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

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Abstract

This paper studies the characteristics of large formal firms that violate workplace regulations and analyzes how regulatory enforcement affects firms and workers. A stylized model in which monopsonistic firms set both wages and working conditions shows that labor market power can lead to violations in workplace regulations, and that the enforcement of such regulations can raise firm employment through an expansion of labor supply. We test the model's predictions using administrative records of stratified random firm inspections in Mexico aimed at enforcing compliance with workplace safety regulations and mandatory worker training, linked to panel surveys and administrative employer-employee data for large manufacturing firms. Consistent with our model, firms that are more likely to be found in violation of regulations have lower productivity, employ a smaller share of women, are located in more concentrated labor markets and have a higher labor market share. We then show that inspections lead to higher levels of regulatory compliance, reflected in greater investment in worker training and fewer workplace accidents. Using a staggered difference-in-differences design, we estimate that inspections increase firm employment by approximately 4% within one year, explained by both an increase in total firm hires and higher retention rates of incumbent workers. Average firm wages decrease by less than 2%, driven equally by changes in workforce composition and adjustments in firms' average wage-setting. Back-of-the-envelope calculations bound workers' willingness to pay for an inspection between 1% and 10% of pre-inspection wages. Our results indicate that enforcing workplace regulations among large manufacturing firms can be an effective policy tool to improve working conditions and mitigate the effects of labor market power.

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1 Introduction

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

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

In this paper, we study the role of labor market power in explaining violations of workplace regulations and in shaping the effects of regulatory enforcement on large firms and their workers. We analyze both aspects theoretically and empirically, leveraging microdata on stratified random workplace inspections in Mexico merged with firm-level outcomes from panel survey and administrative social security data. Our analysis focuses on the manufacturing sector, which is responsible for 43% of all Mexican 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 to guide the empirical analysis in which monopsonistic firms face an upward-sloping labor supply function that depends on both wages and non-wage amenities, with the latter capturing working conditions. Monopsony power enables firms to offer a combination of wages and working conditions below competitive levels. Firms are subject to a minimum mandated level of amenities (workplace regulations), which is imperfectly enforced. Firms providing amenities below this mandatory level are considered non-compliant and are fined if detected through inspections that occur with positive probability. The framework yields two main predictions, which we test empirically. First, a firm's incentive to violate the minimum amenity 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

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

facing a labor supply that is relatively inelastic to non-wage amenities, i.e., firms with greater market power over amenities, are more likely to violate regulations. The second prediction concerns the consequences of enforcement for these non-compliant firms. Firms forced to comply (and therefore to provide higher levels of amenities) can respond by offsetting these costs through lower wages. Crucially, *if* firms possess market power over amenities, then enforcement *can* in principle lead to an increase in total firm employment (Manning, 2003). The intuition behind this result is that, because monopsonistic employers choose a wage—amenity bundle below the competitive benchmark, enforcement moves the firm along its upward-sloping labor supply curve, resulting in higher employment (Lagos, 2026).[2] The rise in labor supply occurs because workers value the improvement in working conditions following enforcement. As long as the accompanying wage reduction does not fully offset this gain, the job becomes more attractive to workers, labor supply to the firm expands, and employment increases.[3]

Second, we empirically assess the model's first prediction by analyzing firm characteristics associated with being found violating workplace regulations during inspections. We leverage records of stratified random firm inspections that enforce compliance with health and safety standards, mandatory training, and general labor conditions. We link these records with detailed survey and administrative employer-employee panel data of large manufacturing firms. The stratification process, which we validate empirically, enables us to control for the inspection probability separately from firm, worker, and local labor market characteristics. We first show that empirical proxies for the model variables determining violation correlate with non-compliance, controlling for the inspection probability and additional firm characteristics selected via a post-double lasso approach. Specifically, firms with a lower share of female workers (a proxy for a workforce with lower relative responsiveness to non-wage conditions, see Maestas et al., 2023), a higher labor market share and location in more concentrated labor markets (proxies for higher labor market power, see Azar et al., 2022) are significantly more likely to be found violating. Low-productivity firms are significantly more likely to be found in violation and subsequently sanctioned.

Third, we test the model's second prediction by studying the impact of enforcing workplace regulations through stratified random workplace inspections on a rich set of firm and worker characteristics. Our estimation is based on a local projections staggered difference-in-differences design (Dube et al., 2025), comparing first-inspected firms to those not yet or never inspected (clean controls) within the same randomization group. The central mechanism underlying this analysis is that inspections increase compliance with workplace regulations, which is well-supported by the institutional context: inspections inspections aim to ensure compliance rather than maximize revenues via

[2] In a monopsonistic setting, the effect of enforcement on employment is determined by its effect on labor supply, provided the mandated amenity level is not high enough to push the firm beyond the competitive allocation. The mandated amenity level in this setting is unlikely to exceed the competitive benchmark given that the requirements consist of minimum legal standards.

[3] This result requires that firms do not find it optimal to fully offset the mandated increase in amenities with lower wages. We formally prove this result by showing that when labor supply is increasing in amenities, an enforcement-induced improvement in amenities can reduce the marginal cost of labor even if average labor costs rise. This leads the firm to offer a higher job value to attract more workers. The mechanism is analogous to the standard monopsony model in which firms set only wages: a binding minimum wage can in principle increase employment by lowering marginal labor costs.

fines, and follow-up inspections are frequent when firms are initially found non-compliant. Additionally, we present several pieces of empirical evidence consistent with increased compliance following inspections. First, inspected firms increase their investment in employee training per worker, a mandated workplace benefit in this context, by approximately 30%,[4] and raise spending on maintaining production equipment, an investment that is closely linked to safer working conditions for production workers, by 5%. Second, inspected firms are less likely to be found violating in subsequent stratified random inspections, suggesting lasting improvements in compliance. Third, inspections are associated with a subsequent decline in workplace accidents at the municipality level by around 9%, consistent with improved compliance with workplace safety regulations. Fourth, inspections do not reduce firm survival among the analyzed large formal firms, consistent with a compliance-oriented approach that emphasizes adherence to regulations rather than punitive enforcement.[5]

We next assess how firms' employment and wage setting respond to the enforcement of workplace regulations through inspections. Consistent with the model's second prediction, we find that firm employment increases by approximately 4% one year after an inspection in both the survey and in the administrative data samples, driven primarily by increases in blue-collar workers. These effects are concentrated among firms found in violation during the inspection, consistent with these firms being most affected by enforcement. This increase in employment reflects both a 6% rise in firm-level hiring during the year following the inspection and a 1 percentage-point increase in the retention of incumbent workers.[6] The average firm wage declines by approximately 1%, and we can reject declines larger than -2.1%. Using a time-varying AKM decomposition (Lachowska et al., 2023), we show that approximately half of the average firm wage decline is driven by changes in workforce composition, while the remaining half is attributable to changes in firms' wage-setting. These results are consistent with the model: firms do partially offset the cost of improved working conditions through lower wages, but the magnitude of this response is small. At the worker level, we find precise null effects of inspections on wages for incumbents, suggesting that wage adjustments are driven primarily by modest changes in the wages of new hires. Back-of-the-envelope calculations imply a wage-setting effect for new workers that is not larger than -2.9 percent. Finally, inspections increase both total revenue and total production costs by approximately 6%, consistent with a real expansion of firms' production following the increase in employment.[7]

These empirical results align with our theoretical model in which firms use their market power to offer substandard working conditions. The combined evidence that inspections improve compliance, increase firm employment, and have only modest effects on worker wages supports the main theoretical mechanism: when enforcement improves working conditions among monopsonistic employers

[4]Under Mexico's Federal Labor Law, firms are required to provide mandatory employee training (LFT, 2021).

[5]Firm-level survey data on regulatory quality (ENCRIGE) indicates that bribes during STPS inspections are extremely rare. Reported compliance costs in this sample are approximately 5% of worker annual wages.

[6]New hires equally stem from other formal firms or from outside formal employment in the last month but with previous formal employment. We do not find an increase in hires without prior formal attachment. The increase in retention of hires is explained by a decrease in job-to-job transitions and not by a decrease in job-to-nonemployment transitions, both consistent with a decline in voluntary separations.

[7]Total hours worked increase, while average hours worked remain unchanged, indicating that firms do not adjust to the changes in wages and employment by altering hours worked. We find suggestive evidence of a decline in firm-level profits. Information on profits is not directly observable and has to be constructed from revenue and observed cost data, likely attenuating the impact of inspections on profits.

and this improvement is not fully offset with lower wages, firm employment rises through an increase in labor supply. The positive effects on both incumbent worker retention and new hiring suggest that the increase in labor supply reflects a greater willingness of both incumbent workers to remain at inspected firms and prospective workers to accept jobs there. We provide additional evidence consistent with this mechanism using Mexico's National Employment Survey, which shows that poor workplace conditions are an important factor in workers' decisions to quit a job. Additionally, the majority of workers in large manufacturing firms report finding their jobs through friends and family. Therefore, it is plausible that newly hired workers are relatively well-informed about minimum workplace conditions and more likely to select into firms where these conditions have improved following an inspection.

We finally discuss four 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, misinformation among firms regarding the potential costs and benefits of improving working conditions (Bertrand and Crépon, 2021), and a change in the managerial structure of the firm. Our empirical evidence suggests that these explanations are either inconsistent with empirical evidence, unlikely to fully account for the magnitude of the employment effects, or they also operate through our main mechanism in which employment rises because inspections increase labor supply to monopsonistic firms.

We conclude by combining our empirical estimates with the theoretical framework to bound the total increase in the value of working conditions between 2.5% and 28%. From this, we infer workers' willingness to pay for an inspection, ranging between 1% and 10% of pre-inspection wages.[8]

Related literature. This paper contributes to three main strands of the literature. First, we contribute to the research on labor regulations (Besley and Burgess, 2004; Botero et al., 2004; Kugler, 2004; Almeida and Carneiro, 2012), and more specifically to the literature on their enforcement. A growing strand of work in this area focuses on the effects of inspections that induce the formalization of previously informal workers. Brotherhood et al. (2024) and do Prado et al. (2025) show that formal firms caught with informal workers during inspections in Brazil experience an increase in formal employment in the short run but a decrease in employment and a rise in exit probability in the long run. Closely related to our setting but with a focus on substantially smaller firms on average, Samaniego de la Parra and Fernández Bujanda (2024) use Mexican firm inspection data linked to social security data and worker survey data across all sectors to show that inspections temporarily increase formalization rates but lead to persistently lower overall formal employment. Evidence on large formal firms in low- and middle-income countries—the firms most comparable to those in our sample—has mostly focused on multinational corporations and global supply chains, with mixed findings. For instance, Boudreau (2024) and Harrison and Scorse (2010) find that stricter labor standards in Bangladesh and Indonesia, respectively, improve workplace conditions or wages without adverse employment effects, whereas Alfaro-Ureña et al. (2025) finds that responsible sourc-

[8]The range of effects on the value of working conditions is consistent with our empirical estimates of the effects of inspections on compliance proxies, including worker training and workplace accidents. The resulting willingness-to-pay estimates are also broadly consistent with estimates for related workplace amenities in the literature (Maestas et al., 2023; Dale-Olsen, 2006).

ing practices in Costa Rica increase wages but reduce employment.[9] Our paper contributes to this literature on regulations and enforcement in two main respects. First, we provide novel theoretical and empirical insights on the role of labor market power in explaining workplace regulation violations (beyond informal employment) among large formal firms and in shaping the consequences of enforcement. This focus motivates our emphasis on larger, more productive manufacturing firms, where monopsony forces are likely to be stronger and informality shares are very low.[10] Second, to our knowledge, this is the first paper to link inspection records to rich firm-level manufacturing surveys. This linkage allows us to estimate the effects of stratified random enforcement of workplace regulations on a broad set of novel firm outcomes, including costs and revenues. It also allows us to present new evidence on the firm, worker, and local labor market characteristics associated with workplace regulation violations, a topic for which evidence remains largely absent from the literature.[11]

Second, we contribute to a growing literature on non-wage job amenities (Mas, 2025). Recent empirical work has shown that workers place substantial value on non-wage amenities and that differences in the provision of these amenities across firms impact measures of labor market inequality (Mas and Pallais, 2017; Maestas et al., 2023; Humlum et al., 2025; Morchio and Moser, 2026; Faberman et al., 2026). However, causal evidence on how firms adjust to exogenously enforced changes in non-wage amenities remains very limited. Dube et al. (2022) and Davies et al. (2026) assess the opposite relationship by studying the impact of increases in the U.S. minimum wage on workplace amenities including job autonomy and coworker relationships, and workplace injuries, respectively. Lagos (2026) studies how firms covered by collective bargaining agreements simultaneously adjust wages, non-wage amenities, and employment in response to an increase in union bargaining power. On the theoretical side, Summers (1989) argues that mandated benefits can reduce employment in competitive labor markets, while Manning (2003) shows that, under monopsony, mandated non-wage benefits can instead increase employment. We contribute to this literature by exploiting exogenous within-firm variation in non-wage working conditions related to workplace safety and employer-provided training to provide novel evidence on the under-provision of minimum amenities and on how firms adjust to improvements in these non-wage amenities. We further combine this empirical analysis with a theoretical framework including labor market power to interpret the mechanisms underlying these adjustments.

Third, this paper contributes to the literature studying monopsonistic power among employers in both developed (Manning, 2004; Berger et al., 2022) and developing countries (Felix, 2022; Amodio and De Roux, 2024; Sharma, 2023). Most of this literature focuses on the impact of labor market

[9] In a high-income setting, Johnson (2020) and Johnson et al. (2023) find that health and safety inspections in the U.S. improve compliance but do not observe other firm outcomes.

[10] Monopsony power over working conditions—which underlies our finding of positive employment effects—is likely much weaker among the sample of smaller firms considered in Samaniego de la Parra and Fernández Bujanda (2024); Brotherhood et al. (2024); do Prado et al. (2025). Appendix E.1 provides a detailed analysis reconciling the findings in these studies with those in this paper.

[11] Most of the literature has focused on the causal effects of enforcement. Firm-level evidence on characteristics correlating with workplace regulation violations is largely absent. Campusano et al. (2024) measure compliance rates with labor regulations among Chilean firms but do not relate non-compliance to firm characteristics. Marinescu et al. (2021) show that in U.S. local labor markets, higher compliance with labor regulations is associated with higher wages, lower labor market concentration, and greater union coverage. Evidence studying the drivers of non-compliance with taxes has evolved much more in this regard (Carrillo et al., 2017; Guyton et al., 2023; Slemrod, 2019).

power on suppressing worker wages and distorting the allocation of workers across firms. A smaller strand of research investigates how monopsony power affects the provision of both wage and non-wage job attributes (Dube et al., 2022; Lagos, 2026).[12] Additionally, this literature has studied various policies to counter labor market power, such as the minimum wage (Engbom and Moser, 2022; Berger et al., 2025; Dustmann et al., 2022) and collective bargaining (Lagos, 2026) which typically affect multiple firms simultaneously. We complement this literature in two ways. First, we provide new evidence consistent with firms using labor market power to offer non-wage working conditions below the required minimum legal standard. This implies that the consequences of monopsony extend beyond wages to the provision of essential workplace amenities. Second, we show how labor market power shapes firms' responses to improvements in non-wage working conditions induced by inspections. By exploiting exogenous within-firm variation in enforcement, our empirical analysis mitigates the spillover and endogeneity concerns that often arise when studying broader policy interventions on the labor market.

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 and verifies the randomization process of inspections. Section 5 tests the first model prediction, reporting characteristics of firms being found violating during inspections. Section 6 provides evidence that inspections increase compliance with workplace regulations and estimates the reduced-form effects of an inspection on firms and workers. Section 7 contrasts our main mechanism with alternative ones and combines it with our empirical estimates to infer workers' willingness to pay for inspections. Section 8 concludes.

2 Theoretical Framework

In this section, we present a simple theoretical framework for assessing the factors that influence firms' decisions to violate workplace regulations and how wages and firm employment respond to enforced compliance. In the model, a representative firm j with productivity z_j produces output $z_j f(n_j)$ with labor n_j. When hiring workers, the firm posts a compensation bundle consisting of a wage, w_j, and a non-wage amenity, a_j, that refers to working conditions. The government imposes a mandated minimum level of amenities per worker, denoted as a_{min}. However, enforcement of this minimum is imperfect, allowing firms to provide amenities below a_{min}. If a firm offers $a_j < a_{min}$ and is inspected, which occurs with probability p, it must pay a fine equal to $\phi \cdot n_j(a_{min} - a_j)$. Based on the institutional setting in Mexico (described in Section 3) the expected fine increases with the firm's total employment and the extent of per worker non-compliance $(a_{min} - a_j)$ (STPS, 2014).[13] We normalize the price of output to one and assume firms to be risk-neutral. Expected firm profits depend on the decision whether to provide amenities above or below the mandated minimum level a_{min} and are given by:

[12] 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).

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

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

where k is the cost of each amenity unit per worker. We assume that $p \cdot \phi < k < \phi$.[14] If $a_j < a_{min}$, the firm provides a level of amenities below the mandatory level and is thus violating workplace regulations; otherwise, it is compliant. We assume the firm faces the following labor supply function, which is increasing in both components of total compensation, following Lagos (2026).[15]

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

where $\beta_w + \beta_a = 1$. β_w and β_a define the relative elasticity of labor supply with respect to wages and amenities, respectively. θ_j represents the elasticity of labor supply with respect to the value of a job, defined as: $V_j = \left(w_j^{\beta_w} \cdot a_j^{\beta_a} \right)$. This Cobb–Douglas labor supply specification implies that wages and workplace amenities are imperfect substitutes, as is common in the literature (Dube et al., 2022; Mas, 2025). 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. While Lagos (2026) models adjustments to wages and amenities as a bargaining solution between firms and unions, we instead focus on how the enforcement of government-mandated minimum workplace amenities affects firms that are initially non-compliant. We solve the model analytically with $f(n_j) = n_j$ in Appendix D and present the main predictions below.

Prediction 1. Compliance with workplace regulations. *If the firm faces a firm-specific labor supply function that is upward sloping in amenities ($\beta_a > 0$) and wages ($\beta_w > 0$), the optimal decision on whether to violate workplace regulations or not will be given by the following expressions:*

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

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

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

A few notable patterns emerge from these expressions. First, all else equal, firms with higher productivity, z_j, are more likely to comply with workplace regulations. Second, all else equal and provided $k > p \cdot \phi$, firms with lower θ_j, meaning they possess greater labor market power, are more

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

[15] We provide a microfoundation for this labor supply function in Appendix D. This setup differs from 'new classical' monopsony models, such as (Berger et al., 2022; Lamadon et al., 2022) which assume that amenities are exogenous for the firm.

inclined to violate regulations. This occurs because workers in these firms are less responsive to unfavorable working conditions, allowing employers to reduce benefits with little impact on their capacity to hire and retain workers. Third, all else equal, non-compliance becomes more likely when $\frac{\beta_a}{\beta_w}$ is lower, as workers are relatively less sensitive to amenities than to wages. Fourth, all else equal, the prevalence of non-compliance decreases as the probability of inspection p increases, as stronger enforcement raises the expected cost of violating workplace regulations. Figure C.2 shows illustrative examples of firm profits as a function of working conditions a_j for different values of θ_j, our main parameter of interest. We focus on three values of θ_j that illustrate the profit function in the case where each of the cases is optimal: non-compliance, constrained compliance, and unconstrained compliance.

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

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

Prediction 2a. Effect on wages. *Wages will adjust downward. The expression for the proportional change in wages for previously non-compliant firms is given by:*[16]

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

Prediction 2b. Effect on total employment. *Employment may increase following enforcement only if firms have labor market power over amenities, i.e. $\beta_a > 0$ and $\theta_j < \infty$.*

In Section D.2.2, we derive the proportional change in employment which is characterized by the expression in Equation (4). Employment will rise when for the newly complying firm it is optimal to *not* fully offset the value of the improvement in working conditions after enforcement ($\Delta\%a_j$) with a lower wage ($\Delta\%w_j$).

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

The finding that the employment effect of enforcement is ambiguous may seem surprising, since higher enforcement raises firms' costs and would therefore reduce employment in a competitive

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

setting. In the monopsonistic environment considered here, however, firms choose a wage–amenity bundle $\{w, a\}$ that lies below the competitive outcome. As a result, employment is constrained by labor supply rather than labor demand (Lagos, 2026). In other words, equilibrium employment is pinned down by the monopsonistic firm's upward-sloping labor supply curve evaluated at the optimal wage–amenity bundle.[17] Therefore, as long as the mandated amenity level a_{min} is not high enough to push the firm to the competitive allocation, the effect of a_{min} on total employment is determined by its effect on labor supply defined in (2). The employment effect depends on how workers value the increase in amenities relative to the decline in wages. If the wage decrease is sufficiently small, overall job value may rise, increasing labor supply and, consequently, firm employment.

