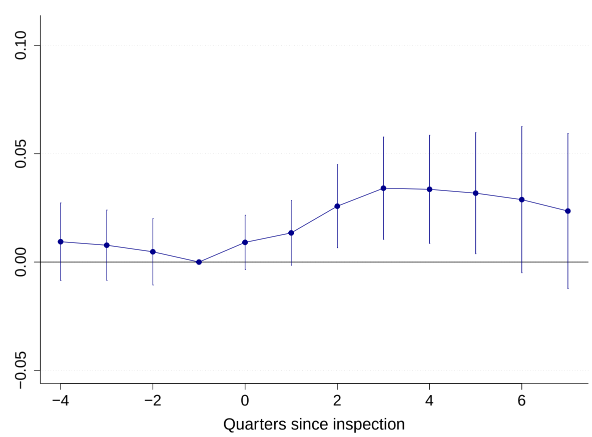


Notes: This figure is constructed using firm level data on inspected firms, from the subsample of inspections merged to DENUE. It shows the coefficients and 95% confidence intervals from regressing a binary variable indicating violation during extraordinary inspection t on a variable indicating how many 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



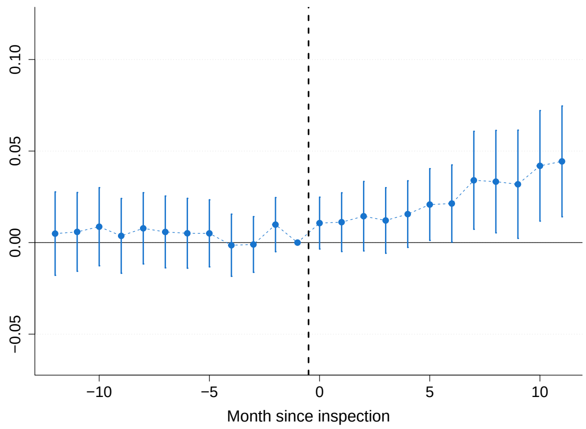
(a) Log Total Workers



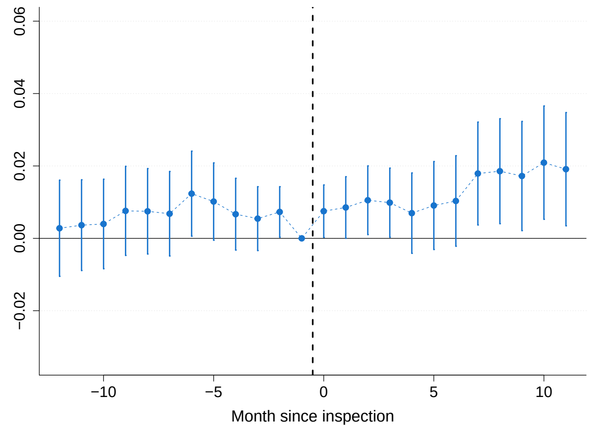
(b) Log Total Production Workers

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the quarterly level for $h \in [-4:8]$. In panel (a) the outcome variable is total workers. In panel (b) 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. Control firms are those untreated for the whole period. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

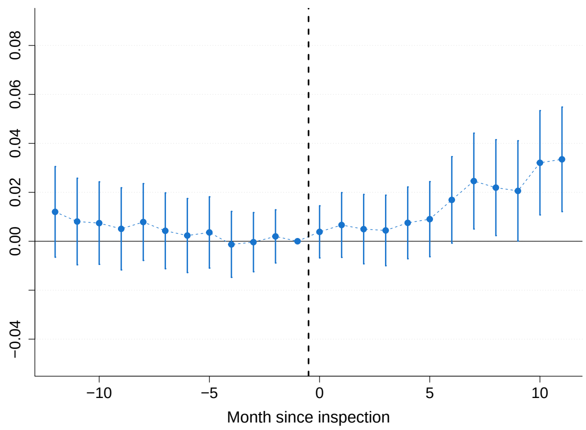
Figure A.12: Effect on Log Total Production Workers - Robustness



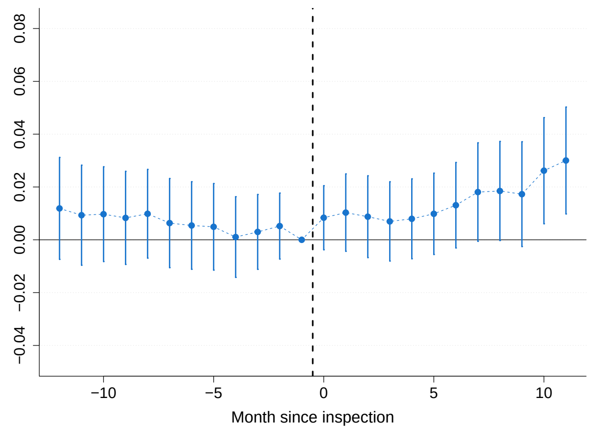
(a) Only Firms with One Establ. in each State