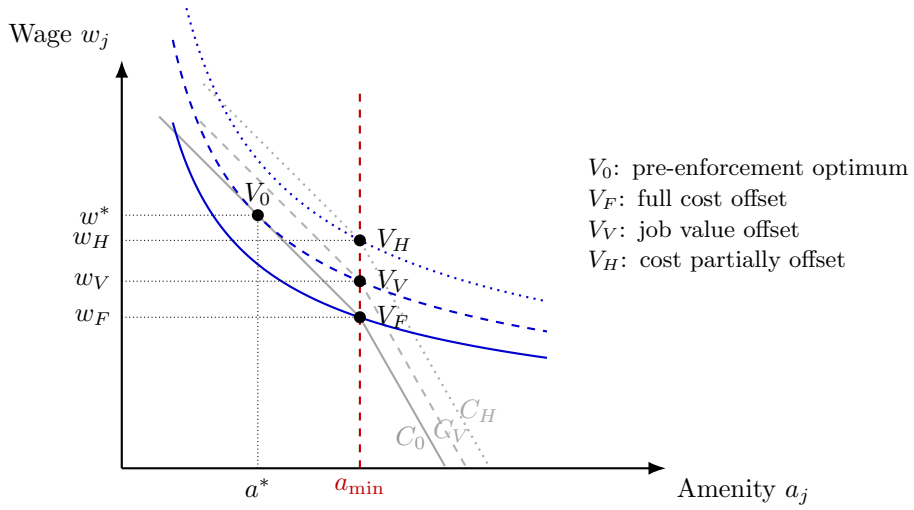
To understand how wages adjust in response to a higher a_{min}, recall that imposing a_{min} on a previously non-compliant firm distorts the firm away from its privately optimal relationship between w_j and a_j defined by (19). In response to the mandatory provision of a_{min}, the firm can react in three ways, which we report for illustrative purposes in Figure 1. First, the firm can keep average costs per worker unchanged by fully offsetting the higher amenity with lower wages. This distorted compensation bundle lowers job value for workers, implying lower employment. The point V_F illustrates this point in comparison to the pre-enforcement optimum V_0. Second, the firm can adjust wages such that V_j remains constant, resulting in the same labor supply as before enforcement. By revealed preferences, this implies weakly higher average costs per worker (and thus total costs as employment stays constant) for the firm. In Figure 1, this corresponds to the point V_V, which delivers the same job value as V_0 but lies on a higher average cost curve per worker, C_V, relative to C_0. Third, the firm may find it optimal to offset only part of the higher amenity costs through lower wages, thereby increasing average costs per worker to C_H, the value of the job to V_H, and consequently employment.

Which of these cases arises as the optimum under constrained compliance depends on the firm's trade-off between the benefit of attracting additional workers through a higher job value (V) and the increase in labor costs. The intuition for the possibility that the third case is optimal under constrained compliance comes from the imperfect substitution between wages and amenities under the Cobb-Douglas specification in the labor supply function. Once enforcement raises amenities above the firm's pre-enforcement level ($a_{min} > a_j$), a given increase in wages generates a larger increase in labor supply than before enforcement because wages are now attached to a higher-amenity job. This creates upward pressure on the optimal post-enforcement wage and reduces the incentive to fully offset the higher amenity costs through lower wages. As a result, the firm may optimally increase total job value, which raises labor supply to the firm and increases employment.

We provide an analytical proof to this result in Section D.2.2. The underlying mechanism behind the proof is that, when labor supply is increasing in amenities a_j, an increase in a_j driven by enforcement of minimum working conditions (a_{min}) can reduce the firm's marginal cost of labor, even though average labor costs increase. Since employment decisions depend on marginal rather than average costs, a firm may optimally expand employment when forced to comply with $a_{\min}$.

[17] A monopsonistic employer chooses employment by equating marginal cost and marginal revenue ($MC = MR$). Since hiring an additional worker requires improving the compensation bundle offered to all workers, the marginal cost of labor exceeds the wage–amenity bundle paid to the marginal worker ($MC > w + a$). Consequently, at the equilibrium wage–amenity bundle, the firm would be willing to hire additional workers, but labor supply is exhausted at that compensation level.

Figure 1: Wage and Amenity Adjustments Following Enforcement of Minimum Amenities



Notes: The figure illustrates possible wage-amenity adjustments following enforcement of a minimum amenity requirement $a_{\min}$. Worker utility is given by $V = (a^{\beta_a} w^{\beta_w})^{\theta}$, while firm iso-average cost curves are given by $C = w + (k - p\phi)a + p\phi \max\{a, a_{\min}\}$. V_0 and C_0 are the value of employment and the iso-average cost curve at the initial equilibrium where the firm is non-compliant, and satisfies the equality in Equation 19. V_V denotes the combination of $\{a, w\}$ required to keep worker utility unchanged post-enforcement, and the corresponding iso-average cost curve. The point V_F illustrates the full wage-offset case, in which compliance costs are fully shifted onto workers through lower wages. The point V_H corresponds to the case where the firm only partially offsets the rise in a with lower w, leading to higher V_j and a higher iso- average cost curve.

This mechanism is analogous to the standard monopsony model where the firm only sets worker wages, where a binding minimum wage can increase employment by reducing marginal labor costs (Manning, 2004).[18]

Section D.3 presents the model predictions under alternative assumptions: First, in Section D.3.1, we assume that labor supply responds only to wages ($\beta_a = 0$), as in a standard monopsony model (Card et al., 2018). Second, in Section D.3.2, we assume markets are perfectly competitive and firms take the value of a job as given, consistent with the theory of compensating differentials (Rosen, 1986). We show that in both cases the enforcement of benefits has an unambiguously negative effect on employment. Third, in Section D.3.3, we show that under the assumption that firms take wages as given and possess only amenity-setting power, enforcement can lead to positive effects on employment similar to our benchmark case. Therefore, observing a zero or positive employment effect from enforcement empirically suggests that firms have labor market power over amenities.[19]

In Section 5,we empirically test Prediction 1 by analyzing the firm-level characteristics that correlate with being found violating workplace regulations. In Section 6, we empirically assess Prediction 2 by estimating the effect of inspections on firm and worker outcomes. We show that

[18] An alternative proof for this result is provided in Chapter 8 of (Manning, 2003), which considers the special case of a monopsonistic employer choosing both wages and a workplace (dis)amenity in the form of worker effort.

[19] Predicion 1 and 2b in this model remain under a more general constant elasticity of substitution (CES) labor supply function which allows for varying degrees of complementarity between w_j and a_j, as in Dube et al. (2022). Under this CES specification, the prediction for wages becomes ambiguous, as the effect of enforcement on wages can even be positive when wages and working conditions are sufficiently strong complements for workers. In this case, inspections improve both wages and working conditions, leading unambiguously to an increase in labor supply and thus in firm employment.

both sets of results allow us to rule out both models of perfectly competitive labor markets and monopsony models in which labor supply responds only to wages.

3 Institutional Context and Data

3.1 Workplace Inspections in Mexico

The Mexican Secretariat of Labor and Social Welfare (STPS) conducts firm inspections to monitor and enforce compliance with workplace regulations (STPS, 2014). Most inspections are conducted by state-level branches of the STPS and generally focus on compliance in three main areas: health and safety regulations, training obligations, and general working conditions.[20]

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

This study focuses on ordinary inspections, for which firm selection follows a stratified random process. Throughout this paper, we use the terms "ordinary" and "stratified random" interchangeably. Each state-level STPS branch organizes firms listed in the National Firm Directory (DNE) into predefined strata, from which firms are randomly selected for inspection on a monthly basis. To ensure transparency, this selection process is conducted through the Labor Inspections Administration System (Sistema de Administración de la Inspección del Trabajo, or SIAPI) (STPS, 2015). These groups are stratified based on sector (25 categories), total employment (above and below 15 workers), years since last inspection (≤ 1, $(1,2]$, $(2,3]$, >3), firm age (<1 , $[1,3]$, >3), and risk class (5 categories).[21] This randomized approach provides a quasi-experimental setting for analyzing the effects of workplace inspections on compliance and firm-level outcomes.

If a violation is found during an inspection, labor inspectors grant employers deadlines ranging from 30 to 90 business days to correct identified non-compliance issues and to provide necessary documentation. An extension of the deadline may be granted once for an equivalent period, upon written request submitted prior to the deadline, provided that worker safety is not compromised.

[20] Examples of health and safety regulations include, for instance, compliance with safety procedures for maintenance of installations, having a grounding system to protect workers against electrical hazards, having a fire-emergency plan. Training obligations refer to providing proof of training provision and training plans, which is mandatory by Mexico's Federal Labor Labor (LFT, 2021). Examples of general working conditions include providing formal work contracts, the minimum wage, the 13th salary (aguinaldo), mandatory vacation days, maximum working hours, mandatory breaks during the workday, payment of extra hours, and mandatory profit-sharing (STPS, 2013).

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

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

3.2 Datasets

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

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

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

[23] While our main dataset does not contain information on fine amounts, we link an auxiliary dataset covering 1,661 fines issued between 2023 and 2025 to firm size. Firms receiving fines are large, with an average size of 2,041 workers and a median size of 86 workers in the quarter preceding the inspection. We find that each additional worker is associated with an increase of approximately 56 Mexican pesos (about USD 3) in the fine amount ($p < 0.001$).

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 effectively functions as a monthly census of large, productive manufacturing establishments in Mexico, resulting in a balanced panel of around 9000 establishments in our study.[24] Appendix Table A.1 reports summary statistics of this survey.

Additional Establishment Surveys. To complement the EMIM, we utilize two additional datasets collected by Mexico's national statistical office (INEGI). First, the Annual Survey of Manufacturing Establishments (*Encuesta Anual de la Industria Manufacturera*, or EAIM) provides yearly information for the same set of firms as EMIM not captured in EMIM, such as total annual revenues, total production costs, 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 2018 Economic Census (published in 2019), which provides a comprehensive snapshot of all 4.6 million business establishments operating in Mexico that year, including small and informal firms across all sectors. Conducted every five years, the census contains rich information on firm-level characteristics such as revenues, value added, profits, investments, capital stock, workforce composition (including informal employees), and social security coverage.[25] In our main analysis, we restrict the Economic Census sample to manufacturing establishments with more than 15 employees.

Administrative Employer-Employee Data (IMSS). Our second monthly establishment dataset consists of administrative records from the Mexican Social Security Institute (*Instituto Mexicano de Seguridad Social*, IMSS), accessed through the *Econlab* at Banco de México. It covers the universe of formal employment relationships in Mexico and is structured as a monthly panel, containing identifiers for firms, establishments, and workers. The dataset provides detailed information on each establishment (sector, municipality, and age) and each worker (gender, age, earnings, and contract type—permanent or temporary) from January 2004 to December 2023. Earnings are measured using the worker's average daily taxable income (*salario base de cotización (SBC)*) per month, 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).[26] Earnings are bottom-coded at the minimum wage and top-coded at 25 UMAs (Units of Measure and Update).[27] This dataset does not contain separate information on hours worked or days worked. Accordingly, our analysis focuses on changes in average daily earnings reported to IMSS, while we use INEGI survey data to analyze changes in hours worked separately. 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

[24] The statistical office (INEGI) does not 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.

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

[26] 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.

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

analyses, as discussed in Section 6.3. 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 C. Appendix Table A.2 reports summary statistics of this constructed sample.

Accident Data. We also leverage data on the total number of accidents per municipality, month, and sector, provided by the Sistema de Avisos de Accidentes de Trabajo (SIAAT) and STPS. The dataset covers the period from January 2016 to April 2024, containing approximately 594,000 accident reports, with an average of more than 5,000 accidents per month (see Appendix Figure A.2a). 67% of accidents happen at the workplace, while 33% are commuting accidents. 250,000 of all accidents (42%) occur in the manufacturing sector. We use the number of accidents as one of several proxies for compliance with workplace regulations in Section 6.2, examining how inspections influence accident rates over time. We provide more descriptive results on accidents in Mexico in Appendix Figures A.1 and A.2.

Data from the National Survey on Regulatory Quality and Government Impact on Firms. We complement our analysis with the 2020 wave of the ENCRIGE survey to provide descriptive evidence on firms' experiences with labor regulations and inspections. ENCRIGE is a nationally representative firm survey conducted by INEGI, designed to collect detailed information on firms' interactions with government regulation, public services, and authorities. It includes questions on regulatory burden, compliance costs, inspections, corruption, and firms' interactions with administrative procedures, as well as broader assessments of the business environment. Importantly for our purposes, the survey contains a dedicated module on firms' experiences with labor inspections carried out by STPS, including information on enforcement costs and compliance effects of inspections. In our analysis, we restrict the sample to manufacturing establishments with more than 15 employees.

Merging Datasets. A key step in constructing our main datasets involves merging inspection records with both establishment-level survey data and social security administrative data. To our knowledge, this is the first study to link administrative data on workplace inspections with detailed establishment survey data. To carry out this merge, we first match inspection logs with INEGI's National Statistical Directory of Economic Units (DENUE), a registry of all economically active units in the country, maintained through biannual on-site verification. Using unique legal firm names (*razón social*), we successfully match 77% of inspection records to DENUE. The DENUE contains a unique firm identifier used by INEGI (CLEE), which enables the link to the establishment-level survey data. In the survey data, we aggregate establishments at the firm level in each state, which is the level at which we link workplace inspections. For the remainder of this paper, the term *firm* in the context of INEGI survey data refers to the firm-state level.[28]

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

[28]We derive similar results when using establishment-level data or restricting the sample to firms with a single establishment per state (Appendix Figure A.16).

enables linkage to administrative employer-employee-wage records from IMSS.[29] In Table C.1, we show that the matched sample closely resembles the full inspection dataset in terms of inspection type, topic, and year. Table 1 reports the number of matched inspections in the EMIM, Economic Census, and constructed IMSS datasets. Across all three datasets, roughly one quarter of inspections are random, while about one fifth of inspections result in a violation. In our two main samples (EMIM and IMSS), as firms tend to be larger, they are more likely to be inspected in comparison to all establishments in the Economic Census. We discuss the relationship between inspection probability and firm characteristics in detail in the next section.

Table 1: Summary Statistics Inspections 2017-2022

Dataset	# firms	# firms inspected	# firms random insp	# inspections	% random inspections	% violations
Economic Census	4 628 822	36 171	13 445	124 177	23	23
Manuf. Survey EMIM	9 109	4 853	2 558	23 788	26	21
Manuf. Admin IMSS	9 416	5 770	2 863	23 052	23	20

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

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. A key feature of our setting is that we know which characteristics STPS conditions on during the randomization process of ordinary inspections, and we observe these characteristics in our microdata. Therefore, we can assign each firm in our data to a randomization group g, defined by its state and the firm characteristics described in Section 3. This allows us to estimate the average experienced 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.

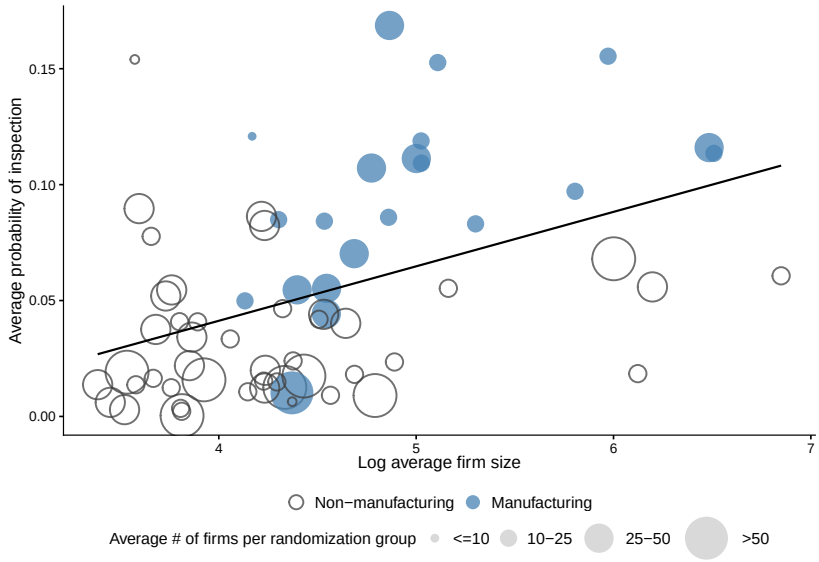
Appendix Figure A.5 displays the distribution of inspection likelihood across randomization groups between 2018 and 2021 for all firms in the Economic Census.[30] We observe substantial

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

[30]We calculate the probability of inspection in a group in a certain year as the likelihood of being inspected at

variation in inspection likelihood across randomization groups, suggesting that certain types of firms are more frequently targeted. Figure 2 provides further evidence on the targeting of ordinary inspections across randomization groups by plotting average estimated inspection probabilities across three-digit NAICS sectors. Three patterns emerge. First, larger firms are more likely to be selected for an ordinary inspection, consistent with the notion that firms' expected costs of non-compliance increase with firm size (Ulyssea, 2020). However, inspection probability also varies conditional on firm size, reflecting that randomization groups are defined based on additional firm characteristics. Second, there is no clear relationship between inspection probability and the average number of firms in a randomization group, suggesting that STPS does not simply allocate a fixed number of inspections to each stratum. Third, manufacturing subsectors exhibit particularly high inspection rates, even after accounting for differences in average firm size. These high inspection rates could be explained by STPS targeting specific sectors, as the manufacturing sector records the highest total and per-worker accident rates (see Appendix A.2). 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.

Figure 2: Inspection Probability, Firm Size, and Randomization Group Size across Sectors



Notes: This Figure shows the share of firms inspected between 2018 and 2019 in different 3-digit NAICS Economic Sectors, against the average firm size in each sector. The size of each dot indicates the average number of firms per randomization group in each sector. Blue circles correspond to sectors in manufacturing. White circles include all other sectors. The figure is built using data on firms with at least 15 workers from the 2018 Economic Census.

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

least once in the year, which is built as one minus the probability of not being inspected in any month of the year $= 1 - \left(1 - \frac{n\ monthly\ insp}{N_g}\right)^{12}$, where N_g is the number of firms in the randomization group and $n\ monthly\ insp$ is the average number of inspections carried out to firms in the randomization group each month. This probability measure accounts for the possibility of being inspected more than once. This estimated probability of inspection is strongly correlated across time. The correlation between $\hat{p}$ in 2018 and 2017 is 0.76, and between 2018 and 2019 it is 0.6.

stratified randomization process via the following univariate regression:

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

where $y_{j,t-1}$ corresponds to a characteristic of firm j in the previous month, and $inspection_{jt}$ is an indicator variable equal to 1 if the firm receives an ordinary inspection at date t (month-year). We estimate two different specifications, where $\phi_{x,t}$ is either a state-by-date fixed effect, or a stratified randomization group-by-date fixed effect (randomization group is defined according to firm characteristics in t-1). We compare the coefficient β across both specifications to evaluate the validity of the randomization. This procedure is analogous to a traditional means balance test between treatment and control groups in a stratified randomized controlled trial, but it explicitly accounts for the staggered timing of treatment in our design.[31]

Table A.3 reports the results for the monthly manufacturing survey, social security data and the economic census. We document significant differences between inspected and non-inspected firms when controlling for only state-by-date fixed effects (Panels A1, B1, C1). Specifically, inspected firms tend to be larger, employ fewer outsourced workers, exhibit lower average hours worked per worker, higher hires and separations, and a higher average wage. In contrast, Panels A2, B2, and C2 show that once randomization group fixed effects are included, these differences largely disappear and lose statistical significance.[32] 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.[33] As a placebo test, Appendix Tables A.4 and A.5 replicate the analysis for non-random inspections in the monthly manufacturing survey and economic census. 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.

5 Characteristics of Violating Firms

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

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

[32] The panels with randomization group fixed-effects show a 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 coefficients being closer to zero.

[33] We observe the same patterns when focusing exclusively on *first* ordinary inspections, which form the core of our causal analysis in Section 6

help predict observed violations.

Between 2018 and 2019, approximately one out of every four to five (ordinary) inspected firms in the manufacturing survey are found to be in violation. Figure A.7 shows that violation rates are highest in inspections related to safety and health, and above 20% in almost all sectors within manufacturing. The analysis of the correlation between firm characteristics and violations presents two empirical challenges. First, our sample of observed violations is drawn from the subset of firms that underwent inspection. Thus, there is potential selection bias when estimating the unconditional relationship between firm characteristics and violation rates (Heckman, 1979). Second, as shown in Section 2, a firm's decision to violate workplace regulations depends not only on its own economic characteristics, such as productivity and labor market power, but also directly on its (perceived) inspection probability. Consequently, when analyzing the determinants of non-compliance, it is important to consider that certain characteristics can directly and indirectly influence the firm's decision to violate through the probability of inspection.[34] To address both challenges, we control for each firm's estimated probability of inspection, described in Section 4. This approach allows us to isolate the relationship between firm characteristics and violation rates while accounting for differential inspection probabilities. This approach additionally allows us to assess the correlation between inspection probability and violation decisions. In particular, we run the following regression:

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

where $violation_{jgt}$ is an indicator for whether the firm is found violating workplace regulations during a stratified random inspection conducted between 2019 and 2021, $X_{j,2018}$ are pre-determined firm characteristics from the 2018 Economic Census, and $\hat{p}_{g,t-1}$ is the firm group's predicted probability of inspection before realized inspection.[35] We correlate a rich set of predetermined firm characteristics from the 2018 Economic Census with the probability of being found violating during an ordinary inspection conducted after the Census. The 2018 Economic Census provides three key advantages over EMIM and IMSS for this analysis. First, it covers the universe of formal and informal establishments, enabling a more accurate measurement of local labor market characteristics. Second, it provides a more comprehensive set of firm characteristics. Third, it allows for mild data restrictions, allowing us to focus on all relatively large establishments with firm size above 15 in the manufacturing sector.

Given the large number of potentially correlated pre-determined firm characteristics to include in $X_{i,2018}$, we use the structure of the theoretical framework from Section 2 to guide variable selection. Specifically, we include empirical proxies for the determinants of non-compliance highlighted in Prediction 1: firm productivity (z_j), workers' relative valuation of working conditions (β_a/β_w), labor supply elasticity (θ), and inspection probability (p). We include additional firm and labor market characteristics as controls, selected using a post-double selection Lasso procedure following

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

[35] The results are robust to estimating the relationship between firm characteristics and violation probabilities using a probit model.

Belloni et al. (2014).[36] All explanatory variables $X_{j,2018}$ are standardized to have mean zero and standard deviation one.[37] Table 2 reports the results. Columns 1–4 use as the outcome an indicator for whether a violation is detected, while Columns 5–8 focus on a stricter measure equal to one if a violation is detected and a sanctioning process is initiated, capturing more severe non-compliance. We report coefficients for each model-based proxy and indicate their mapping to the model parameters. We consider two alternative sets of proxies and report specifications with and without Lasso-selected controls.[38]

The estimates provide empirical support for Prediction 1 of the model. Among inspected firms, those with lower profits and lower value added per worker, our proxies for productivity, are significantly more likely to be found in severe violation followed by sanctions. This is consistent with Prediction 1, which links lower productivity to higher non-compliance.[39] To proxy workers' relative valuation of working conditions, β_a/β_w, we use the share of female workers in the establishment, following evidence that women place relatively greater weight on non-wage job amenities (Maestas et al., 2023; Faberman et al., 2026). Consistent with the model's prediction, Table 2 shows that inspected firms with a higher share of female workers are significantly less likely to violate labor regulations. A one standard deviation increase in the share of female workers—roughly equivalent to moving from the median firm to the 75th percentile of the female-share distribution—is associated with a 3.5 percentage point (14%) reduction in the probability of being found non-compliant and an approximately 3 percentage point (20%) reduction in the probability of receiving both a violation and a sanction. We next assess proxies for labor market power, measured by local labor market concentration (HHI) and a firm's labor market share, as employer market power has been shown to increase with labor market concentration and employer dominance (Berger et al., 2022; Amodio et al., 2025). Consistent with the first model prediction, firms operating in more concentrated labor markets or holding a larger local labor market share are significantly more likely to violate labor regulations. Specifically, a one standard deviation increase in either local labor market concentration or firm labor market share is associated with an approximately 2 percentage point (8%) increase in the probability of non-compliance and a 2.5 percentage point (17%) increase in the probability of severe non-compliance.[40] This result is also consistent with evidence in Marinescu et al. (2021), who document that local industries with higher violation rates exhibit greater concentration.

We do not find a statistically significant relationship between the predicted probability of inspection and violation outcomes. One interpretation is that our measure of inspection probabil-

[36] Post-double selection Lasso is run separately for the different sets of model proxies, therefore the set of controls varies depending on the model proxies used. The full set of lasso controls for the violation regressions is: total workers, firm age, export share, total capital, multinational status, total profits, average worker age, share of informal workers, share of workers with higher education, average wage, average hours worked. All regressions use heteroskedasticity-robust standard errors, following Belloni et al. (2014), who show that standard OLS inference remains valid after variable selection using Lasso.

[37] The estimated inspection probability is not standardized.

[38] We report two representative combinations of the available proxies. Results are robust across alternative specifications using the full set of nine proxies.

[39] The coefficients on value added per worker and revenue per worker become statistically insignificant once the full set of controls is included. This likely reflects their strong correlation with controls selected by the post-double-selection lasso, such as capital and total profits.

[40] Equivalently, moving from the 50th to the 90th percentile of the HHI and labor market share distributions increases the probability of non-compliance by 11% and 8%, respectively.

ity—constructed from realized inspections within randomization groups—may differ from firms' perceived enforcement risk. Measurement error in this proxy would attenuate the estimated coefficient. Nevertheless, the estimated coefficients are consistently negative across all specifications, in line with the model's Prediction 1 that higher inspection probabilities reduce non-compliance.

Table A.6 shows that the results remain robust when controlling flexibly for quintiles of inspection probability fixed effects, rather than a continuous probability control. This specification also shows that, relative to firms in the lowest inspection-probability quintile, firms in the fourth quintile are significantly less likely to be found in violation during an inspection. Columns 2, 5, 8 and 10 show that results are robust to controlling for sector and state fixed effects. In columns 3 and 6, we use propensity score re-weighting (Hirano et al., 2003) instead of linear controls for inspection probability, yielding similar results.

Table 2: Correlations between model-selected firm characteristics and violation probability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep Var:	Violation				Violation + Sanction			
Proxies for z_j								
VA per worker	-0.033	0.025			-0.058**	0.006		
	(0.054)	(0.070)			(0.027)	(0.024)		
Profits per worker			-0.039	0.034			-0.064**	0.009
			(0.064)	(0.084)			(0.029)	(0.029)
Proxy for β_a/β_w								
Share women	-0.030**	-0.034***	-0.032***	-0.036***	-0.025***	-0.029***	-0.028***	-0.032***
	(0.010)	(0.011)	(0.010)	(0.011)	(0.009)	(0.010)	(0.009)	(0.010)
Proxies for θ_j								
LLM Herfindahl Index	0.021**	0.020**			0.026***	0.026***		
	(0.010)	(0.010)			(0.009)	(0.010)		
Firm Market Share in LLM			0.021**	0.022**			0.026***	0.027***
			(0.009)	(0.009)			(0.009)	(0.009)
Proxy for p								
Estimated inspection prob.	-0.078	-0.072	-0.081	-0.079	-0.050	-0.052	-0.054	-0.059
	(0.050)	(0.052)	(0.051)	(0.052)	(0.046)	(0.047)	(0.047)	(0.048)
Double-Lasso controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,997	1,997	1,997	1,997	1,812	1,812	1,812	1,812

Notes: This table shows the coefficients of multivariate regressions of a binary variable indicating whether an inspected firm was found violating on empirical proxies for model parameters defined in Section 2. In Columns 1 to 4, the outcome variable is whether the firm was found in violation. In Columns 5 to 8, the outcome is 1 if the firm was found violating and a sanction procedure was initiated (firms violating without a sanction procedure are excluded from this estimation). Independent variables are standardized. The regressions are estimated using a sample of manufacturing firms from the 2018 Economic Census with more than 15 workers that received an ordinary inspection between 2019 and 2021. Firm characteristics correspond to the values measured during the 2018 Economic Census. Columns 2,4,6,8 include control variables selected via post selection double lasso. The mean of the outcome variable is 0.24 for Columns 1-4 and 0.15 for Columns 5-8. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

To further explore firm characteristics that correlate with non-compliance, Appendix Table A.7 reports univariate regressions of Equation (6) using a broader set of firm characteristics. We group explanatory variables into firm characteristics, workforce composition, and local labor market conditions, defined at the municipality-by-2-digit NAICS level. In addition to the model-selected variables, we identify a set of additional firm characteristics that are systematically correlated with violation probability among inspected firms. Firms with a lower share of workers receiving 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 workplace regulations. Furthermore, firms that rely more heavily on outsourcing are more likely to be found in violation, potentially because outsourcing facilitates avoidance of labor benefits, and has also been shown to correlate with high monopsony power (Casco et al., 2024; Estefan et al., 2024).[41] This correlation also relates to Prediction 1 that firms with higher labor market power are more likely to be non-compliant with workplace regulations. Violating firms do not appear to provide higher wages to workers, and firms with severe violations pay, on average, even lower wages. Furthermore, the positive relationship between labor market concentration, labor market share, and violation rates persists when labor market concentration and labor market shares are measured using firms' wage bills instead of total workers. Finally, neither a firm's share of own informal workers nor the share of informal workers in its local labor market is significantly correlated with the probability of being found in violation. This finding provides further evidence that labor violations among large manufacturing firms are not primarily associated with informality. Instead, they appear to reflect poor working conditions among formally employed workers.

Finally, as a complementary approach, we implement a data-driven variable selection procedure using Lasso regularization, without an ex-ante selection of proxies for our model variables. We then include the empirically selected variables in post-Lasso OLS regressions that control for inspection probability.[42] Figure A.8 reports the results. Variables selected by Lasso and significantly associated with violation outcomes include the share of outsourced workers, training intensity, average wages, the share of female workers, and local labor market share. Notably, several of these variables overlap with those motivated by the theoretical framework, indicating that the model-based approach captures empirically relevant determinants of non-compliance.

Overall, these findings align with the theoretical framework presented in Section 2, which emphasizes the importance of firm productivity, worker preferences, inspection probability, and labor market power in influencing firms' decisions to provide bad working conditions. Next, we empirically assess the causal impact of enforcing workplace regulations on firms and workers, corresponding to a test of Prediction 2 from our theoretical framework.

6 Effect of Workplace Inspections

6.1 Empirical Strategy

In our main reduced-form analysis, we estimate the causal effect of first-time stratified random inspections on firm outcomes using a staggered difference-in-differences design. The treatment is random, stratified by firms j's group in the month prior to inspection ($g = g(t-1)$). We apply the local projection difference-in-differences (LP-DID) approach (Dube et al., 2025). This methodology has two features that are particularly relevant for our setting. First, it allows for the estimation

[41] This period in which firm characteristics are measured (2018) precedes the 2021 outsourcing ban, meaning that outsourcing in itself was not a violation at the time.

[42] The post-Lasso OLS estimator achieves similar convergence rates as Lasso while reducing shrinkage bias (Belloni et al., 2010).

of causal effects in a staggered treatment setting. Second, unlike standard event-study TWFE specifications or most alternative estimators proposed in the recent literature, LP-DID allows us to directly control for pre-treatment values of time-varying covariates. This feature is crucial in our context because it enables us to control for the randomization group in the month prior to inspection, which varies over time as years since the last inspection, firm age, and firm size change. An additional advantage of this approach is its computational efficiency, which is particularly important in our setting given the large number of randomization-group fixed effects.

We estimate separate regressions for $h \in [-H_{min} : -2] \cup [0 : H_{max}]$:

$$y_{j,t+h} - y_{j,t-1} = \beta^h \Delta D_{jt} + \gamma^h_{t,g_{(t-1)}} + e^h_{jt}, \tag{7}$$

where H_{min} and H_{max} denote the maximum number of pre- and post-inspection periods included in the estimation. $y_{j,t+h}$ is the outcome of firm j at month-year $t+h$ and g is the randomization group of firm j at $t-1$. $D_{jt} = 1$ marks post-treatment periods, and $\Delta D_{jt} = 1$ indicates the month of the first stratified random inspection. Thus, β^h can be interpreted as the effect of a first ordinary inspection in t on the outcome in $t+h$. The parameter $\gamma^h_{t,g_{(t-1)}}$ is a date by randomization group fixed effect, where the firm's randomization group $g_{(t-1)}$ is defined according to the firm's characteristics in t-1.[43] Following Dube et al. (2025), we restrict the sample to newly treated observations ($\Delta D_{it} = 1$) or clean controls—those not yet or never treated ($D_{j,t+h} = 0$).[44] The identifying assumptions of this approach are conditional no anticipation and conditional parallel trends (Dube et al. (2025), section 4.1), which, we argue, should hold after conditioning on randomization group fixed effects because of the random nature of the treatment.

One potential threat to identification is a violation of the Stable Unit Treatment Value Assumption (SUTVA), arising from spillover effects on control firms. For example, untreated firms may increase compliance with labor regulations when nearby firms are inspected. Several pieces of evidence suggest that this concern is limited in our setting. First, we do not find any effects on control firms located in municipalities with a high concentration of treated firms. Second, we find no evidence of spillover effects on establishments belonging to the same firm but located in different states. Third, the share of treated firms is relatively small in any given month, reducing the scope for spillovers.

We apply this LP-DID approach to the monthly manufacturing sample (EMIM) and the comparable monthly administrative employer-employee data (IMSS), and cluster standard errors at the firm level. The monthly manufacturing survey (EMIM) covers around 9,000 establishments each month, of which 980 receive their first random inspection between 2018 and 2019. The comparable

[43] 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.

[44] In most specifications, we use a more restrictive control group by excluding establishments inspected before H_{max}, therefore keeping the composition of the control group constant across time horizons h (see Section 3.6 of Dube et al. (2023)). For the yearly EAIM regressions we instead restrict the control group to establishments not inspected until the corresponding event time h, following the main specification in Dube et al. (2023), to increase power in these regressions, as the H_{max} restriction reduces statistical power at longer horizons. For pre-treatment horizons ($h < 0$), clean controls are also required to be untreated at $t-1$.

IMSS-based sample is a balanced monthly panel of 9,416 establishments observed between 2017 and 2020. Within this sample, we observe 1,069 first inspections conducted between February 2018 and December 2019.[45]

The similarity of the results presented in the following sections across two independent datasets strengthens the validity of the matching procedure and the robustness of our results. It also suggests that our findings are not an artifact of the exact EMIM sampling design, thereby increasing the external validity of our conclusions for large and productive firms in Mexico.

6.2 Effect of Workplace Inspections on Compliance

The Mexican Labor and Social Security Secretariat (STPS) conducts workplace inspections to check and improve compliance with labor standards. Hence, the effect of inspections on compliance is an outcome of first-order importance. Measuring compliance with workplace regulations is generally challenging. Much of the existing literature has relied on survey-based estimates of informality—a specific form of non-compliance—and a few studies focus on field experiments involving small samples of firms. While we cannot directly measure compliance, we present five types of evidence that firms increase compliance after being inspected: empirical evidence of follow-up inspections, the effects of inspection on training expenditures and on costs for maintenance of equipment, the relation between inspections and workplace accidents, and the relation between violation rates and number of inspections received. We additionally provide evidence on bribes and approximate compliance costs from the firm survey ENCRIGE.

Follow-up inspections. First, we analyze follow-up inspections, a specific type of inspection aimed at verifying compliance with workplace regulations after a violation is identified. Approximately 40% of inspections where a violation is found and firms are provided with additional time to fix their insufficient working conditions receive a follow-up inspection.[46] 82% of these follow-up inspections happen within 12 months of the inspection with the violation. These patterns suggest that follow-up inspections serve as a mechanism for the labor authorities to verify compliance after violations are detected.

Effect on training expenditures. Second, we assess the impact of inspections on worker training expenses, a direct and indirect proxy for compliance with workplace regulations. It is a direct measure as firms in Mexico are obliged by the Federal Labor Law (LFT, 2021) to provide annual training to their workers, and certain workplace inspections specifically check for compliance with this mandate. It is also an indirect proxy for compliance with workplace regulations, as better-trained workers should be more capable of safely operating machines in the manufacturing sector. We observe annual worker training expenses and apply our main empirical approach from equation (7) at the yearly level. Figure 3 depicts the effect of workplace inspections on firms' average training costs per worker and training costs over total labor costs. We observe that after an inspection firms

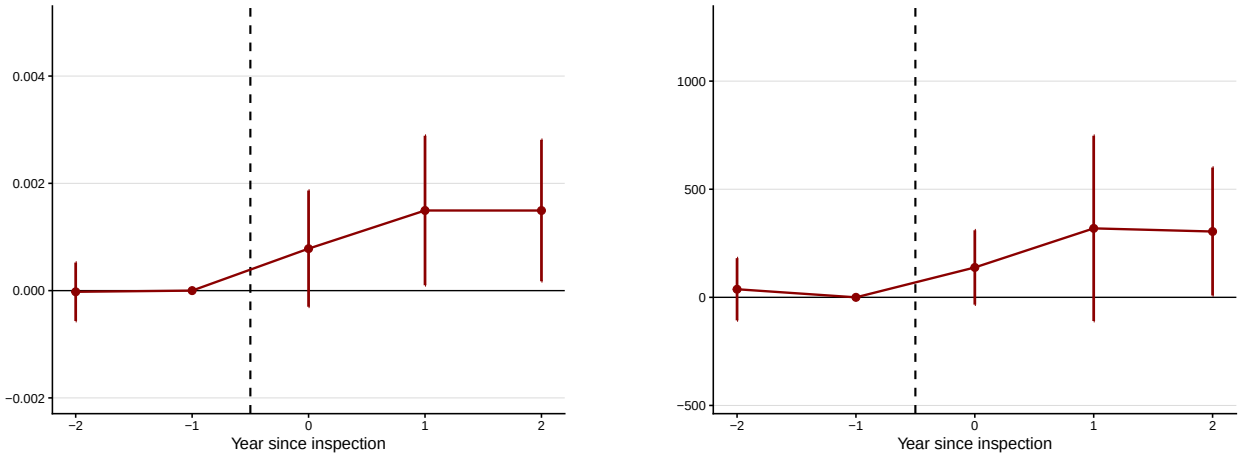
[45] We exclude inspections conducted in January 2018 from the IMSS analysis because employment levels in IMSS fluctuate substantially between December 2017 and January 2018. Combined with the high number of inspections conducted in January 2018, these fluctuations introduce considerable noise, as our outcomes are measured relative to the month preceding the inspection.

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

increase training costs as a share of total costs by approximately 0.2 percentage points on average (40% relative to the 2017 mean), and increase their training costs per worker by approximately 300 MXN (30% relative to the 2017 mean). This effect aligns with firms improving working conditions rather than just learning how to conceal poor practices.

Effect on maintenance costs. Third, we estimate the effect of inspection on firms' expenditures on repairs and replacement parts used for equipment maintenance. While this variable is not a direct measure of compliance, it is plausibly related to compliance, as maintaining machinery and production equipment is an important component of providing a safe working environment and complying with occupational safety regulations. Figure A.9 shows that inspections increase firms' total maintenance expenditures by approximately 5%. Estimated effects on maintenance expenditures per worker are also positive, at around 2.5%, although they are imprecisely estimated. These findings suggest that inspections induce firms to increase investments in the upkeep of production equipment, consistent with improvements in workplace conditions to ensure worker safety.

Figure 3: Effect of Inspections on Training Costs



(a) Training Costs over Total Labor Costs

(b) Training Costs per Worker

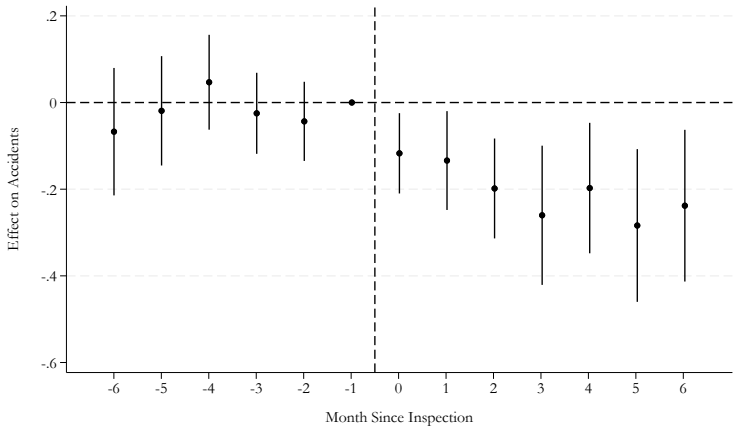
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the yearly level for $h \in [-2:2]$. The outcome variables are training costs as a percentage of total costs and training costs per worker. Regressions are estimated based on a balanced sample of firms 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.