(b) Years 2017 - 2019



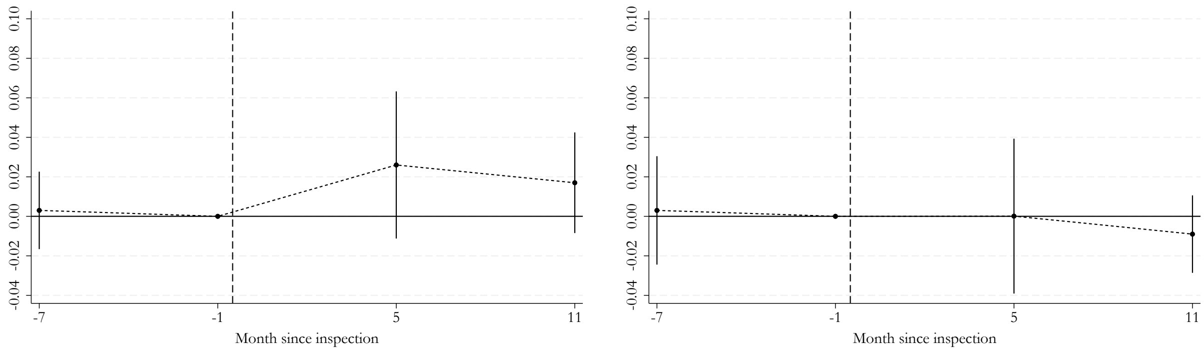
(c) Random. Grp. with broader Sector x Date FEs



(d) Propensity Score Quantile x Date Fixed Effects

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 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), (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.13: Effects on Hiring and Separation Rates

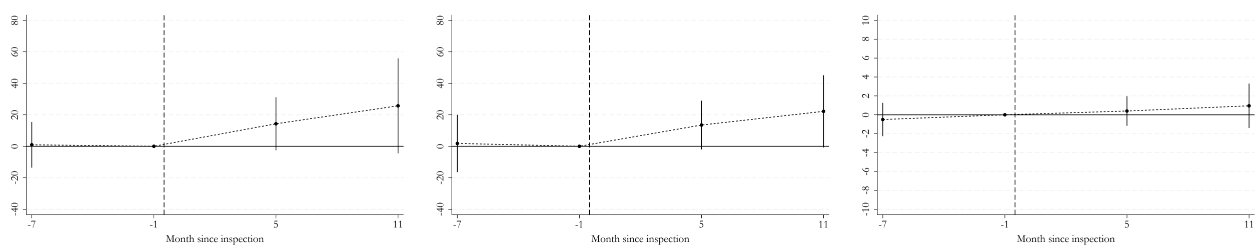


(a) Hiring Rate past 6 Months (Monthly Admin Data)

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

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 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.14: Effects on Type of Hire



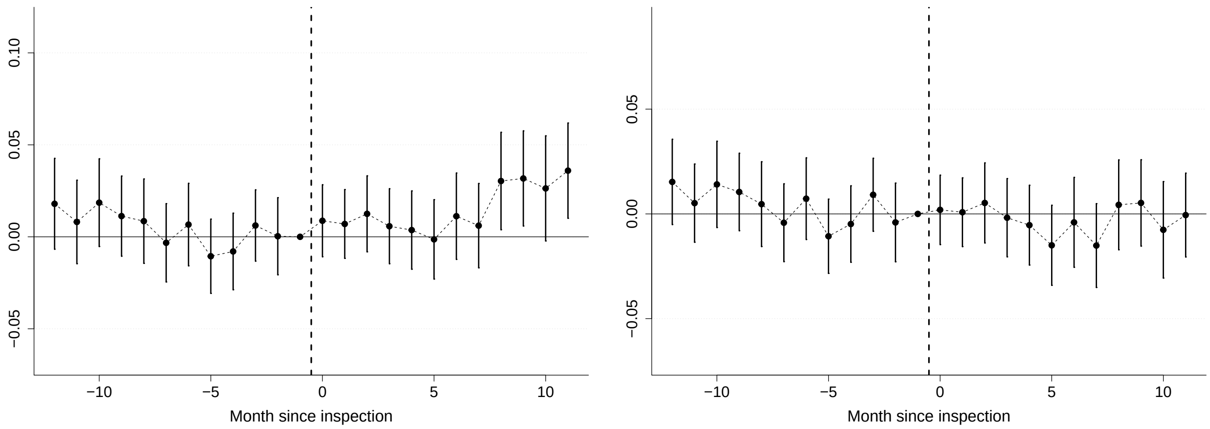
(a) Hires from other Firm

(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 4 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]

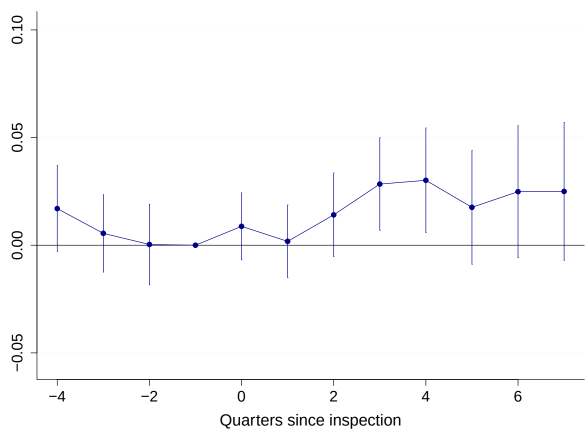
Figure A.15: Effect on Labor Costs



(a) Log Total Labor Costs (b) Log Labor Costs per Worker

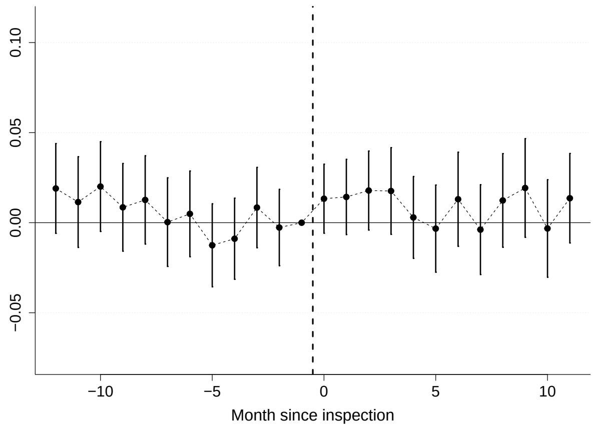
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variable is total labor costs. In panel (b) the outcome variable is average labor costs. Labor costs include wages, social security contributions and costs of outsourced 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 untreated for the whole period. Standard errors are clustered at the firm level.

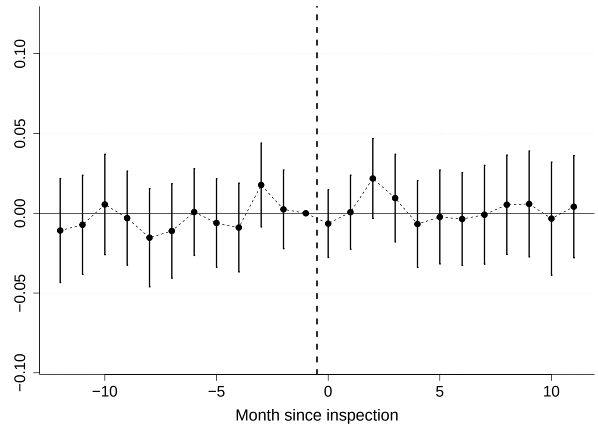
Figure A.16: Effect on Log Total Labor Costs



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the quarterly level for $h \in [-4 : 8]$. The outcome variable is average labor costs. 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 untreated for the whole period. Standard errors are clustered at the firm level.

Figure A.17: Effect on Average Labor Costs per Blue- or White-collar Workers



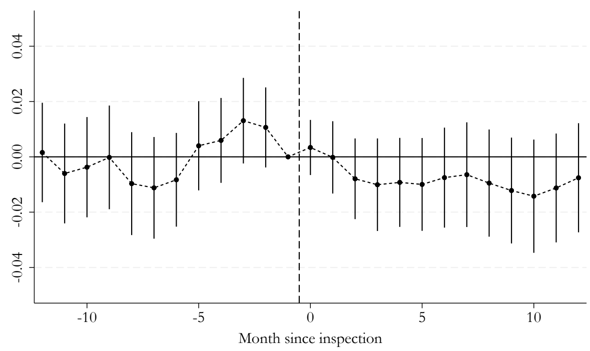


(a) Average Wage Blue-collar

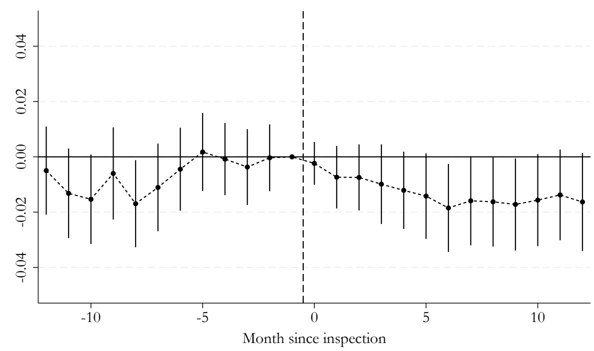
(b) Average Wage White-collar

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variable is average labor costs for blue-collar workers. In panel (b) the outcome is average labor costs for white-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 untreated for the whole period. Standard errors are clustered at the firm level.

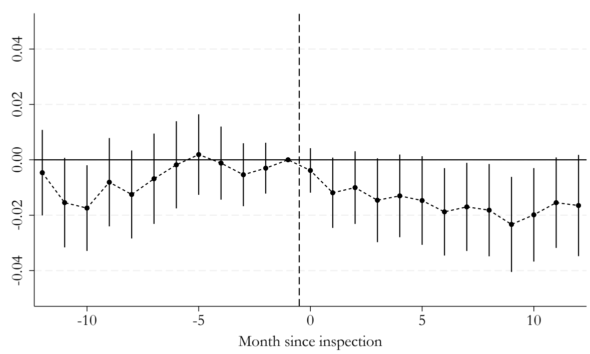
Figure A.18: Effect on Firm Wage Percentiles