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

$$WorkAcc_{m,t+h} - WorkAcc_{m,t-1} = \beta^h RandomInspections_{mt} + f(\hat{p}_{mt}) + \lambda^h_{t,a_q} + \delta^h_m + v^h_{mt}, \quad (8)$$

where δ_m^h captures municipality fixed effects, respectively, and v_{mt}^h is the error term. While we cannot control for randomization group fixed-effects as in Equation (7), we account for cross-municipality differences in inspection probability by estimating the average probability of inspection at date t for firms in municipality m, and controlling flexibly for a third-order polynomial in this average inspection probability $f(\hat{p}_{mt})$.[47] We additionally control for average accidents in the pre-period quintiles (a_q for $q \in [1:5]$) by time fixed-effects λ_{t,a_q}^h.[48] The coefficient β^h can be interpreted as the effect of an inspection in t on the number of accidents in $t+h$. The regressions are based on a balanced sample, considering ordinary inspections from January 2018 until June 2019.[49] We keep only clean controls, i.e., municipality-month observations without additional inspections until $t+6$.[50]

Figure 4: Relation Between Inspections and Work Accidents



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

Figure 4 depicts our main result. One more ordinary inspection in a municipality is associated with a decrease of around 0.2 (9%) accidents in this municipality compared to the pre-inspection month. The effect is statistically significant at the 5% level for all post-inspection periods, while the coefficients for periods before the inspection are not significantly different from 0. In Appendix A.11, we show that our results are robust to changing the set of controls, treatment definition, outcome, and considered time period.

Violations and past inspections. Fifth, we examine how the likelihood of being found violating decreases in the number of stratified random inspections a firm has undergone via the following regression:

[47] Inspection probability for municipality m, date t is constructed by first calculating inspection probability for each randomization group in each date using the same methodology as in Section 4, and then taking the average randomization group inspection probability across all firms in municipality m.

[48] This control helps us account for potential mean reversion in the number of accidents.

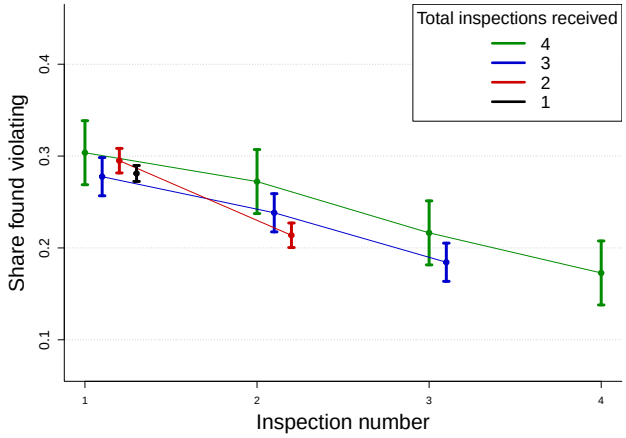
[49] In contrast to our main analysis, we do not consider inspections after June 2019 due to the pandemic.

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

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

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

Figure 5: Share of Firms Violating, by Inspection Number



Notes: This figure is constructed using firm-level data on stratified random inspected firms from 2017 to 06/2022. It displays the coefficients and 95% confidence intervals from regressing a binary variable indicating whether a violation occurred during inspection n on a variable indicating the number of stratified random inspections the firm has received up until inspection n. The sample is restricted to firms that received N ordinary inspections in total. Each color corresponds to a different $N \in [1:4]$. Standard errors are clustered at the firm level.

Figure 5 reports the estimated violation rates, showing the proportion of firms with a violation in their first, second, third, and fourth stratified random inspections (n), separately by the total number of stratified random inspections received (N). This allows us to compare coefficients within a group (same color) without different sample selections, as firms receiving a different number of total stratified random inspections (across colors) may differ. Reassuringly, all four groups have a very similar violation rate of around 30% during their first inspections. This probability of being found in violation of workplace regulations decreases in the number of inspections they have already received. For instance, a firm is 4 to 8 percentage points (13–26%) less likely to be found in violation during its second inspection compared to its first.[51]

Evidence on bribes and compliance costs. A potential threat to the effect of inspections on compliance arises if inspectors solicit bribes, allowing firms to circumvent regulatory requirements

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

during an inspection. To assess this concern, we use data from the 2020 firm-level survey on Regulatory Quality and Government Impact on Firm Activity (ENCRIGE), which collects information on firms' experiences during STPS inspections (among other regulatory interactions). Summary statistics for a sample of large manufacturing firms included in the survey are reported in Appendix Table A.8.

Reported bribery during inspections is rare. Only 3% of firms indicate that they were offered the possibility of bribing the inspection officer during an STPS labor inspection, and only 2% report that they actually paid a bribe.[52]

Table A.8 also provides evidence on compliance costs for firms. Firms report that the annual cost of complying with *all* laws and regulations is approximately 6,000 MXN per worker, which corresponds to about 5.5% of the average annual wage in our sample. In addition, firms report allocating approximately 3% of their workforce to activities related to regulatory compliance in 2020. These measures should be interpreted with caution, as they capture the cost of complying with *all* regulations—not only labor regulations.[53] Nevertheless, they provide a useful benchmark for the magnitude of regulatory burdens faced by firms. Finally, the table shows that only 0.8% of firms report being forced to close or having their license revoked following an STPS inspection, whereas the share of firms granted time to comply is ten times larger. These patterns are consistent with the STPS's stated objective of promoting compliance rather than imposing punitive sanctions.

Firm Exit. We next address the potential concern that our findings on compliance and firm outcomes in the subsequent section are driven by firm exit. If less productive firms unable to comply with workplace regulations are more prone to exiting the market after an inspection, this attrition could bias our sample and, consequently, our results. As depicted in Figure A.12, 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 but will eventually be inspected from the control group to avoid survival bias, as their continued operation is guaranteed until inspection. These findings are consistent with results by Samaniego de la Parra and Fernández Bujanda (2024) who show that labor inspections in Mexico do not affect firms' probability of survival in the formal sector, even among on average much smaller firms in their analysis.

Overall, the empirical results presented in this section further corroborate our earlier institutional analysis, indicating that inspections aim to promote compliance rather than generate revenue through penalties or induce firm closures.

6.3 Effect of Inspections on Firm-level Outcomes

6.3.1 Employment

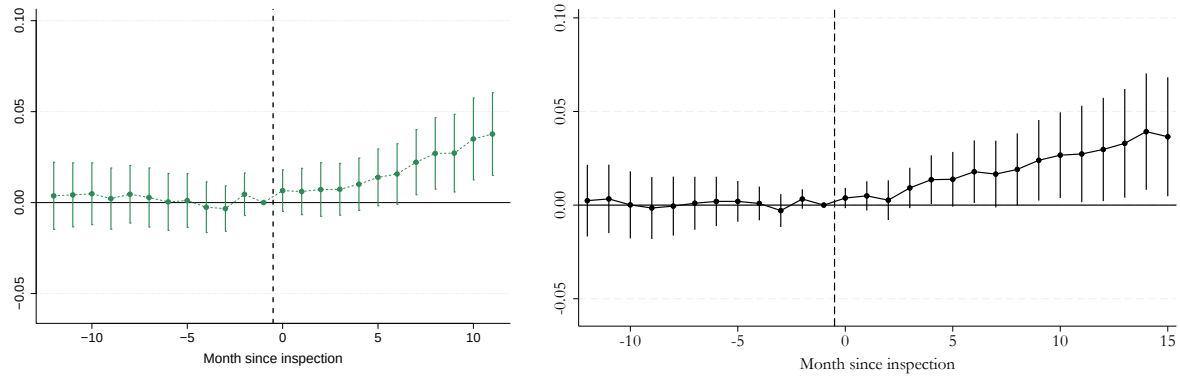
Having shown that inspections tend to improve compliance, we now turn to evaluating Prediction 2 from Section 2 by estimating the impact of inspections on firm-level outcomes, beginning with

[52] This low incidence is unlikely to reflect underreporting, as firms report substantially higher rates of bribery in other interactions with public officials. For example, more than 50% of firms report bribery in encounters with the police.

[53] They may also omit relevant components of compliance costs, such as worker compensation.

total employment. Figure 6 displays the estimated coefficients from a LP-DID model for log total employment, over a one year pre- and post-inspection horizon. Panel (a) shows that in the EMIM sample, inspected firms experience a statistically significant increase in employment of approximately 4% one year after the inspection. We observe very similar results in the IMSS data in Panel (b), where firm employment rises also by about 4% one year following an inspection.[54] Appendix Figure A.14 shows that this positive effect on employment persists up to two years post-inspection.[55]

Figure 6: Effect of Inspections on Employment



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

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$ in panel (a) and for $h \in [-12 : 15]$ in panel (b). The outcome variable is log total firm employment. Regressions are estimated based on a balanced sample of firms between 2017 and 2020 from EMIM in panel (a) and from IMSS in panel (b). Treated firms are those receiving the first ordinary inspection between January 2018 and December 2019 in EMIM or between February 2018 and December 2019 in IMSS. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

As shown in Figure 7a the increase is primarily driven by growth in blue-collar employment. In contrast, we find no statistically significant effect on white-collar employment. This is consistent with the focus of workplace regulation enforcement on areas such as safety, health, training, which predominantly affect blue-collar roles. Furthermore, Figure 7b shows that employment rises similarly for men and women, by about 4 percent. As discussed in Section 5, women may place greater value on improvements in working conditions. At the same time, these improvements are concentrated among blue-collar workers, who are more likely to be male. These two forces can rationalize the comparable total employment increases for both genders.[56]

Appendix Figure C.6 shows that a similar increase in blue-collar employment is observed when focusing exclusively on ordinary inspections related to safety and health. We do not find statistically

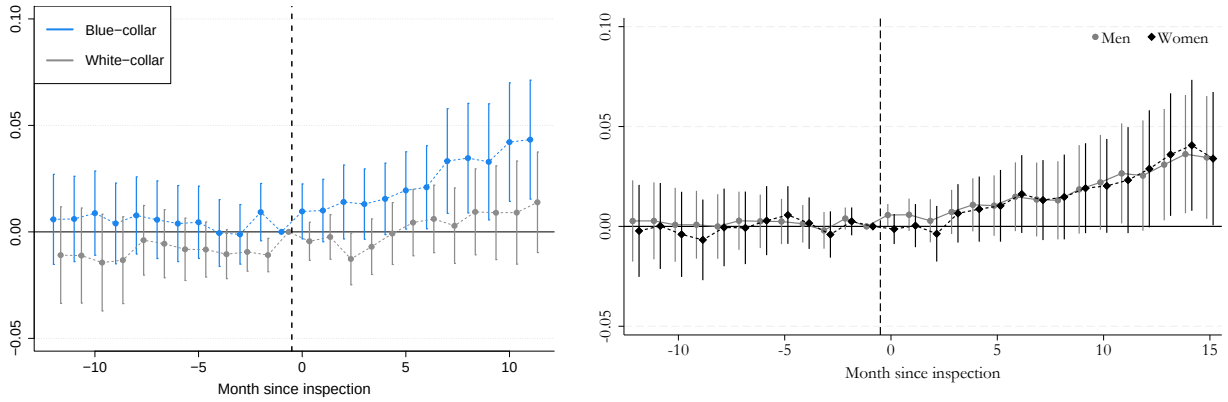
[54] Both results correspond to around 20 additional workers.

[55] Long-run estimates are estimated less precisely because the LPDID estimator restricts the comparison group at each horizon to firms that remain untreated through the end of that horizon. As the horizon increases, fewer treated firms have valid not-yet-treated controls observed far enough into the future. The effective estimation sample therefore becomes smaller at longer horizons, reducing statistical power.

[56] Appendix Figure A.25 shows that inspections have no effect on the share of outsourced workers. While this may appear at odds with the findings in Section 5, which documents a positive correlation between outsourcing and violation probability, both results are consistent with the institutional framework. Outsourcing likely facilitates the provision of substandard working conditions but was not itself illegal during this period.

significant increases in employment for inspections targeting mandatory training and general working conditions, although the magnitude of the effect for training inspections is comparable to that of safety and health inspections. Furthermore, we separately estimate the effects of inspections on the number of blue-collar workers for firms found violating workplace regulations and for those not found violating. As shown in Figure A.17 the positive employment effects are concentrated among firms found to be violating workplace regulations, which likely implement adjustments in response to the inspection.[57] These results are consistent with our finding in Figure A.7, which shows that violation rates are highest for safety and health inspections—areas that predominantly concern blue-collar working conditions.

Figure 7: Effect by Type of Employment and by Gender



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

(b) Log Men and Women (Monthly Admin Data)

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:11]$ in panel (a) and for $h \in [-12:15]$ in panel (b). In panel (a), the outcome variables are log total blue-collar workers and log total white-collar workers at the firm in EMIM. In panel (b), the outcome variables are log total men and log total women. Regressions are estimated based on a balanced sample of firms between 2017 and 2020 from EMIM in panel (a) and from IMSS in panel (b). Treated firms are those receiving the first ordinary inspection between January 2018 and December 2019 in EMIM or between February 2018 and December 2019 in IMSS. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

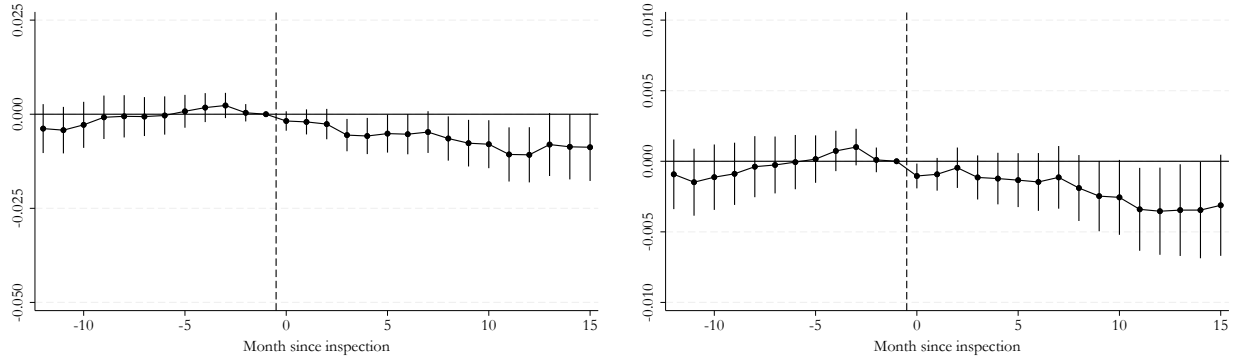
Appendix Figures A.15 and A.16 show that these results are robust to several alternative specifications: different aggregations on the firm- or establishment-level, extending the inspection period, restricting the sample to firms with only one establishment per state, and incorporating various sets of fixed effects within the EMIM sample.

Workforce composition. The observed increase in firm employment affects workforce composition at inspected firms. Figure 8a and 8b show a 1% decrease in average formal labor market experience and a 0.3% drop in average worker age. Both results are in line with an increased stock of blue-collar workers who are likely to have less labor market experience and to be younger than white-collar workers. Average firm tenure decreases by around 2.8%. This outcome reflects both changes in firms' hiring patterns following inspection and changes in the separation rates of workers

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

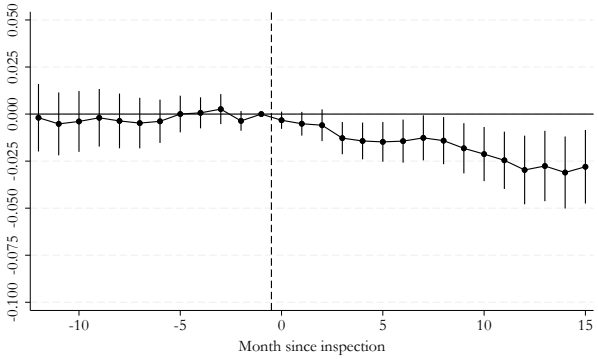
with different tenure profiles.

Figure 8: Effect on Workforce Experience, Age, and Tenure



(a) Log Average Formal Work Experience

(b) Log Average Worker Age



(c) Log Average Firm Tenure

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:15]$. In panel (a), the outcome variable is log average formal labor market experience at the firm level. In panel (b), the outcome variable is log average worker age at the firm level. In panel (c), the outcome is the log of average tenure among the firm's workforce. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019 in IMSS. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

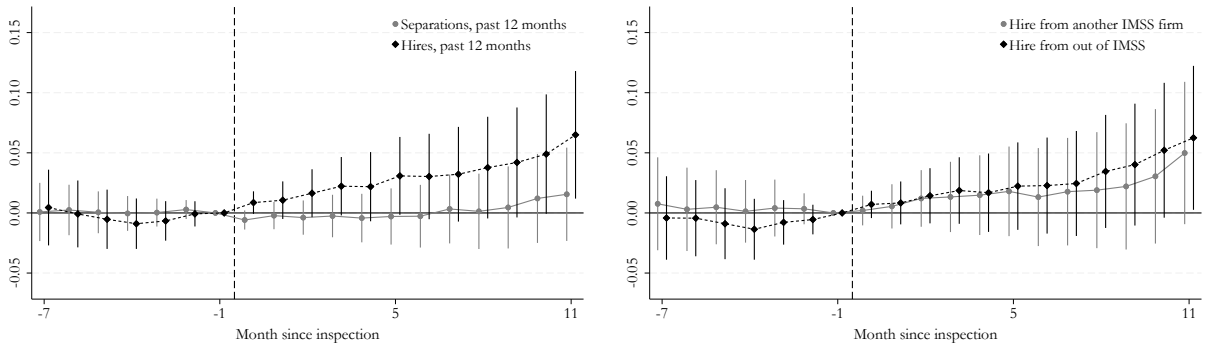
Hires and Separations. We next assess the extent to which the observed rise in employment is driven by changes in firm hires and separations. The analysis of hires and separations faces two key challenges. First, EMIM does not include information on hires or separations, which restricts our attention to the administrative IMSS sample. 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 construct separate 12-month cumulative measures of hires and separations, each covering the current month and the preceding 11 months. We present the estimated effects in Figure 9a.[58] Cumulative hires increase steadily after the inspection and are about 6 percent higher one year later, when the 12-month window captures only post-inspection months. By contrast, we do not find a

[58]The outcome as a cumulative sum smooths short-run fluctuations, but mechanically attenuates the estimated post-inspection response in the first months after the inspection, since the cumulative window still includes pre-inspection months.

significant change in separations.[59] Importantly, firm-level separation measures conflate changes in workers' separation behavior with changes in workforce composition. Because inspected firms also hire more workers, in particular blue-collar, young and inexperienced workers who may have higher turnover rates, unchanged firm-level separations need not imply unchanged separation rates at the worker-level. We return to this issue in Section 6.4, where we implement a worker-level analysis of incumbent workers—which holds workforce composition fixed—to estimate the effect on separations.

Figure 9b shows that the observed increase in hires is driven by both firm-to-firm moves and by individuals who are not registered in the IMSS system in the prior month (e.g., unemployed or out of the labor force) but who have prior formal sector experience. We find similar results when estimating the effects on the stock of workers who joined the firm after December 2015, distinguishing workers by hiring origin. Specifically, Appendix Figure A.18 shows positive effects on the stock of workers who previously worked at another formal IMSS firm and on workers who came from outside IMSS but have been registered in IMSS before.[60] As we discuss further in Section E.1, these results suggest that the increase in firm employment is largely driven by hiring already formal workers rather than formalizing informal workers.

Figure 9: Effect on Hires and Separations



(a) Log Total Hires and Sep. in the past 12 Months (b) Log Total Hires in past 12 Months by Type

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:11]$. In panel (a), the outcome variables are the log sum of hires (black diamonds) and the log sum of separations (grey dots) in the past 12 months. In panel (b), the outcome variables are the log sum of hires from outside IMSS (black diamonds) and the log sum of hires from another IMSS firm (grey dots) in the past 12 months. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019 in IMSS. Control firms are those that are never treated or not yet treated until H_{max}. 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, improvements in working conditions induced by inspections can increase employment by expanding labor supply. The observed increase in hires points to the fact that better working conditions make firms more attractive to potential workers. Section 7 discusses direct empirical evidence in line with this mechanism. In addition, Section 6.4

[59] The same holds when separations are normalized by current or lagged employment.

[60] We find no statistically significant effect on hires or on the stock of workers with no previous IMSS employment history; these estimates are omitted for brevity.