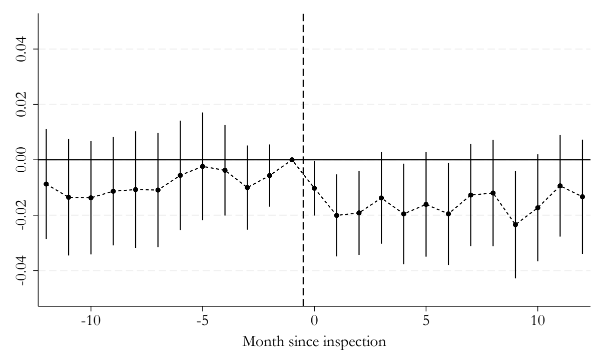
(a) Log Wage P10



(b) Log Wage P25



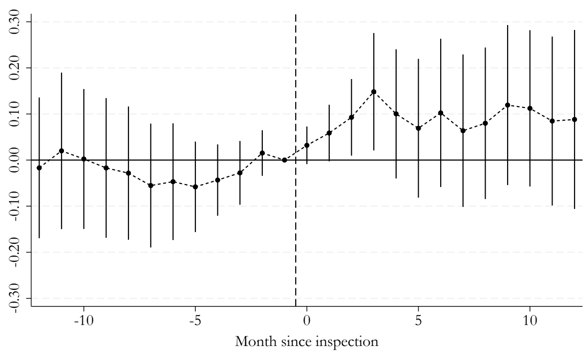
(c) Log Wage P50



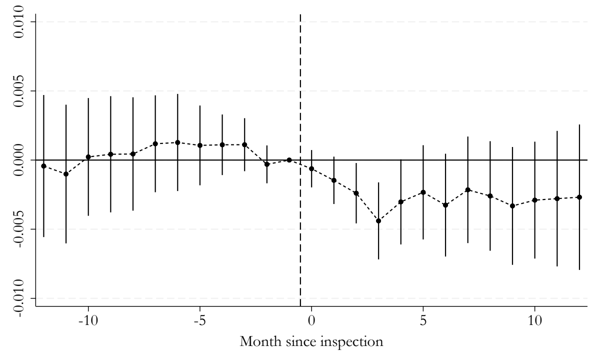
(d) Log Wage P90

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 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.19: Effect on Average Worker Age and Year of IMSS Registrations



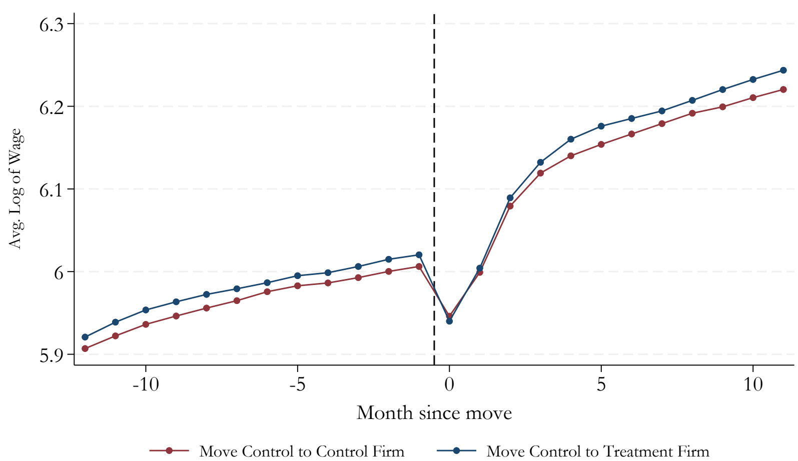
(a) Average Year of Worker Registration in IMSS



(b) Average Worker Age

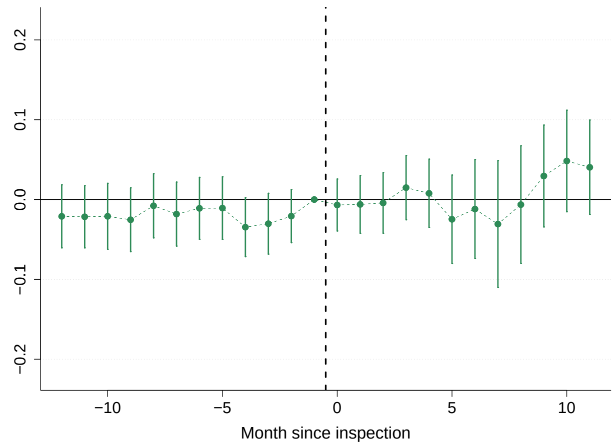
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. In panel (a) the outcome variable is average year of worker registration in IMSS. In panel (b) the outcome is average worker age . Regressions are estimated based 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.20: Log Wage of New Workers



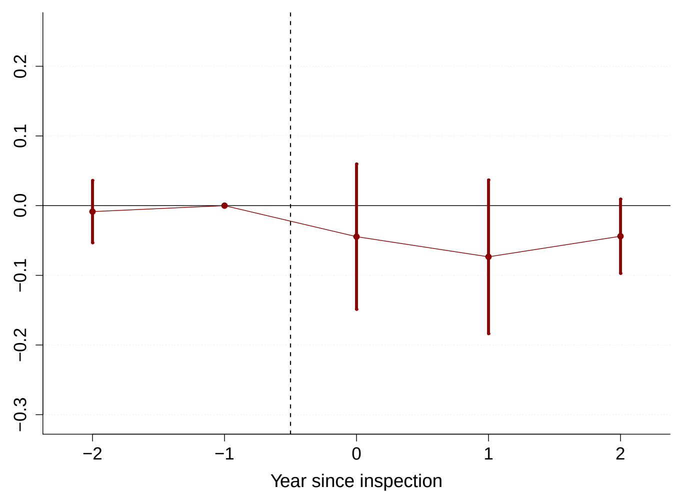
Notes: This Figure the log wage of new workers (hires) who were employed for at least 12 months at firms that had not yet been, or never would be, inspected, and who subsequently switch to either an already (blue line) or another not yet or never inspected (red line) firm, remaining in the new firm for 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.21: Effect on Log Revenue per Worker



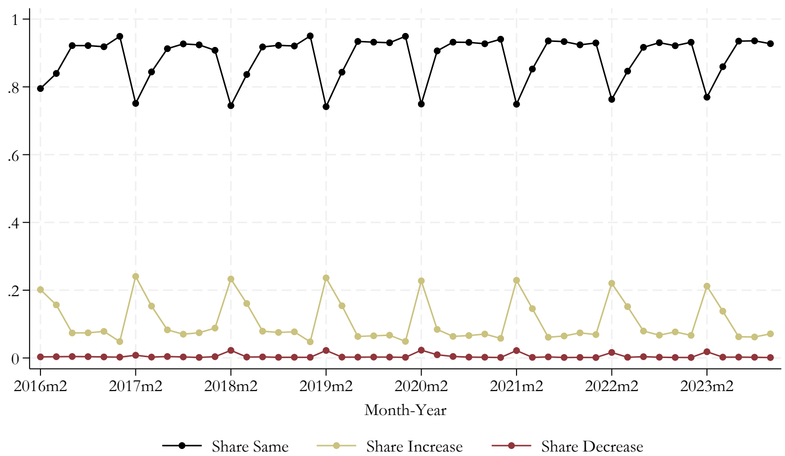
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the monthly level for $h \in [-12 : 11]$. The outcome variable is log monthly revenue per worker. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those untreated for the whole period. Standard errors are clustered at the firm level.

Figure A.22: Effect on Profits over Value Added



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 at the yearly level for $h \in [-2 : 2]$. The outcome variable is yearly profits over 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.23: 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

	Mean	Standard Deviation
Total workers	373	614
Avg. monthly labor costs	17.03	12.38
Share blue-collar workers	0.77	0.17
Share of workers outsourced	0.21	0.39
Yearly profits	156 616	1 016 691
Firm age	24.24	15.71
Multinational	0.29	0.45
N Firms	8793	

Notes: This table reports the mean and standard deviation for main firm characteristics. The sample is based on sample of firms from EMIM. Nominal variables are in thousands of Mexican Pesos.

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 from panel B include state x date fixed effects. The regressions from panel B include randomization group x 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 from panel A include state x date fixed effects. The regressions from panel B include randomization group x 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***	0.0702***	-0.0717***
	(35.82)	(0.00821)	(0.00948)	(2.998)	(3.379)	(0.162)	(10.62)	(10.46)	(0.0134)	(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*	0.00843	-0.00754*
	(47.49)	(0.00805)	(0.0111)	(4.036)	(4.396)	(0.168)	(8.813)	(8.608)	(0.0147)	(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: Summary Statistics of Survey Responses by Salary Category

	Monthly salary		
	Below 10k	Above 10k	All
When choosing current job, considered:			
Salary	0.62	0.83	0.75
Vacation	0.18	0.28	0.25
Growth opportunities	0.32	0.48	0.43
Profit-sharing	0.02	0.19	0.13
On the job training	0.42	0.35	0.39
Working conditions	0.1	0.11	0.1
Performance Bonuses	0.2	0.36	0.31
N	50	75	134

Notes: This table shows responses for self-conducted survey to Mexican workers via Prolific (N=134). 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?.*

Table A.6: 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)
Propensity score	-	-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
Prop score 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 3 the outcome variable is whether the firm was found violation. In Columns 4 and 5 the outcome is 1 if the firm was found violating and a sanction procedure was initiated (firms violating with no sanction procedure are excluded from this estimation). The regressions are run on the sample of manufacturing firms in the Economic Census with more than 20 workers receiving an ordinary inspection between 2019 and 2021. Firm characteristics are correspond to the values in 2018 measured during the economic census. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$. Back to Section 5

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 x state aggregation does not strongly affect the interpretation, nor 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. Overall, we demonstrate that our main results are robust when focusing exclusively on single-establishment firms in Appendix Figure A.12.

Table B.1: Comparison between Original and Matched Datasets

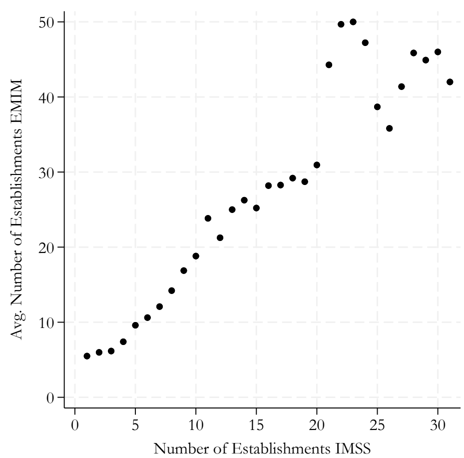
	(1)	(2)	(3)	(4)	(5)
	Inspections	Inspections matched w. DENUE		Inspections matched with DNE	
	Share	Share	$\|(2)-(1)\|$	Share	$\|(4)-(1)\|$
Inspection Type					
extraordinary	0.712	0.694	0.018	0.705	0.007
ordinary	0.241	0.258	0.017	0.248	0.007
follow - up	0.035	0.038	0.003	0.036	0.001
Inspection Topic					
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]