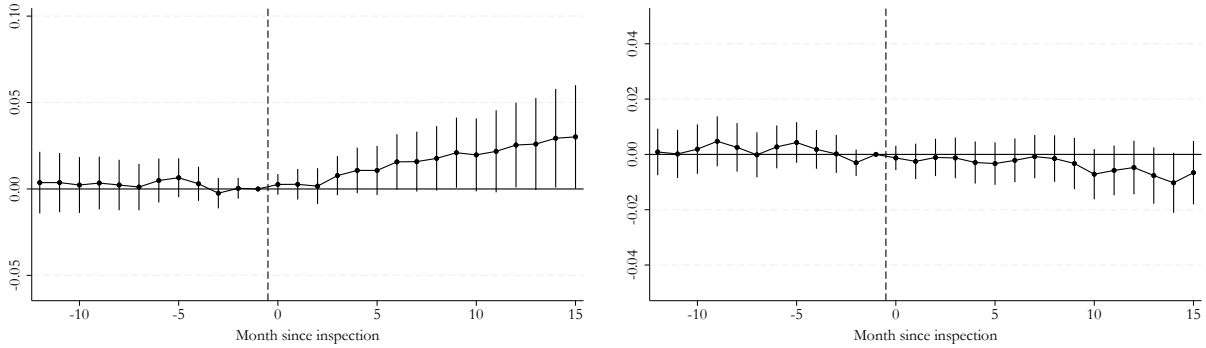
shows that the increase in employment is also partly driven by improved worker retention, consistent with incumbent workers valuing better working conditions.

A necessary implication of this interpretation is that any wage adjustment to improved working conditions be limited. If firms offset improvements in non-wage amenities by lowering wages, the overall value of the job to workers may not increase sufficiently to rationalize an increase in labor supply. In the next subsection, we show that wage effects are indeed small.

6.3.2 Labor Costs

In this section, we assess the impact of inspections on firms' labor costs. Prediction 2a in Section 2 implies that enforcing higher workplace standards may reduce wages because wages and working conditions are imperfect substitutes in the workers' labor supply. As inspections improve working conditions, firms can therefore partially offset the associated increase in labor costs by lowering wages.

Figure 10: Effect on Labor Costs



(a) Log Total Daily Firm Wages

(b) Log Avg. Daily Firm Wage

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 15]$. In panel (a), the outcome variable is log total daily wages in the firm. In panel (b), the outcome variable is log average daily firm wage. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019 in IMSS. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

Consistent with the increase in firm employment, Figure 10a shows that total IMSS daily firm wages rise by around 3 percent one year after an inspection. We find a similar pattern in the monthly survey data, reported in Figure A.25a.[61] Since total employment increases by about 4 percent, the 3 percent increase in total daily wages implies a decline in average daily firm wages of roughly 1 percent. Figure 10b confirms this effect size: coefficients for average log wage effects are approximately -0.01 and 95% confidence intervals rule out wage declines larger than 2.1 percent. We find similar effects for median firm wages and report effects across the wage distribution in Appendix Figure A.21. We also find almost the same pattern when grouping establishments at the firm-by-state level, reported

[61] The EMIM survey measure of total labor costs includes benefits such as social security contributions and profit sharing at the monthly level.

in Appendix Figure A.22. Figure A.19 shows that effects are very similar for the average firm wages of men and women, with slightly larger decreases for women.[62]

Importantly, changes in average firm wages post-inspection combine the effects of firms' wage-setting decisions with changes in workforce composition. As shown in Section 6.3.1, inspections shift the workforce toward younger, less experienced, and more blue-collar workers, implying that changes in composition are likely to contribute to lower average wages. In the next subsection, we disentangle these composition effects from changes in firms' wage-setting.

6.3.3 Labor Costs Decomposition with a Time-Varying AKM Model

To distinguish between changes in firm wage-setting and changes in workforce composition following an inspection, we estimate a time-varying AKM model (TV-AKM) following Lachowska et al. (2023). This approach is based on the framework by Abowd, Kramarz, and Margolis Abowd et al. (1999), which has been widely applied to a range of labor market questions, including inequality (Card et al., 2013) or compensating wage differentials (Sorkin, 2018). We explain the exact data construction of the underlying sample—the largest connected set—in detail in Appendix B, where we also provide a series of validity checks. Of the 9,416 establishments in our IMSS sample, 9,411 belong to the largest connected set. We then estimate the effect of inspections on the different components recovered from this model, allowing us to decompose the overall effect on average firm wages into changes in wage-setting and workforce composition.

TV-AKM Model. The model allows for a decomposition of worker wages into worker fixed effects, time-varying firm effects, age controls, and a residual component:

$$\log(wage_{it}) = \alpha_i + \psi_{j(i,t),t} + x'_{it}\beta + \epsilon_{it} \tag{10}$$

where $\log(wage_{it})$ denotes the average log real daily wage of worker i in year t between 2015 and 2024. α_i denotes the worker fixed effect, which captures permanent differences in workers' earnings capacity. $\psi_{j(i,t),t}$ denotes the time-varying firm fixed effect for firm j with worker i in year t, which captures changes in firm-specific wage premia.[63] x'_{it} includes quadratic and cubic terms in age, following Card et al. (2013).[64]

Appendix Figure A.23 compares the distributions of estimated firm and worker fixed effects in the largest connected set and the main IMSS inspection sample. The distribution of firm fixed effects in the our manufacturing IMSS sample is shifted toward higher values, consistent with the fact that our sampling procedure selects large and more productive firms on average. In contrast, the distribution of worker fixed effects is highly similar across the two samples.

We next use the estimated worker and firm fixed effects to decompose the effect of an inspection

[62]Figure A.20 shows no detectable effects on average firm wages for total, blue-collar, or white-collar workers in EMIM. However, labor costs in EMIM are measured with error during this period because the legal use of outsourcing affects how firms report labor costs (see Colonna and Aldeco (2025) for details). We therefore focus our wage analysis on IMSS, which provides more accurate measures of worker wages.

[63]Throughout this section, we use the terms establishment and firm interchangeably.

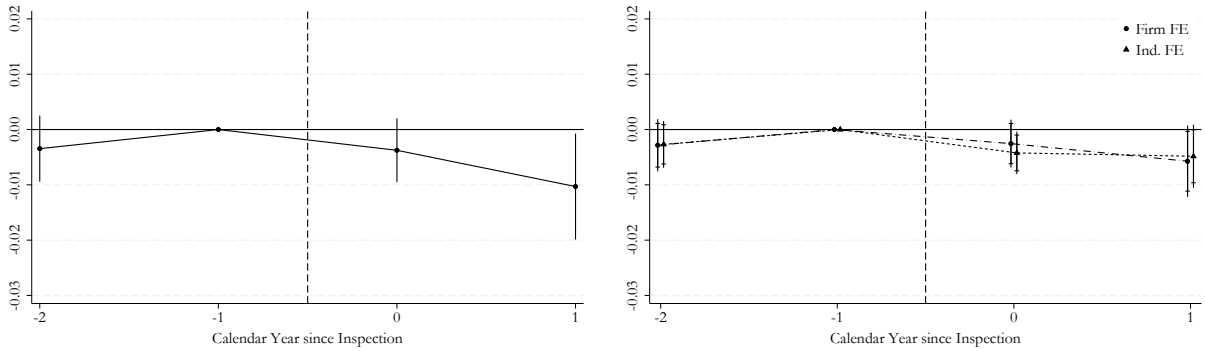
[64]These age terms are normalized at age 40, where the wage-age slope is flat. Figure B.1 shows this estimated relationship between log real daily wage and age. We do not include educational interactions, because education is not observed in IMSS.

on average firm wages into workforce composition and wage-setting effects. For our analysis, we aggregate worker fixed effects within each firm-year and define the average worker effect as $\bar{\hat{\alpha}}_{jt} = \frac{1}{N_{jt}} \sum_{i \in \mathcal{I}_{jt}} \hat{\alpha}_i$. We then separately estimate the effects of inspections on the estimated firm fixed effect, $\hat{\psi}_{j(i,t),t}$, and the average worker effect, $\bar{\hat{\alpha}}_{jt}$, using the LP-DiD specification in Equation (7) at the calendar-year level in the largest connected set of the main IMSS sample.[65] The former outcome represents the effect of an inspection on firms' wage setting after controlling for worker composition, while the latter outcome reflects the effect of an inspection on the average firm wage that is explained by changes in the type of workers employed post-inspection.

Reassuringly, we replicate the main finding for the effect on average firm wages. As shown in Figure 11a, average firm wages are 1.03% lower in the year following an inspection relative to the pre-inspection period.[66] Figure 11b shows that the firm effect declines by 0.57%, accounting for approximately 54% of the total wage decline, and that the average worker effect declines by 0.48%, accounting for the remaining 46%.[67]

Figure 11: Decomposition of Wage Results

(a) Firm Average Log Real Wage per Year (b) Firm and Average Individual AKM Fixed Effects



Notes: The figure reports LP-DiD estimates of β^h, 95% and 90% confidence intervals from estimating Equation (7) at the yearly level for $h \in [-2:1]$. Panel (a) shows effects on average log real wages at the firm-year level. Panel (b) shows dots for the effects on the estimated time-varying firm fixed effect and triangles for the effect on the average worker fixed effect within the firm-year. Firm and worker effects are estimated using Equation (10). The sample consists of the 9,411 establishments in the main IMSS inspection sample that belong to the largest connected set of the AKM model. Event time is measured in calendar years relative to the year of inspection. Vertical lines indicate 95% confidence intervals. Vertical capped lines indicate 90% confidence intervals. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019 in IMSS. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

Taken together, these results suggest that the modest decline in firm wages following inspections is driven roughly equally by changes in firm wage-setting behavior and changes in workforce composition. The composition effect is consistent with the changes in the occupational and demographic composition of the workforce documented above, as blue-collar and inexperienced workers tend to

[65] As in the main analysis, we consider first random inspections occurring between February 2018 and December 2019.

[66] This corresponds to a reduction of about 4 Mexican pesos in daily wages, consistent with results from a regression of wages in levels.

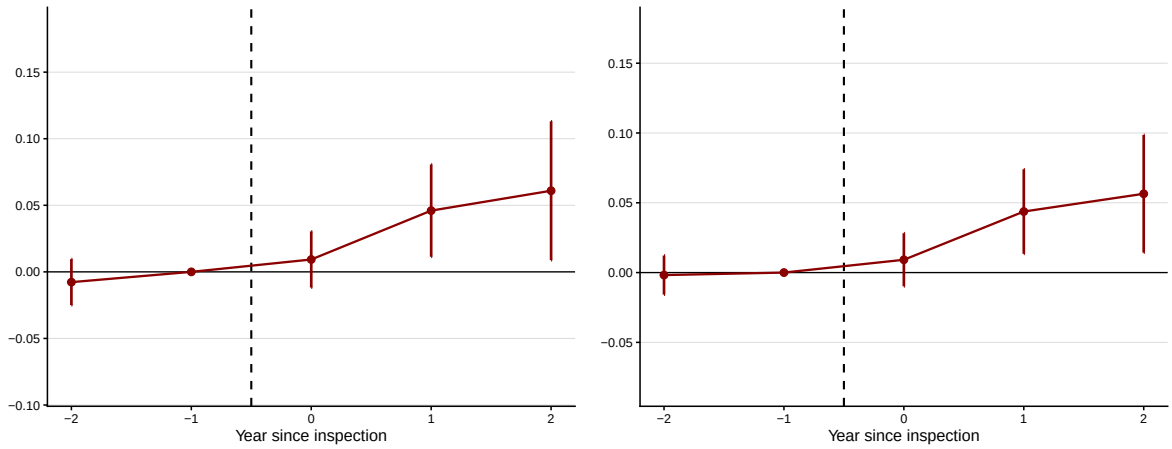
[67] Changes in the age-control terms explain only a negligible share of the overall effect (+0.02%), while the average residual component is zero.

have lower estimated worker fixed effects than the average worker employed by the firm prior to the inspection. Meanwhile, the decomposition indicates that firm substitution for better working conditions with lower wages was modest, as we can reject changes in firms' wage premia larger than -1.2%.

6.3.4 Production Process: Revenues, Costs, and Profits

Figures 12a and 12b show that the increase in firm employment is accompanied by significant increases in both total annual revenue and total production costs.[68] The estimated coefficients are approximately 6% two years post-inspection for both outcomes, and we cannot reject effects equal to the 4% increase in total employment. In line with these results, we find no significant change in revenue per worker nor production costs per worker as shown in Figure A.24, suggesting that while inspections increase production, they do not alter average worker productivity. Additionally, appendix Figure A.25 further shows an increase in firms' reported capacity utilization. Overall, these results reflect that the rise in employment post-inspection is accompanied by a real expansion of the firms' production process. They also indicate that the observed employment gains are unlikely to reflect the formalization of previously informal workers, but instead represent genuine growth in firm size, as discussed in more detail in Appendix Section E.1.

Figure 12: Effect on Revenues and Production Costs



(a) Log revenue (b) Log total production costs

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the year level for $h \in [-2:2]$. In panel (a), the outcome variable is log of yearly revenue. In panel (b) the outcome is log of total production costs, which is the sum of total costs of goods and services for production and total labor costs. Regressions are estimated based on a balanced sample of firms from EAIM. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are not treated until period h. Standard errors are clustered at the firm level.

Appendix Figure A.13 shows positive effects on total hours worked, yet no significant effects on average hours worked among blue- and white-collar workers. This indicates that the increase in total employment was not offset by changes in working hours. Finally, the effects on firm profit margins

[68] This variable includes total costs of goods and services for production and total labor costs.

are reported in Figure A.26. While the coefficients are negative, the estimates are not statistically distinguishable from zero at the 95% confidence level.[69] This suggests that the observed increase in total measured costs (including labor costs and production related costs) is not fully offset by the rise in total revenue (Figure 12a), potentially leading to a modest decline in firm profitability. It is important to note that profits are not directly observed in the firm survey and are instead inferred as the difference between revenues and all reported costs. As discussed in Section 6.2, firms are not asked to directly report compliance related costs nor costs from inspection fines in the EMIM and EAIM surveys. Because these costs may not be fully captured in the other cost categories included in the survey, total costs may be understated, which would in turn attenuate our estimated effects enforcing compliance of workplace regulations on profits. The absolute magnitude of the estimated effects on profits should therefore be interpreted as a lower bound.

6.4 Effect of Inspections on Worker-level Outcomes

We complement the firm-level analysis with worker-level event studies around inspections. This analysis allows us to isolate the effects of inspections on incumbent workers' retention probability and wages from changes in workforce composition among treated firms. We construct a balanced monthly panel of around 8 million workers ever employed between 2017 and 2020 in one of the 9,416 establishments in the main IMSS sample.[70] Using this sample, we define a worker as treated in t if they are employed at an establishment that receives its first inspection in month $t+1$, and compare them to control workers employed at establishments in the same randomization group that have not yet been inspected over the relevant event-study horizon. We exclude workers at never inspected establishments from the sample. As in our main analysis, we focus on first random inspections conducted between February 2018 and December 2019. Formally, we estimate for each horizon h:

$$Y_{i,t+h} - Y_{i,t} = \beta^h \Delta D_{j,t+1} + \gamma^h_{t,g_{(t)}} + \varepsilon^h_{ij,t}, \tag{11}$$

where worker i is assigned to the establishment j at which she is employed in the baseline month t. The variable $\Delta D_{j,t+1}$ equals one if establishment j receives its first inspection in the following month. The fixed effect $\gamma^h_{t,g_{(t)}}$ denotes a randomization-group-by-month fixed effect, defined using the worker's baseline establishment. Standard errors are clustered at the baseline establishment level. As treatment is assigned according to the worker's establishment in the month before the inspection, irrespective of whether the worker remains at that establishment afterwards, the coefficient β^h has an intent-to-treat interpretation. This timing is important because post-inspection employment at the inspected firm is endogenous to the treatment.

Figure 13a reports the first stage: the effect on working at an establishment that has already been inspected.[71] Before the inspection, the coefficients are close to zero, with tight confidence intervals.

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

[70] Workers remain in the panel regardless of where they are employed in a given month. If a worker is not observed in IMSS in a month, we set employment equal to zero. Hence, throughout this section, employment refers to formal-sector employment.

[71] In the event-time figures, the baseline month t is labeled -1, and the inspection month $t+1$ is labeled 0.

In the inspection month, the coefficient is about 95 percentage points, reflecting that most baseline workers are still employed at the treated establishment when it is inspected. The first stage remains large over time, but declines to about 74 percentage points after eleven months, consistent with regular worker turnover among incumbents.

We first assess whether inspections impact incumbent workers' employment trajectories. Figure 13b shows no effect on formal employment. The pre-inspection coefficients do not indicate differential trends, and the post-inspection estimates are tightly centered around zero, with confidence intervals of roughly one percentage point. Thus, inspections do not appear to change incumbent workers' likelihood of remaining formally employed. Figure 13c instead shows a positive effect on workers' likelihood of remaining employed at the baseline establishment. Incumbent workers at inspected firms are about one percentage point more likely to stay with their employer than workers at comparable not-yet-inspected firms. This increase is mirrored by a roughly one percentage point decline in job-to-job transitions, which are more likely to represent voluntary job moves. By contrast, Appendix Figure A.27a shows no effect on transitions out of IMSS after the inspection, which are more likely to capture changes of involuntary layoffs. Taken together, these findings suggest that inspections increase voluntary worker retention rather than reducing involuntary separations.

Finally, Figures 13e and 13f reports wage effects. We consider both an unconditional wage measure, which assigns zero wages in months without formal employment, and conditional log wages among formally employed workers. Both estimates are close to zero and precisely estimated. In Appendix Figure A.27b, we report similar null effects on log wages of workers, conditional on working at the baseline establishments.[72].

Combining the null wage effects for incumbent workers with the decline in the TV-AKM firm wage premium documented in Section 6.3.3 allows us to bound the wage-setting effect for new hires.[73] A back-of-the-envelope calculation that assigns the full estimated decline in firm wage premia to new workers, and a share of new hires in the year after inspection equal to 41.9 percent, implies a wage-setting effect for new workers of -1.36 percent. Using the lower bound of the overall wage-setting effect of -1.2 percent implies a maximum decline of -2.86 percent for new worker wages.[74] Therefore, even if we attribute the estimated decline in firm wage premia entirely to new workers, the implied wage-setting effect for this group would remain small.

Overall, the results in this section show that inspected establishments retain incumbent workers at higher rates, likely driven by an increase in voluntary retention, with little evidence of offsetting wage adjustments. This pattern is consistent with incumbent workers placing positive value on the improvements in working conditions induced by inspections, thereby increasing the overall value of

[72]This result holds for restricting to balanced sample of workers who work consecutively at their baseline establishment in the year before until the year

[73]Directly estimating average wages of new hires would not isolate wage setting, since the composition of hires may also change after inspections.

[74]Table A.2 reports a pre-inspection monthly separation rate of 5.1 percent. Combining this with the 1 percentage point reduction in incumbent separations documented in Section 6.4 gives a post-inspection monthly separation rate of 4.1 percent. If this rate applies over the year, $0.959^{12} = 60.5$ percent of incumbents remain employed after 12 months. This represents 344 workers on average, given an average pre-inspection employment of 569 workers. With a 4 percent employment increase post-inspection, employment rises to 592 workers, implying that 248 workers, or 41.9 percent of post-inspection employment, are new. Assigning the full firm wage premium effect of -0.57 percent to this group implies an effect of -1.36 percent for new workers; using the lower bound of -1.2 percent implies -2.86 percent.

their jobs and making them less likely to leave the firm. These findings are therefore consistent with Prediction 2 that inspections increase labor supply to inspected firms.

7 Evidence on mechanism and willingness to pay for an inspection

7.1 Empirical evidence on main mechanism

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

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

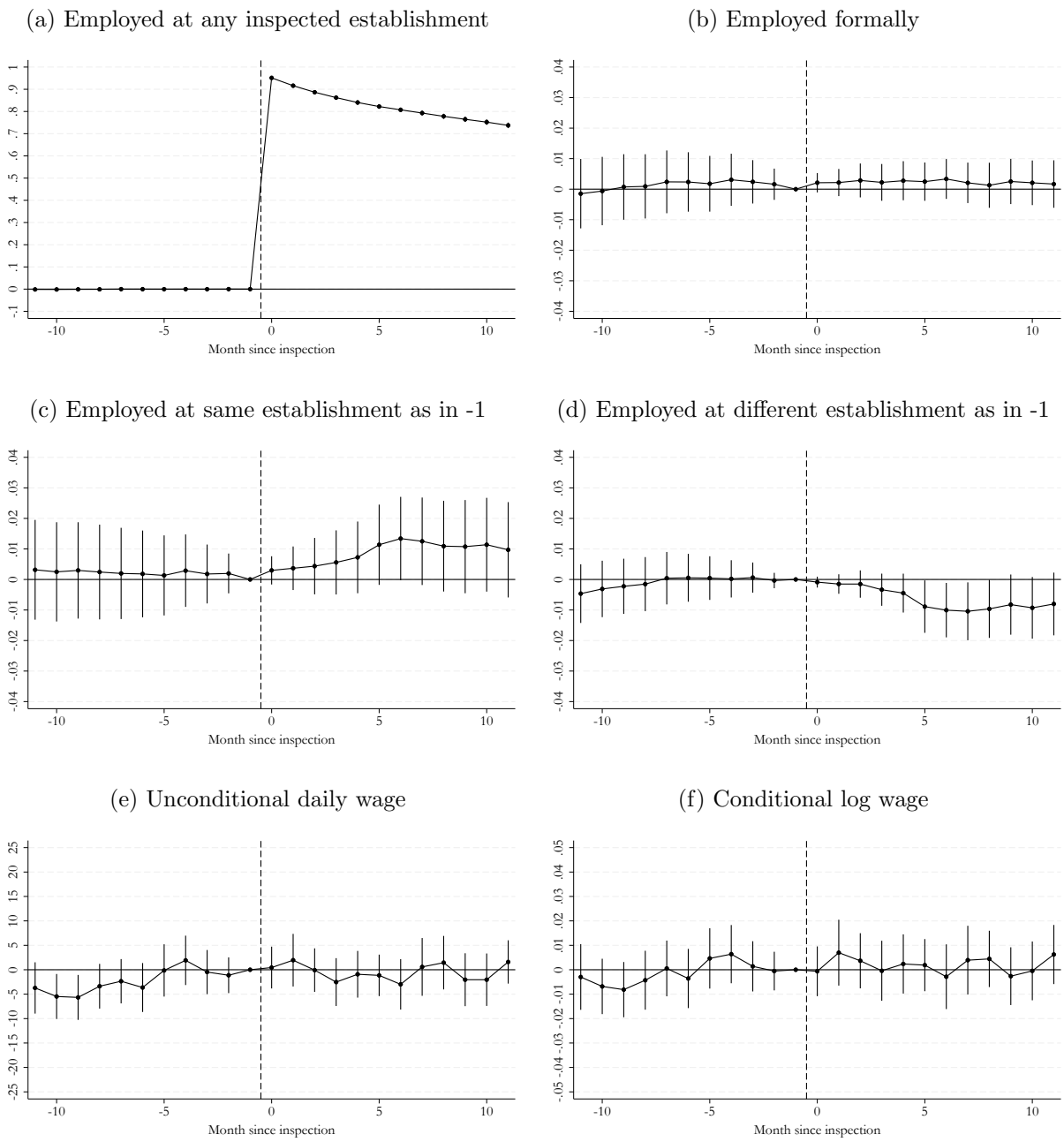
To empirically assess whether working conditions play a role in workers' labor supply decisions, we first draw on survey data from Mexico's National Survey on Occupation and Employment (ENOE), a nationally representative household survey conducted by INEGI. In this survey, workers report their reasons for quitting a previous job. We restrict the sample to workers who indicated that they had quit from their previous job, and whose previous job had been a formal firm in the private sector (N = 3477).[75] Panel (a) of Figure A.28 illustrates the distribution of these responses for the 2018–2020 period.[76] We find that poor working conditions are a key reason for quits alongside better pay. Hazardous work environments—directly aligned with our empirical findings—also play an important role, with 5% of workers explicitly citing workplace risks as their main reason for quitting. This finding is consistent with our empirical evidence in Figure A.17, which shows that the increase in firm employment is concentrated among firms found violating during inspections. Prior to inspection, these firms do not meet minimum working condition standards and are therefore likely less attractive to workers than compliant firms. Following inspections, improvements in working conditions likely improve these firms' attractiveness, leading to an increase in labor supply and firm employment.

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

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

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

Figure 13: Worker-level effects on employment and wages



Notes: This figure shows estimates of β^h and 95% confidence intervals from estimating Equation (11) at the worker-month level for $h \in [-12, 11]$. In panel (a), the outcome is working at an establishment that has already been inspected. In panel (b), the outcome is formal employment. In panel (c), the outcome is working at the same establishment as in the month before inspection. In panel (d), the outcome is working at a different establishment as in the month before inspection. In panel (e), the outcome is unconditional formal daily wages. In panel (f), the outcome is log wages conditional on positive formal employment. Regressions are estimated using a balanced monthly panel of workers ever employed between 2017 and 2020 in one of the establishments in the experimental sample. Treated workers are those employed in the month before inspection at an establishment receiving its first inspection in the following month between February 2018 and December 2019. Control workers are employed in the same calendar month at establishments in the same randomization group that are not yet treated until H_{max}. Workers not observed in IMSS in a given month are coded as not formally employed, and unconditional wages are set to zero in those months. Standard errors are clustered at the baseline establishment level.

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.[77] Panel (b) of Figure A.28 displays the distribution of responses. Over 40% of workers report learning about their job through family or friends—sources likely to convey information about working conditions that would not be disclosed in a typical job advertisement.

To further explore the salience of working conditions in workers' job selection, we conducted a complementary survey on Prolific with a sample of 638 Mexican workers.[78]. Respondents were asked to indicate the job characteristics they considered when choosing their current employment. Table A.9 reports summary statistics, disaggregated by income group. Roughly 14% of respondents cite working conditions and nearly 35% highlight training opportunities as a salient factor in their employment choice.

Taken together, these patterns suggest that working conditions, training opportunities, and safe working environments are meaningful components of both incumbent and newly hired workers' labor supply decisions. They are in line with the fact that improvements in working conditions can impact both workers' decision to quit a job or to stay, consistent with the observed decrease in separations. Moreover, the evidence suggests that these features are also relevant for new workers, who are also likely to have information on these firm characteristics when deciding whether to work for the firm.

Alternative mechanisms. In Appendix E.1, we consider four 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, (iii) firm misinformation regarding the potential costs and benefits of improving working conditions, and (iv) changes in the management structure of the firm. We provide empirical evidence suggesting that these explanations are either inconsistent with the empirical evidence, unlikely to fully account for the magnitude of the observed effects, particularly in terms of employment, or they also operate through our main mechanism in which employment rises because inspections increase labor supply to monopsonistic firms. We summarize the main arguments developed in detail in Appendix E.1 below.

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 social security payments per worker. Moreover, we observe that the increase in employment is substantially driven by hires from other formal firms and increases in worker retention, rather than by first-time hires from outside IMSS. In addition, the strongest employment effects follow inspections related to safety and training, rather than those targeting general labor conditions (including informality). In addition, employment increases gradually after inspections, rather than exhibiting the immediate jump that would be expected if the effects were primarily driven by formalization, as in Brotherhood et al. (2024).

Second, while some inspections may lead firms to mechanically hire additional personnel to meet specific safety or compliance standards, such requirements are limited in scope and unlikely to explain

[77] The sample consists of workers employed in private manufacturing firms with more than 20 employees (N = 46,779).
[78] Details on the sample characteristics can be found in Colonna and Aldeco (2025)

the increase in total employment. Additionally, this explanation would not be consistent with the observed rise in total revenues and production costs.

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

Fourth, our evidence and institutional setting are not consistent with the effects being driven by firms replacing managers after an inspection with more compliant or more capable ones. While we cannot directly identify managers in our data, we test this mechanism by assessing whether inspections increase the hiring of workers with top-coded wages in the IMSS, who are likely to occupy managerial positions. We find no effect on this outcome, providing no evidence that inspections induce managerial turnover.

Section E.2 further reconciles the findings presented in Section 6.3 with the prior literature on labor inspections (Samaniego de la Parra and Fernández Bujanda, 2024; Brotherhood et al., 2024). The key distinction is that, in our sample of large manufacturing firms, inspections primarily improve working conditions for formal workers, and the resulting employment effects are consistent with a monopsony model in which firms possess market power over working conditions. By contrast, the existing literature has focused primarily on smaller firms, where inspections mainly operate through the formalization of informal workers and where firms are likely to possess substantially less labor market power than those in our setting.

7.2 Inferred change in amenities and willingness to pay for an inspection

The provided mechanism relies on the finding that workers value inspections because they improve non-wage amenities. In this section, we combine our empirical estimates of the effects on employment and wages with the structure of the theoretical framework presented in Section 2 to infer a quantitative value for the implied change in workplace amenities and workers' willingness to pay for an inspection.

Let (w_0, a_0) and (w_1, a_1) denote the pre- and post-inspection job bundles. Define WTP as the wage change at the initial amenity level that makes workers indifferent to the post-inspection bundle. Using the utility function $V(w, a) = w^{\beta_w} a^{\beta_a}$ defined in Section D, this can be expressed as:

$$(w_0 + WTP)^{\beta_w} a_0^{\beta_a} = w_1^{\beta_w} a_1^{\beta_a}. \tag{12}$$

Solving and log-linearizing gives

$$\frac{WTP}{w_0} = \frac{w_1}{w_0} \left(\frac{a_1}{a_0} \right)^{\frac{\beta_a}{\beta_w}} - 1 \approx \Delta \log w + \frac{\beta_a}{\beta_w} \Delta \log a.$$

Because

$$\Delta \log V = \beta_w \Delta \log w + \beta_a \Delta \log a, \tag{13}$$

the wage-equivalent value can be written as

$$\frac{WTP}{w_0} \approx \frac{\Delta \log V}{\beta_w}.$$

Using labor supply $n = V^\theta$, we have $\Delta \log V = \Delta \log n/\theta$. Therefore:

$$\frac{WTP}{w_0} \approx \frac{\Delta \log n}{\theta \beta_w}. \tag{14}$$

Benchmark example. Using the empirical estimates from Section 6.3, inspections increase employment by about 4%, which we assume stems entirely from an increase in labor supply, while average wages decrease by at most 2%. For a benchmark calibration, we set $\theta = 2$ and assume $\beta_w = 0.55$ and $\beta_a = 0.45$.[79] The labor-supply equation then implies that job value increases by 2%, since $\Delta \log V = \Delta \log n/\theta = 0.04/2 = 0.02$. Combining this with the wage decrease of 2%, the job-value equation (13) implies an amenity improvement of 6.8%. The corresponding wage-equivalent willingness to pay, given by (14) is 3.6% of pre-inspection wages.

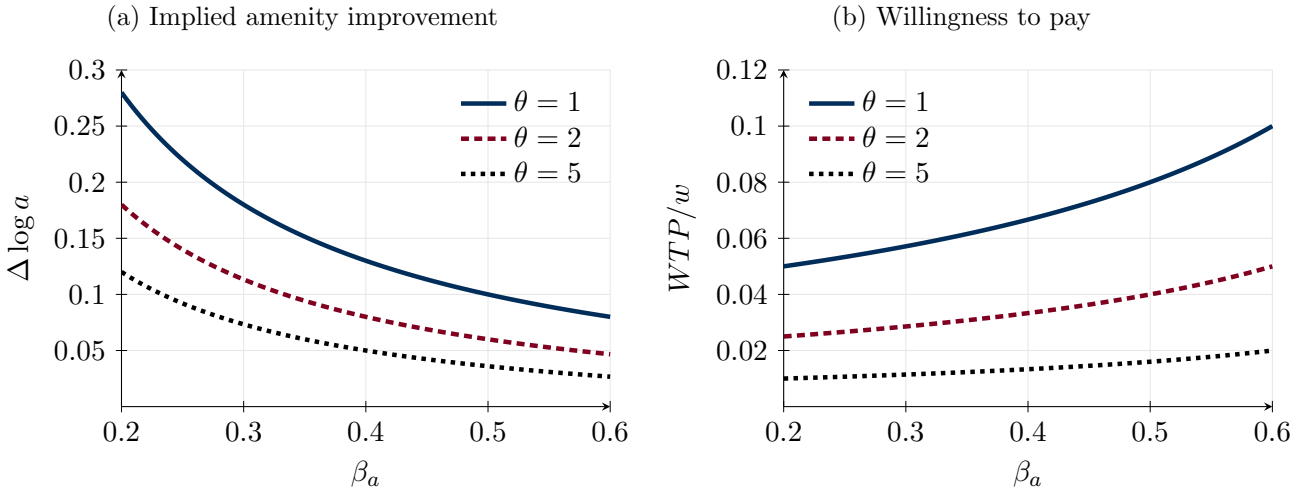
Results across parameter values. We repeat the calibration for a wider set of parameters to assess the robustness of our estimates. We allow $\theta \in \{1, 2, 5\}$ and $\beta_a \in [0.2, 0.6]$, with $\beta_w = 1 - \beta_a$. Figure 14a presents the implied amenity improvement. Across the parameter range, the implied amenity improvement varies from approximately 2.5% to 28%.[80] These magnitudes are plausible relative to the empirical evidence presented in Section 6.2, where we show that inspections increase training costs per worker by around 30% and reduce workplace accidents by around 10%. Figure 14b shows that the willingness to pay for an inspection ranges across parameter values from about 1% to 10%, expressed in terms of pre-inspection wages. While we are not aware of previous estimates of workers' willingness to pay for workplace inspections, our estimates are broadly in line with willingness-to-pay estimates for related workplace amenities in the literature. For instance, Maestas et al. (2023) find that workers are willing to pay 5% of wages for access to job training opportunities and 15% of wages to avoid jobs involving heavy physical activity.[81]

[79] We set $\theta = 2$ based on the cross-country labor supply elasticity estimates in Armangué-Jubert et al. (2025). This corresponds to the average estimated labor supply elasticity for countries with GDP per capita levels comparable to Mexico's. β_w and β_a are set to match the ratio between the labor supply elasticity with respect to wages and amenities estimated in (Lagos, 2026).

[80] Using a wage effect of -1% instead of -2% would lead to a very similar range of 2% to 24%.

[81] Dale-Olsen (2006) estimates that workers in Norway are willing to pay approximately 0.2% of annual wages for a 1% reduction in the annual workplace injury hazard. In our setting, inspections are associated with an approximately 9% reduction in workplace accidents (Section 6.2). Applying the estimate from Dale-Olsen (2006) would therefore imply a willingness to pay of around 1.8% of annual wages, which is broadly consistent with our estimates.

Figure 14: Implied amenity improvement and willingness to pay for an inspection



Notes: The figure reports implied values from the back-of-the-envelope calculation using $\Delta \log n = 0.04$ and $\Delta \log w = -0.02$. Panel (a) plots the implied amenity improvement, $\Delta \log a = (0.04/\theta + 0.02(1 - \beta_a))/\beta_a$. Panel (b) plots the wage-equivalent willingness to pay, $WTP/w = 0.04/[\theta(1 - \beta_a)]$. The parameter β_a varies from 0.2 to 0.6, with $\beta_w = 1 - \beta_a$. The blue solid lines corresponds to $\theta = 1$, the red dashed line corresponds to $\theta = 2$, the black dotted lines corresponds to $\theta = 5$.

Both the implied increase in amenities and the willingness to pay are increasing in labor market power, $1/\theta$, because a less elastic labor supply requires a larger improvement in job value, and consequently a higher willingness to pay, to rationalize the 4% employment increase. The implied amenity improvement is larger when workers' valuation of amenities relative to wages, β_a, is relatively low, since a given change in job value then requires a larger proportional change in amenities to rationalize the empirical findings. By contrast, willingness-to-pay estimates increase with β_a, because a higher amenity weight implies a lower wage weight, $\beta_w = 1 - \beta_a$, so a given change in job value corresponds to a larger wage-equivalent value.

8 Conclusion

Despite advances in the implementation of labor regulations, such as standards on safety, health, training, and general labor conditions, enforcement remains a persistent concern for worker welfare in many lower- and middle-income countries (ILO, 2026). This paper contributes to understanding how labor market power helps explain non-compliance with workplace regulations and shapes the consequences of enforcement for firms and workers. We address this question by combining stratified random inspection records with rich administrative and survey data covering large manufacturing firms in Mexico. We find that firms which employ a lower share of female workers, have lower productivity, have a greater labor market share and are in more concentrated labor markets are more likely to be found violating workplace regulations during inspections. We show that enforcement through these inspections enhances compliance, as evidenced by increased investment in worker training and maintenance of equipment, reductions in workplace accidents, and a lower probability of future violations. An inspection leads to a 4% increase in firm-level employment within one year,

driven by an increase in hires of new workers and a rise in retention of incumbent workers. Average firm wages decrease only modestly, driven both by compositional changes and by changes in wage-setting. These empirical results align with a theoretical model in which firms use their market power to offer below-minimum workplace standards. When enforcement improves working conditions and firms do not fully offset these improvements through lower wages, workers receive a higher value from employment at inspected firms, which increases firm-specific labor supply and ultimately firm employment.

These results carry important implications. A common view is that workplace regulations and their enforcement raise firms' labor costs and thereby reduce employment, creating a trade-off between firm performance and better working conditions. We show, however, that the presence of monopsony power over working conditions in large manufacturing firms can mitigate this tradeoff: in our setting, inspections raise compliance and expand firm employment, with only limited negative effects on wages.

These findings can inform the optimal targeting of inspections, particularly in settings with limited enforcement capacity, where governments may seek not only to maximize detection and improve worker well-being, but also to allocate enforcement toward firms where compliance generates the largest economic gains relative to its costs. More broadly, however, the welfare and efficiency implications of enforcement depend on general equilibrium effects, including the reallocation of workers across firms and resulting market-wide adjustments, which are beyond the scope of this paper. Understanding these general equilibrium effects, and their implications for the optimal design of labor inspections, therefore remains an important avenue for future research.

Moreover, this paper remains agnostic about the underlying sources of labor market power over working conditions—whether driven by heterogeneous worker preferences, the salience of job attributes, or other factors. Advancing our understanding of these determinants is another important research direction, as it would guide the design of complementary policies to curb labor market power.

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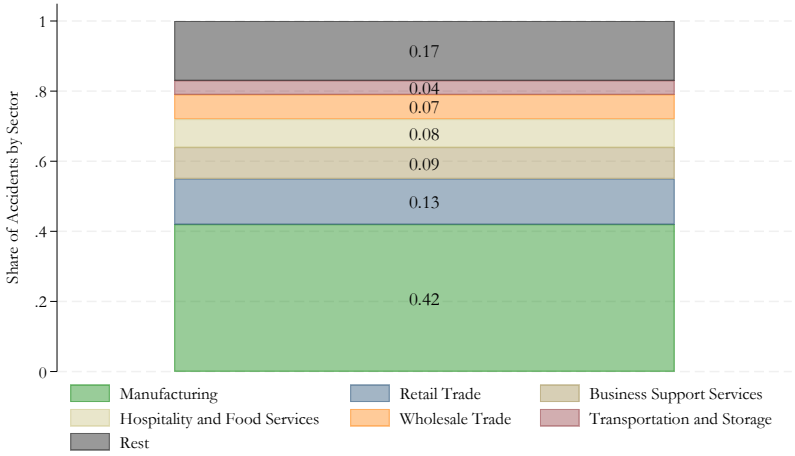
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A Appendix: Additional Tables and Figures