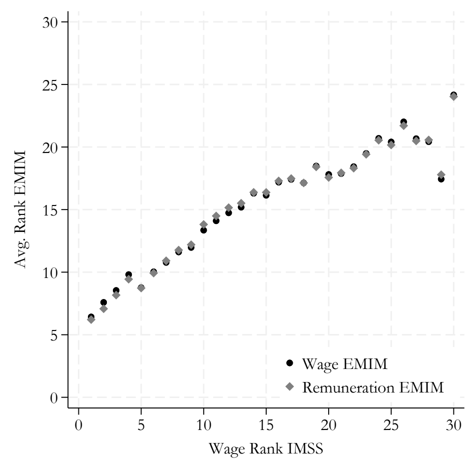
Sample construction with social security data (IMSS) – For the results with the establishment data (EMIM) and social security data (IMSS) to be comparable, it is necessary to work with a sample from the social security data that is comparable to the EMIM data. We construct this sample in IMSS by replicating the EMIM distribution across month, 3-digit manufacturing sector, establishments size (five categories), and city. 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 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 micro-founding the labor supply function 1 using a static discrete choice framework, as is common in the monopsony literature (Cardoso et al., 2018; Berger et al., 2022a). The indirect utility of worker i for working in firm j is:

$$U_i(w_j, a_j) = \beta_w \cdot log(w_j) + \beta_a \cdot log(a_j) + \epsilon_{ij} \tag{8}$$

Where ϵ_{ij} is an idiosyncratic shock of working at firm j, due to commuting costs, heterogeneous preferences, or heterogeneous information frictions, among others. If we assume ϵ_{ij} follows a nested GEV Type I extreme value distribution, where the nests correspond to local labor markets, and that there is strategic complementarity across firms in a local labor market, each firm will face a firm-specific labor supply curve of the following form:

$$n^s(w_j, a_j) = \left(w_j^{\beta_w} \cdot a_j^{\beta_a}\right)^{\theta_j} \tag{9}$$

Where the elasticity of labor supply with respect to the wage is captured by $\beta_w\theta_j$ and the elasticity with respect to the non-wage benefit by $\beta_a\theta_j$. It is a standard result that the labor supply elasticity faced by the firm, θ_j will depend negatively on its labor market share (Berger et al., 2022a).

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 (i) it's total employment n_j and (ii) in the difference between the minimum level of mandatory benefits and the level of benefits it provides. Thus, firm profits are given by the following expression:

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

where firms produce output with labor n_j and firm-specific productivity z_j. Firms pay firm-specific wages w_j. 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 the firm is violating labor regulations, while if $a_j \geq a_{min}$ we state the the firm is in compliance of labor regulations.

C.1 Scenario where firm violates labor regulations $a_j < a_{min}$

FOC w.r.t. firm-specific wage and labor benefit levels:

$$(w_j) : z_j = w_j \left(1 + \frac{1}{\eta_{n,w}}\right) + k a_j + p \cdot \phi(a_{min} - a_j) \tag{10}$$

$$(a_j) : z_j = ka_j \left(1 + \frac{1}{\eta_{n,a}} \left(1 - p \cdot \frac{\phi}{k}\right)\right) + w_j + p \cdot \phi(a_{min} - a_j) \tag{11}$$

If we join both FOC we get:

$$\frac{w_j}{\eta_{n,w}} = \frac{ka_j}{\eta_{n,a}} \left(1 - p \cdot \frac{\phi}{k}\right) \tag{12}$$

Thus, the marginal cost of labor in this scenario is:

$$ka_j \left(1 + \frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a}\right) \left(1 - p \cdot \frac{\phi}{k}\right) + p \cdot \phi \cdot a_{min} \tag{13}$$

Using 11 and 12 we obtain the following expressions for a_j and w_j.

$$a_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p \cdot \phi)} \tag{14}$$

$$w_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)} \tag{15}$$

Using 14 and 15 we obtain the following expression for total employment:

$$n_j^v = \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)}\right)^{\theta_j \beta_w} \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)\left(1 - p \cdot \frac{\phi}{k}\right)}\right)^{\theta_j \beta_a} \tag{16}$$

Profits will be given by:

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

Finally, note that the firm will only be in this scenario if the expression of a_j in Equation 14 is lower than a_{min}. This simplifies to:

$$z_j < ka_{min} \left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right) - p \cdot \phi \cdot a_{min} \left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a}\right) \tag{18}$$

C.2 Scenario where firm complies with labor 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:

$$(w_j) : z_j = w_j \left(1 + \frac{1}{\eta_{n,w}}\right) + ka_j \tag{19}$$

$$(a_j) : z_j = ka_j \left(1 + \frac{1}{\eta_{n,a}}\right) + w_j \tag{20}$$

If we join both FOC we get:

$$\frac{w_j}{ka_j} = \frac{\eta_{n,w}}{\eta_{n,a}}, \tag{21}$$

where the ratio of marginal costs (LHS) is equal to the ratio of labor supply elasticities (RHS). The marginal cost of labor in this scenario is:

$$ka_j\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right) = w_j\left(\frac{1}{\theta_j\beta_w} + \frac{\beta_a}{\beta_w} + 1\right) \tag{22}$$

The optimal a_j and w_j are:

$$a_j^{uc} = \frac{z_j}{k\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)} \tag{23}$$

$$w_j^{uc} = \frac{z_j}{\left(\frac{1}{\theta_j\beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)} \tag{24}$$