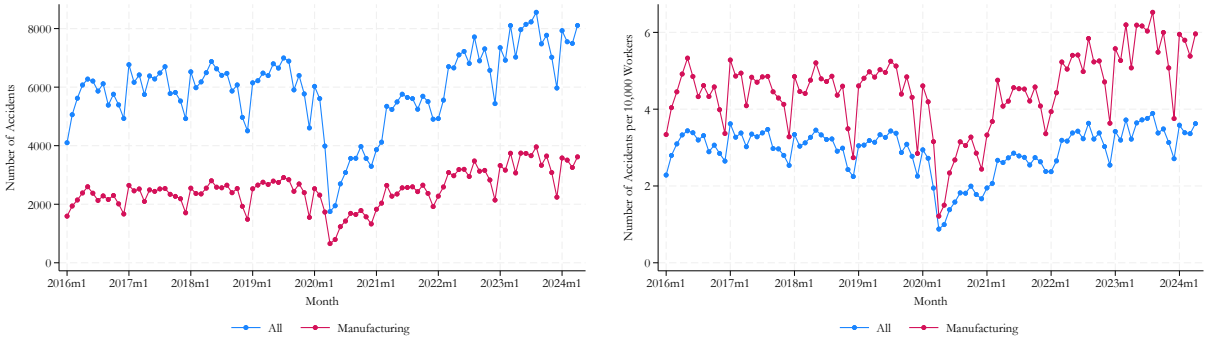
A.1 Figures Appendix

Figure A.1: Share of Accidents by Sector



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

Figure A.2: Accidents over time

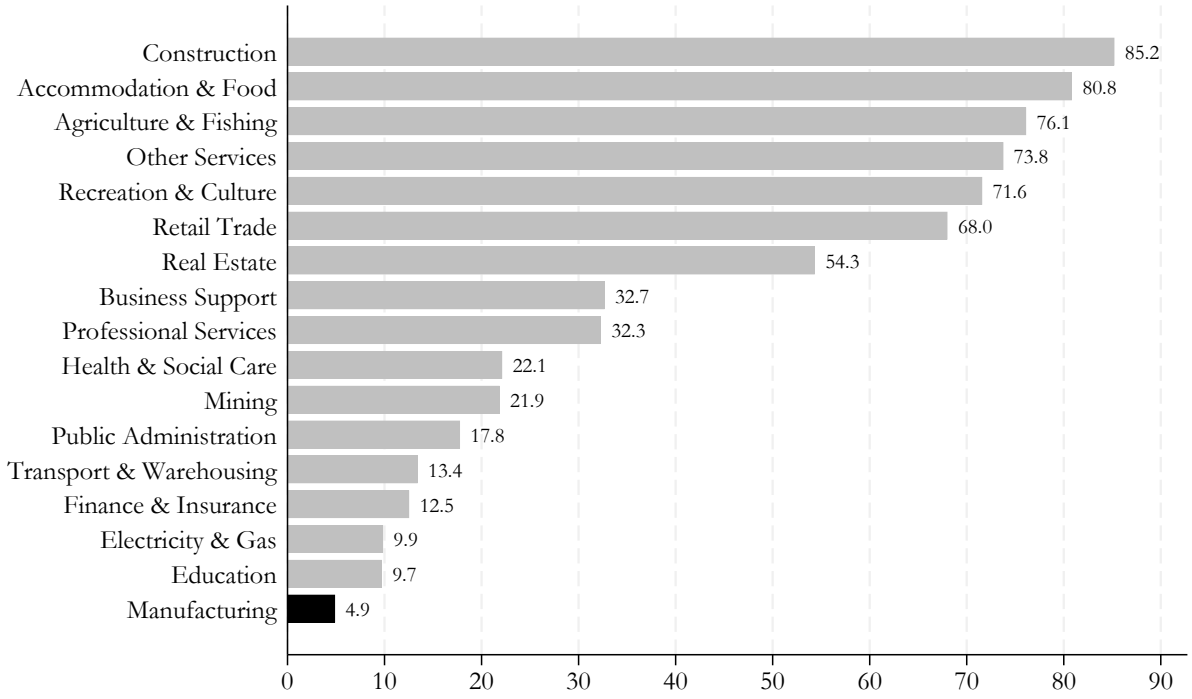


(a) Accidents over Time

(b) Accidents over Time per 10,000 Workers

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

Figure A.3: Share of Informality by Sector



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

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

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

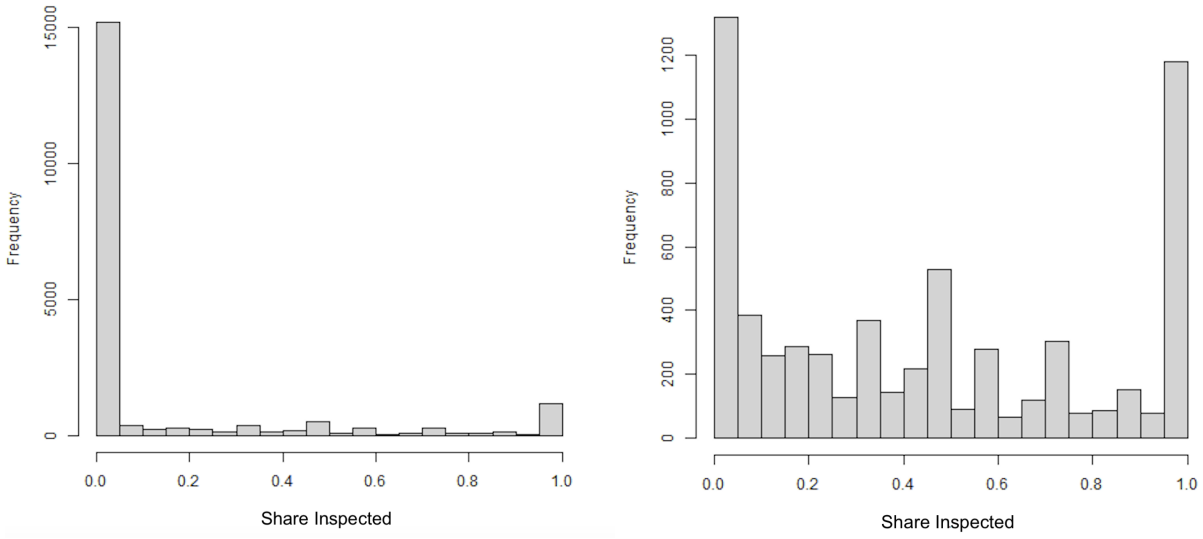
- **Fecha de Alta en el Directorio Nacional de Empresas:** % Menor a 1 año - % De 1 a 3 años - % Mayor a 3 años.
- **Tamaño de la Empresa:** Mínimo 15 trabajadores – Máximo sin límite de trabajadores.
- **Porcentaje de la Clase de Riesgo de IMSS:** %Clase I - %Clase II - %Clase III - %Clase IV - %Clase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

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

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

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

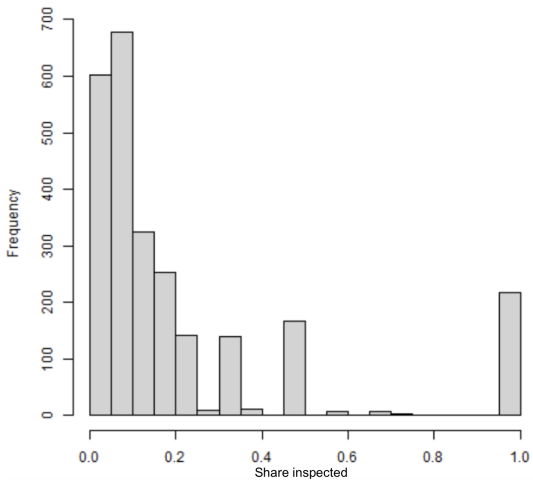
Figure A.5: Distribution in Probability of Inspection across Randomization Groups



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

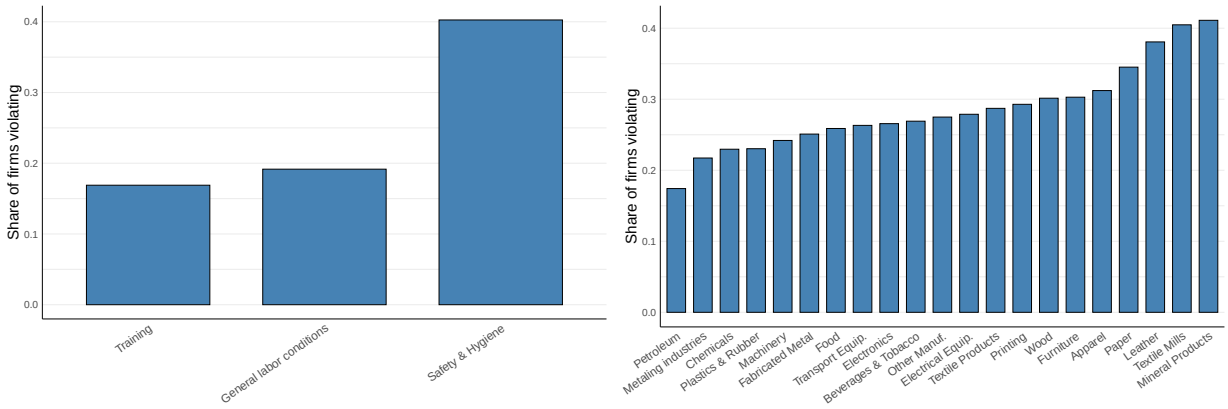
Notes: This figure shows the distribution of the estimated probability of inspection across randomization groups in the Economic Census. Each randomization group is a sector x age group x size group x years since last inspection group x state stratum. The probability of inspection for each group is calculated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

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



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

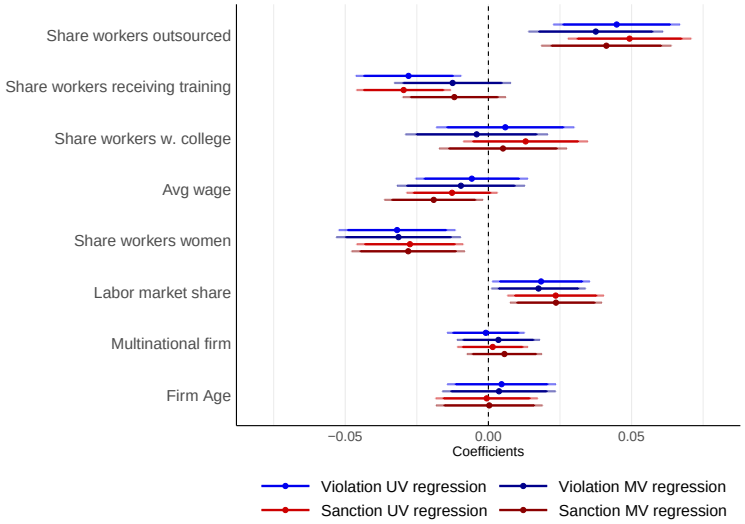
Figure A.7: Patterns in Firms Found Violating Workplace Regulations



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

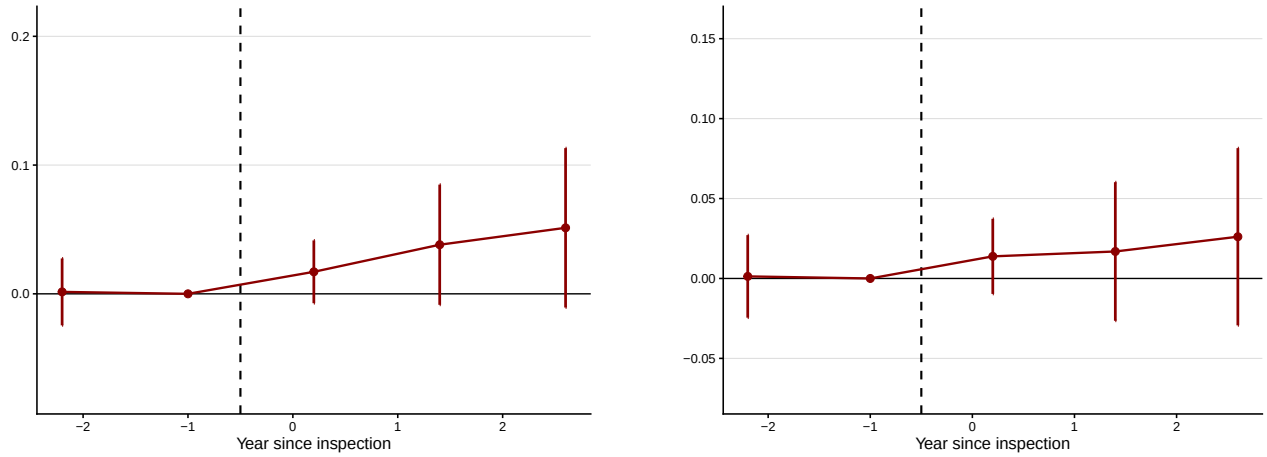
Notes: This Figure shows the share of firms that are found violating workplace regulations during an ordinary inspection by inspection topic (Panel a) and by 21 manufacturing subsectors (Panel b). Data from EMIM.

Figure A.8: Coefficients of Violation Regression - Variables Selected via 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 sanction (red) and the explanatory variables are a subset of firm characteristics from the Economic Census. Coefficients estimated on a sample of manufacturing firms with over 15 workers from Economic Census which received an ordinary inspection between 2019 and 2021. The figure shows results from univariate regressions, and multivariate OLS regressions, where variables are selected using Lasso regularization. All explanatory variables are standardized. All regressions control for estimated probability of inspection. [Back to Section 5]

Figure A.9: Effect of Inspections on Costs on Maintenance of Equipment

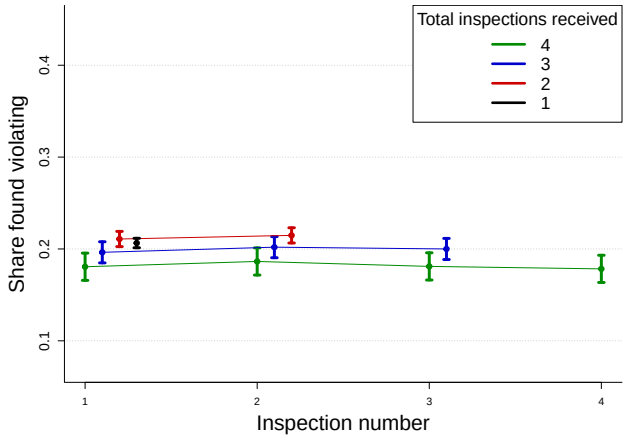


(a) Total maintenance costs

(b) Maintenance costs per worker

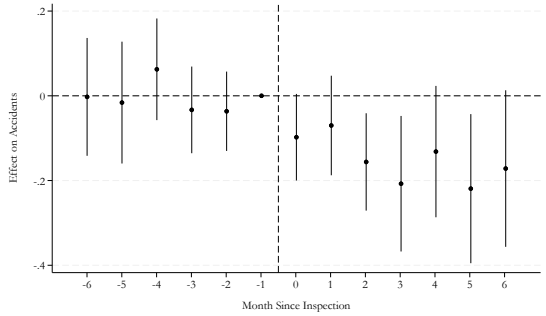
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the yearly level for $h \in [-2:2]$. The outcome variables are log total annual costs on repairs and replacement parts for maintenance of equipment and log total annual costs on repairs and replacement parts for maintenance of equipment per worker. 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.

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

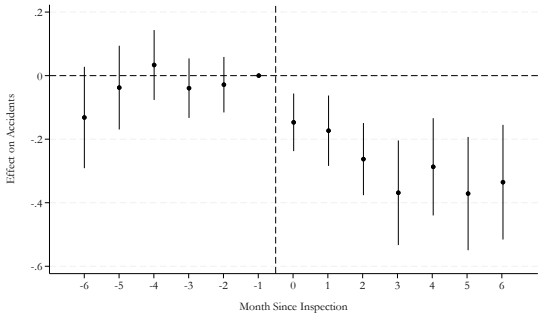


Notes: This figure is constructed using firm-level data on inspected firms between 2017 and 06/2022, from the subsample of inspections merged to DENUE. It shows the coefficients and 95% confidence intervals from regressing a binary variable indicating violation during extraordinary inspection t on a 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.

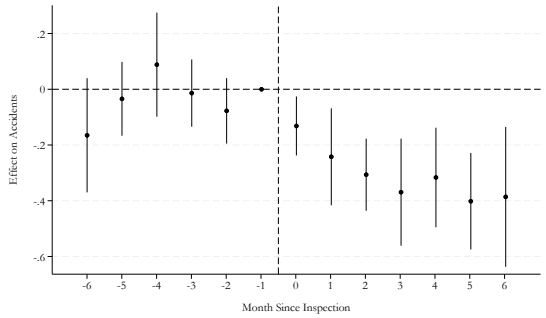
Figure A.11: Robustness Results Effect of Inspections on Accidents



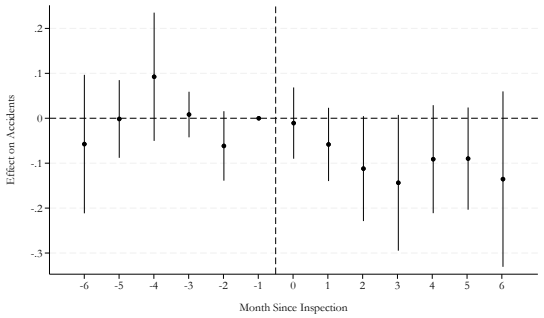
(a) Effect of Ordinary Inspections on Work Accidents - $\hat{p}$ quintiles x time FE



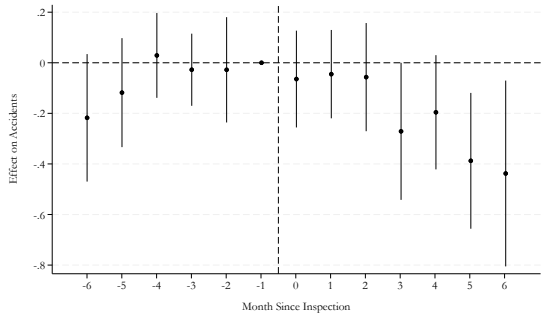
(b) Effect of Ordinary Inspections on Work Accidents - No controls



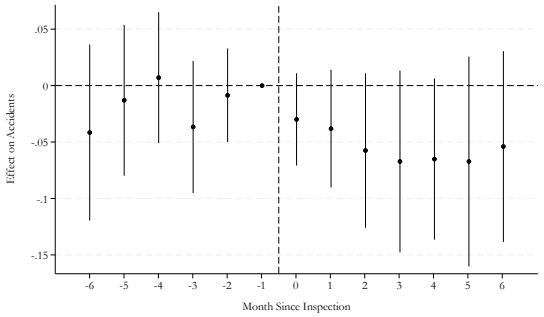
(c) Effect of Ordinary Inspections on Total Accidents



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



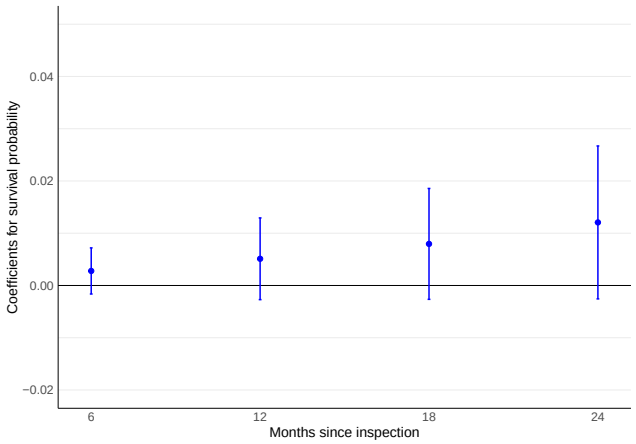
(e) Effect of First Ordinary Inspections on Work Accidents



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

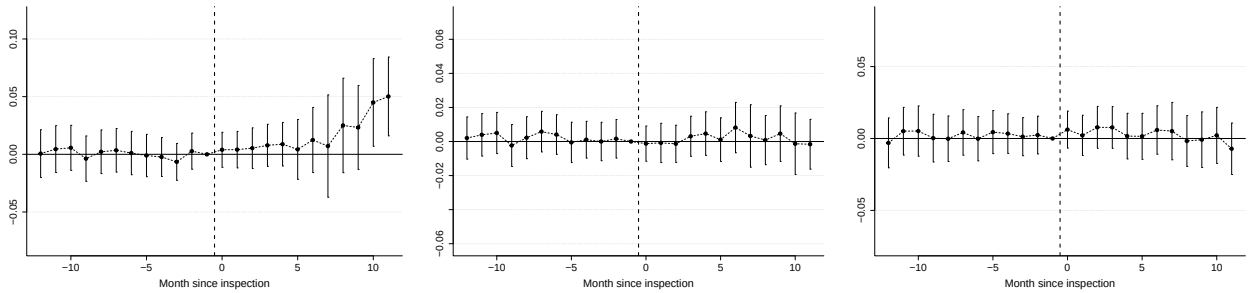
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 8 at the monthly level for $h \in [-6:6]$. Regressions are estimated based on a balanced sample of municipalities from the accident data. Treated municipalities are those receiving a stratified random inspection 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 the number of work accidents at the municipality level. In panel (a) we control for inspection probability quintile by time fixed effects instead of an inspection probability polynomial. In panel (b) we do not include any controls in the regression. Panels (c) and (d) show the effects of ordinary inspections on *total* accidents and the effect of ordinary inspections on total accidents in the manufacturing sector. In panel (e) we consider only *first* stratified random inspection between January 2018 and June 2019. In panel (f) we extend the treatment window to July 2017 until June 2022. [Back to Section 6.2]

Figure A.12: Effect of Inspection on Firm Survival



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [6, 12, 18, 24]$. The outcome variable is an indicator variable equal to 1 if the firm has not exited at $t + h$. Regressions are carried out on a sample of firms from EMIM between 2017 and 2021 that have not yet exited until time t. Treated firms are those receiving the first ordinary inspection between 2018 and 2019 in EMIM. Standard errors are clustered at the firm level.