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

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

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

$$z_j > ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right) \tag{25}$$

C.3 Scenario where firm complies with labor regulations $a_j = a_{min}$

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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:

$$w_j\left(1 + \frac{1}{\theta_j\beta_w}\right) + ka_{min} \tag{26}$$

The resulting wage and labor and profits are:

$$w_j^{cc} = \frac{z_j - ka_{min}}{1 + \frac{1}{\theta_j\beta_w}} \tag{27}$$

$$n_j^{cc} = \left(\frac{z_j - ka_{min}}{1 + \frac{1}{\theta_j\beta_w}}\right)^{\beta_w\theta_j}(a_{min})^{\beta_a\theta_j} \tag{28}$$

$$\Pi(a_j = a_{min}) = \frac{1}{\eta_{n,w}} \left(\frac{z_j - ka_{min}}{1 + \frac{1}{\theta_j \beta_w}} \right)^{\theta_j \beta_w + 1} (a_{min})^{\theta_j \beta_a} \tag{29}$$

C.4 When do firms violate regulations

Using 18 and 25, we arrive at the following expressions for when the firm will decide to violate:

Non-compliance (violation). If $z_j < ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right) - p \cdot \phi a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a}\right)$ 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 $z \in \left[ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right) - p \cdot \phi a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a}\right), ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)\right]$ the 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:

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

Unconstrained compliance. If $z_j > ka_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)$, the optimal decision of the firm will be to comply with labor regulations and provide $a_j > a_{min}$. The optimal a_j and w_j will be:

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

A few notable patterns emerge from these expressions. First, all else equal, firms with higher productivity, z_j, are more likely to comply with labor regulations. Second, 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, 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 labor regulations.

C.5 Solution with alternative labor supply

In this section, we solve the model under two alternative specifications of the labor supply function the firm faces. First, we analyze a setting where the firm faces an upward-sloping labor supply curve that depends solely on the wage, as in conventional monopsony models (Cardoso 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 (Dube et al., 2022; Mas, 2024).

We obtain two key results. First, in the first 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 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. In the second 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.

C.5.1 Labor supply depending only on w_j

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

$$n^s = w^{\theta_j \beta_w} \tag{30}$$

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:

$$n_j^{violate} = (z_j - p\phi a_{min})^{\theta_j \beta_w} \left(\frac{\theta_j \beta_w}{1+\theta_j \beta_w}\right)^{\theta_j \beta_w} \tag{31}$$

$$\Pi_j^{violate} = \left(\frac{\theta_j \beta_w}{1+\theta_j \beta_w}\right)^{\theta_j \beta_w} \left(\frac{1}{1+\theta_j \beta_w}\right) (z_j - p\phi a_{min})^{1+\beta_w \theta_j} \tag{32}$$

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

$$n_j^{comply} = (z_j - k a_{min})^{\theta_j \beta_w} \left(\frac{\theta_j \beta_w}{1+\theta_j \beta_w}\right)^{\theta_j \beta_w} \tag{33}$$

$$\Pi_j^{comply} = \left(\frac{\theta_j \beta_w}{1+\theta_j \beta_w}\right)^{\theta_j \beta_w} \left(\frac{1}{1+\theta_j \beta_w}\right) (z_j - k a_{min})^{1+\beta_w \theta_j} \tag{34}$$

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 31 and 33 we can see than 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.5.2 Perfectly competitive labor market

In a perfectly competitive labor market, the elasticity of the labor supply function in 9, $\theta_j \to \infty$. The utility of total compensation $\bar{U}$, is determined in the competitive market and taken as given by the firm (Mas, 2024).

$$w_j^{\beta_w} \cdot a_j^{\beta_a} = \bar{U} \tag{35}$$

Using Equation 35, 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] When the firm does not comply with minimum amenities requirements ($a_j < a_{min}$), the first-order condition with respect to w_j and n_j are:

$$(w_j): \quad w_j = \left((k - p\phi) \bar{U}^{\frac{1}{\beta_a}} \frac{\beta_w}{\beta_a} \right)^{\frac{\beta_a}{\beta_a + \beta_w}} \tag{36}$$

$$(n_j): \quad z_j f'(n_j) = w_j + (k - p\phi) \underbrace{\bar{U}^{\frac{1}{\beta_a}} w_j^{-\frac{\beta_w}{\beta_a}}}_{a_j} + p \cdot \phi a_{min} \tag{37}$$

The firm will only be in the non-compliance scenario if $a_j < a_{min}$. Substituting the expression for a_j into 36, this implies:

$$\bar{U}^{\frac{1}{\beta_a}} \cdot \left((k - p\phi) \bar{U}^{\frac{1}{\beta_a}} \frac{\beta_w}{\beta_a} \right)^{\frac{-\beta_w}{\beta_a + \beta_w}} < a_{min} \tag{38}$$

When the firm complies with labor regulations by providing exactly $a_j = a_{min}$ (constrained compliance), we can use Equation 35 and the FOC of the profit function with respect to n_j to derive the expressions for wages and labor:

$$w_j = \left(\frac{\bar{U}}{a_{min}^{\beta_a}} \right)^{\frac{1}{\beta_w}} \tag{39}$$

$$z_j f'(n_j) = \left(\frac{\bar{U}}{a_{min}^{\beta_a}} \right)^{\frac{1}{\beta_w}} + k a_{min} \tag{40}$$

[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.

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 (35) (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 37, 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 labor hired, defined by 40, 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.5.3 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:

$$n^s = a_j^{\theta_j \beta_a} \tag{41}$$

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:

$$a_j = \frac{z_j - \tilde{w} - p\phi a_{min}}{\left(1 + \frac{1}{\theta_j \beta_a}\right)(k - p\phi)} \tag{42}$$

While labor is given by:

$$n_j = \left(\frac{z_j - \tilde{w} - p\phi a_{min}}{\left(1 + \frac{1}{\theta_j \beta_a}\right)(k - p\phi)} \right)^{\theta_j \beta_a} \tag{43}$$

The firm will be in the non-compliance scenario if the expression in 42 is lower than a_{min}. This translates into:

$$z_j - \tilde{w} < k a_{min} \left(\frac{1}{\theta_j \beta_a} + 1 \right) - p \cdot \phi \cdot a_{min} \frac{1}{\theta_j \beta_a} \tag{44}$$

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 41 and given by:

$$a_{min}^{\theta_j \beta_a} \tag{45}$$

Notably, in this scenario, both Predictions 1 and 2 from the benchmark model are confirmed. In contrast to the alternative case where labor supply depends solely on wages, Equation 44 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 43 and 45, 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 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 mechanisms 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.

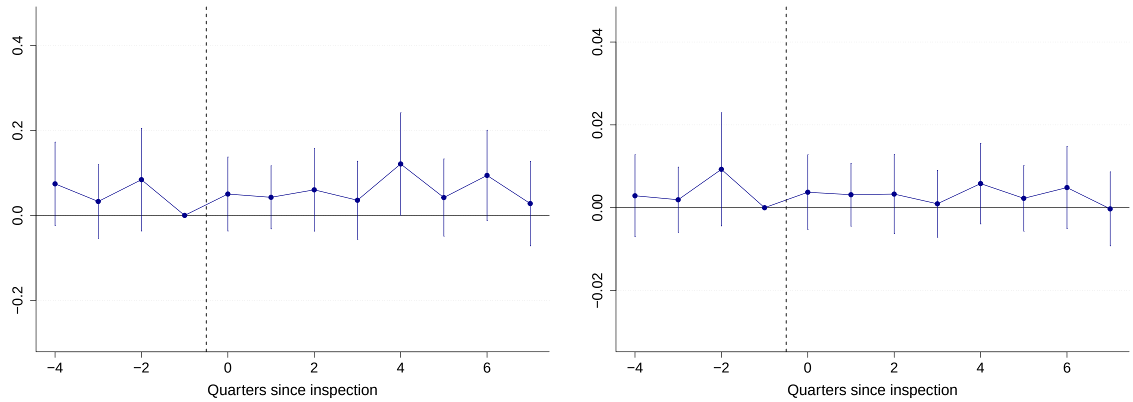
First, the monthly manufacturing survey EMIM is designed to capture information on total workforce, including both formal and informal workers. Consequently, we would should not expect to see an increase in total employment if firms only formalize informal workers.[53] Even if firms were inclined to mis-report 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.2, we find no significant effect of inspections on social security contributions per worker or over average wage at the 5% level.

[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.

Figure B.2: Effect on Average Social Security Contributions



(a) Social Sec. Contrib. per Worker

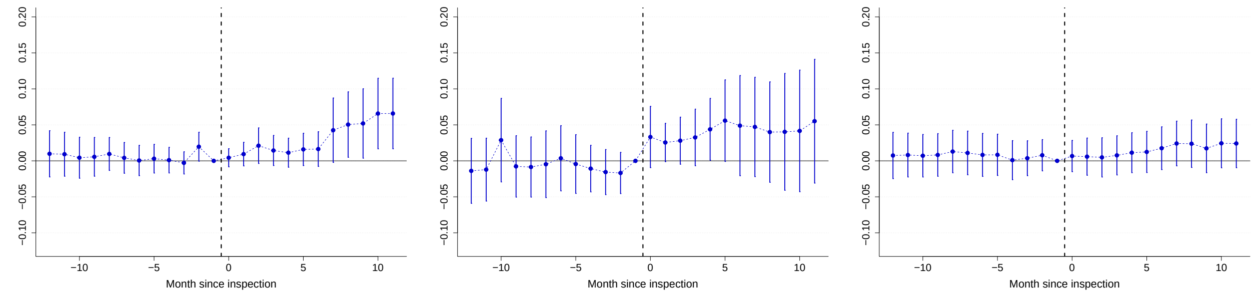
(b) Social Sec. Contrib. over Average Wage

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 4 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 over the average wage. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those untreated for the whole 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.3 show the opposite pattern. The most significant effects emerge from inspections related to workplace safety, hygiene, and training.

Fourth, while the increase in employment is primarily driven by additional hires, Figure A.14a shows that approximately half of these additional hires are workers previously employed at other firms 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.3: 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 4 at the monthly level for $h \in [-12 : 11]$. The outcome variable is total 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 untreated for the whole 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 indicates that firms classified with a high risk of fire must form 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 machinery. If existing staff aren't sufficient or qualified, 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 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:

[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

$$\Pi_j = z_j n_j - w_j \underbrace{n_j}_{\substack{\text{productive}\\\text{employment}}} - \tilde{w} \underbrace{q \cdot n_j}_{\substack{\text{specialized}\\\text{employment}}} \tag{46}$$

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.5, 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 the 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.13 shows that inspections do not cause elevated separation rates. Thus, a mechanical increase in 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.3 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 was 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 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 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.