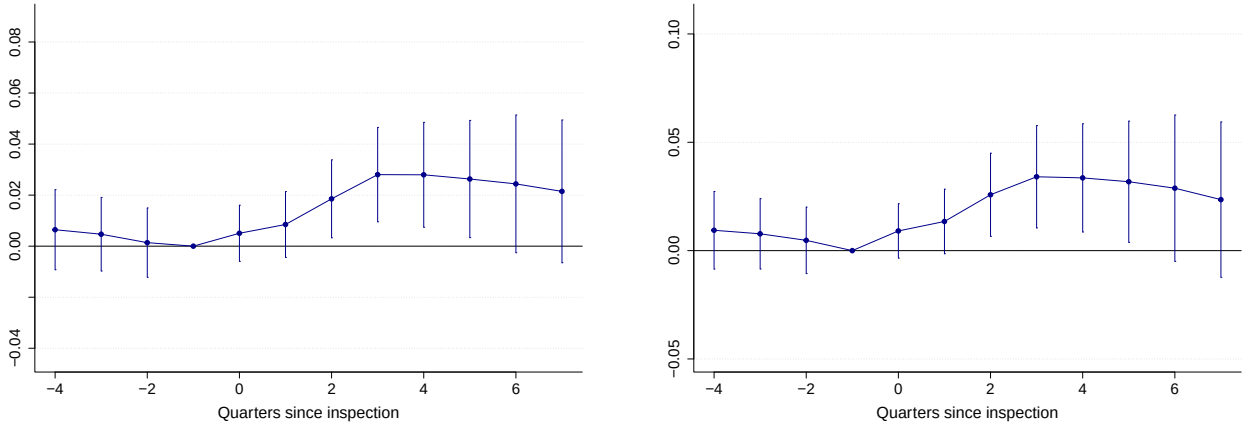
Figure A.13: Effect on hours worked



(a) Total Hours Worked (b) Avg hs worked blue-collar (c) Avg hs worked white-collar

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. In panel (a), the outcome is log of total hours worked. In panel (b) the outcome variable is log average hours worked among blue-collar workers. In panel (c), the outcome variable is log average hours worked among 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 in EMIM. Control firms are those that are never treated or not yet treated until $h = 11$. Standard errors are clustered at the firm level.

Figure A.14: Effect on Log Workers - Quarterly Outcomes

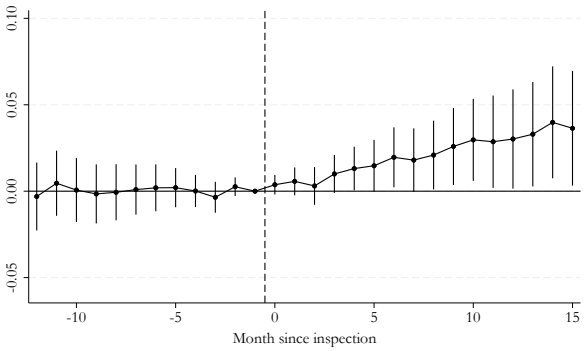


(a) Log Total Workers

(b) Log Total Production Workers

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the quarterly level for $h \in [-4:7]$. In panel (a), the outcome variable is total workers. In panel (b), the outcome is the total number of blue-collar workers. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

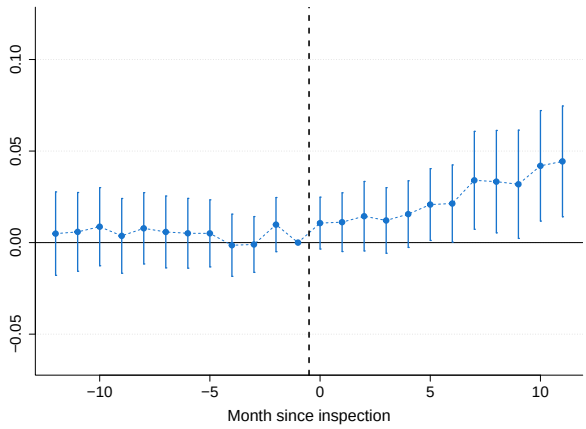
Figure A.15: Effect on Log Workers - Robustness



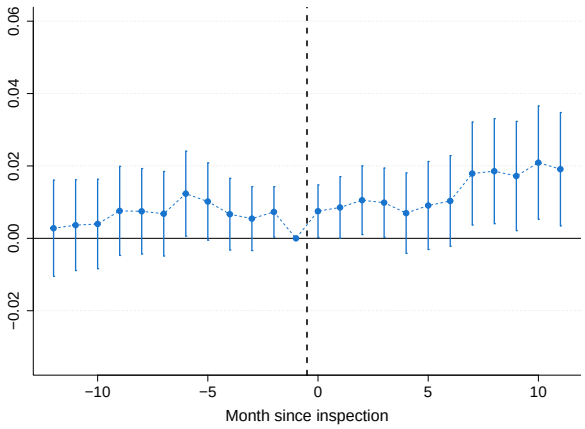
(a) Log Total Workers of Firm x State

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:15]$. The outcome is the log of total workers at the firm-by-state. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level. [Back to Section 6.3.1]

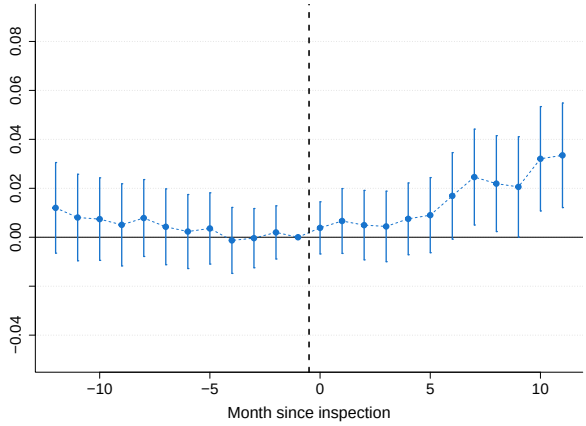
Figure A.16: Effect on Log Total Production Workers - Robustness



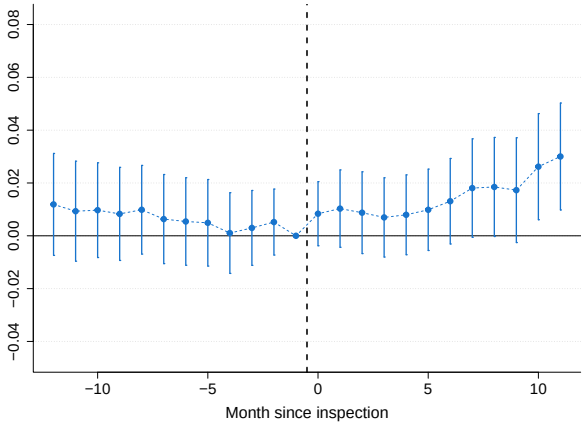
(a) Only Firms with One Establ. in each State



(b) Years 2017 - 2019



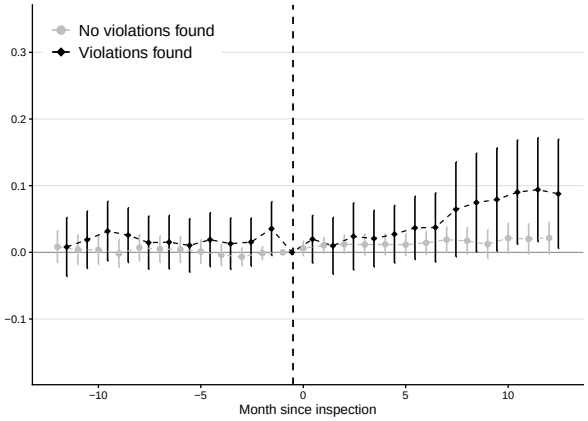
(c) Random. Grp. with broader Sector x Date FEs



(d) Propensity Score Quantile x Date Fixed Effects

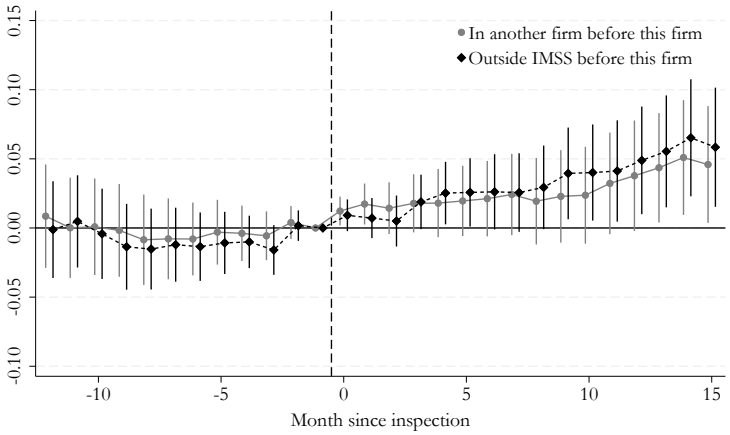
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. The outcome is total blue-collar workers. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those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

Figure A.17: Heterogeneous effects on Blue-Collar Workers by Violations



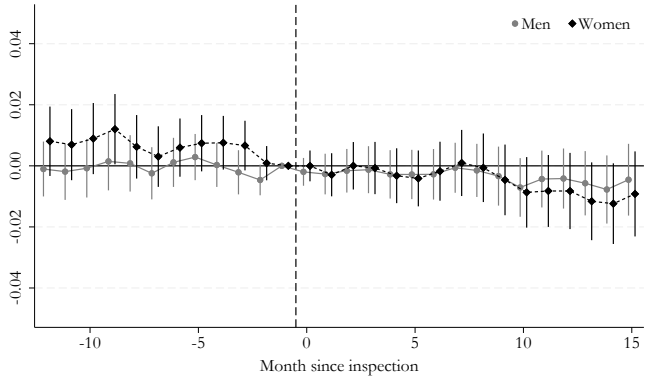
Notes: This figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:11]$ on total blue-collar workers. The figure reports separate estimates for establishments with and without violations detected during the inspection. Grey circles denote establishments without violations, while black diamonds denote establishments with at least one violation. 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: Log Total Workers by Type of Hire
(Monthly Admin Data)



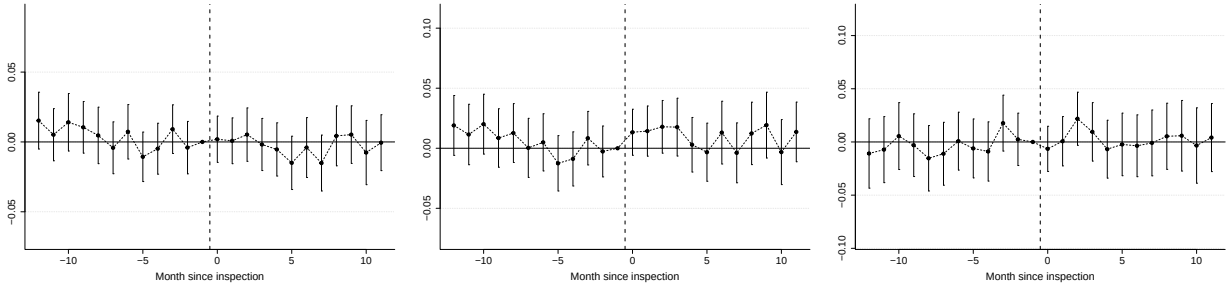
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [12:15]$. The outcome variable is the log stock of workers who joined the firm after December 2015 and are classified by whether they previously worked in another formal IMSS firm (black diamonds) or came from outside IMSS (grey dots). Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019 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.19: Log Average Wage of Men and Women



Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:15]$. The outcome variable is the log average daily wage of men (grey dots) and women (black diamonds), respectively. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019 in IMSS. Control firms are those untreated for the whole period. Standard errors are clustered at the firm level.

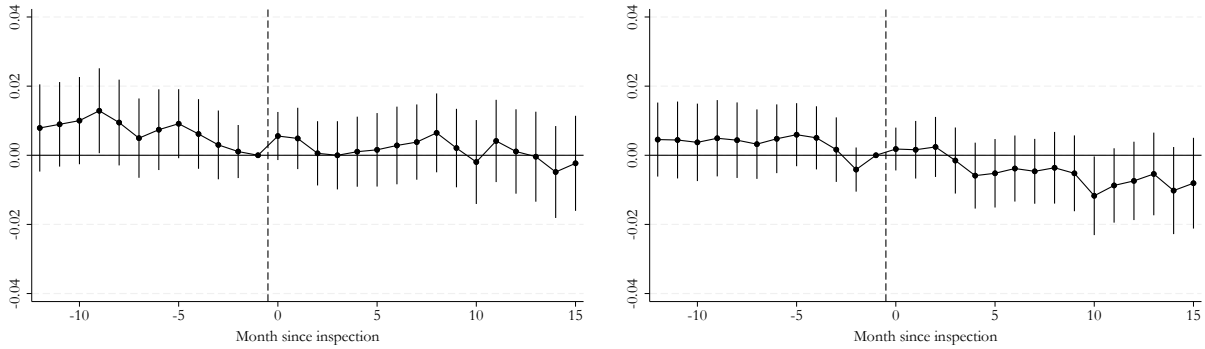
Figure A.20: Effect on Log Average Labor Costs for Blue- and White-collar Workers



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

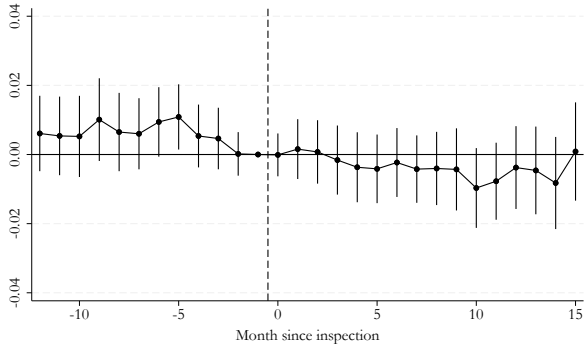
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:11]$. In panel (a), the outcome variable is the log of average labor costs. Labor costs encompass wages, social security contributions, and expenses associated with outsourced workers. In panel (b), the outcome variable is log average labor costs for blue-collar workers at the firm level. In panel (c), the outcome is log average labor costs for white-collar workers at the firm level. Regressions are estimated based on a balanced sample of firms from EMIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

Figure A.21: Effect on Firm Wage Percentiles

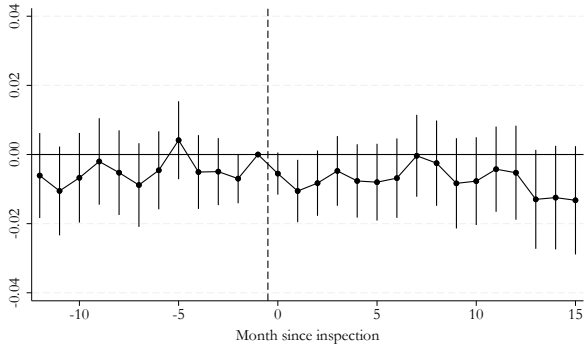


(a) Log Wage P10

(b) Log Wage P25



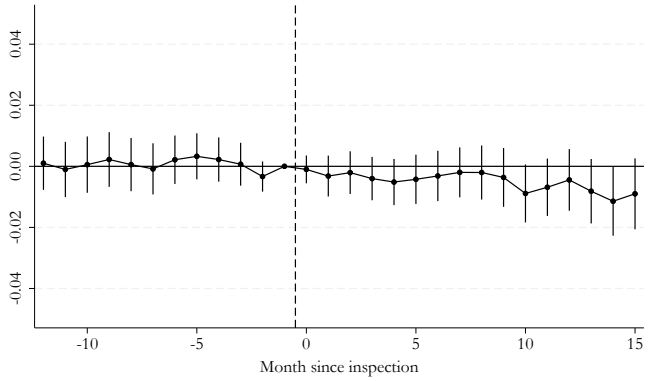
(c) Log Wage P50



(d) Log Wage P90

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:15]$. In panel (a), (b), (c), (d) the outcome variable is the log of 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 2020. 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.

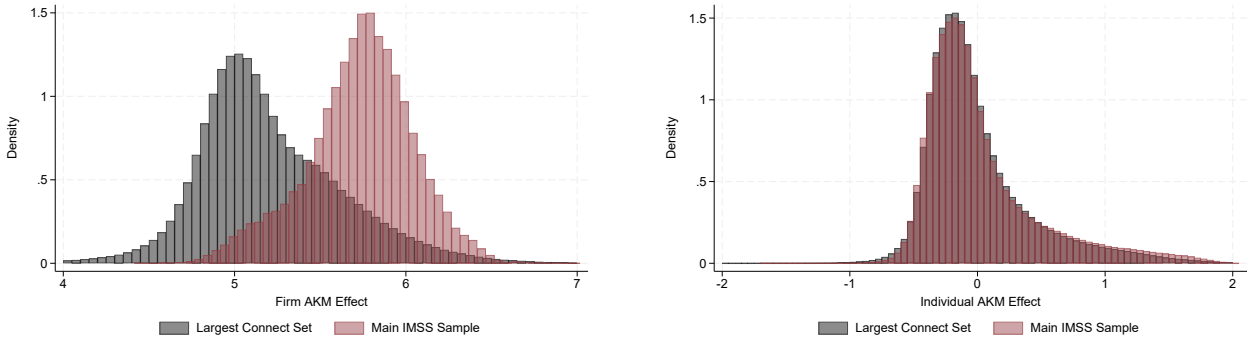
Figure A.22: Effect on Average Firm Wage - Robustness



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

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:15]$ at the firm x state level. Regressions are estimated based on a balanced sample of firms from IMSS between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between February 2018 and December 2019 in IMSS. Control firms are those untreated for the whole period. Standard errors are clustered at the firm x state level in panel.

Figure A.23: Distribution of firm and worker fixed effects

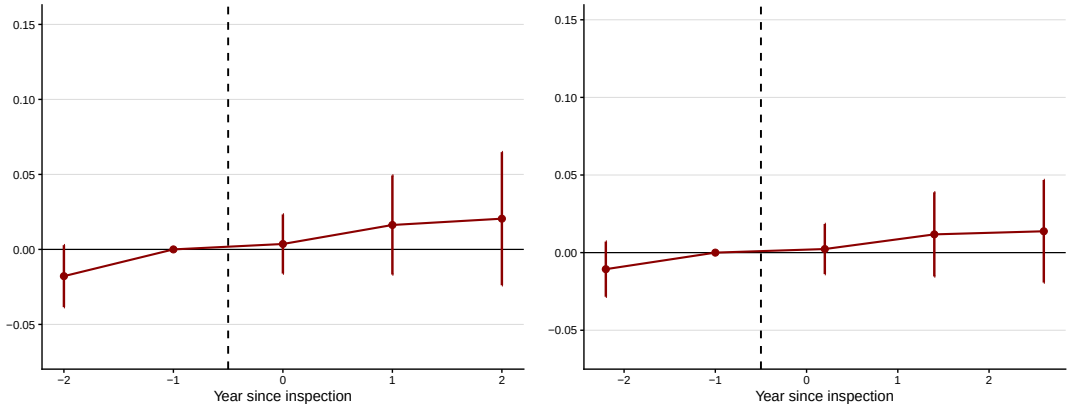


(a) Firm Fixed Effects

(b) Worker Fixed Effects

Notes: The figure compares the distributions of estimated firm and worker fixed effects from the time-varying AKM model between the largest connected set and the sample of large manufacturing firms in the IMSS inspection sample for the period 2017–2020. Panel (a) reports the distribution of firm fixed effects based on 3.7 million establishment-year observations in the largest connected set and 37,644 establishment-year observations in the IMSS sample. Panel (b) reports the distribution of worker fixed effects based on 100.2 million worker-year observations in the largest connected set and 18.4 million worker-year observations in the main IMSS sample. Histograms are normalized to integrate to one within each sample. Fixed effects are estimated using Equation (10).

Figure A.24: Effect on Revenue and Production Costs per worker

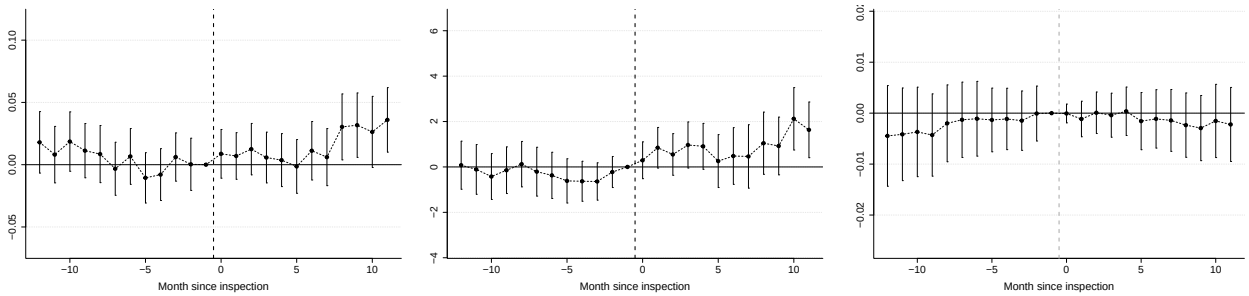


(a) Log revenue per worker

(b) Log production costs per worker

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the year level for $h \in [-2:2]$. In panel (a), the outcome variable is log of yearly revenue per worker. In panel (b) the outcome is log of total production costs per worker, where total production costs are the sum of total costs of goods and services in production and total labor costs. Regressions are estimated based on a balanced sample of firms from EAIM. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

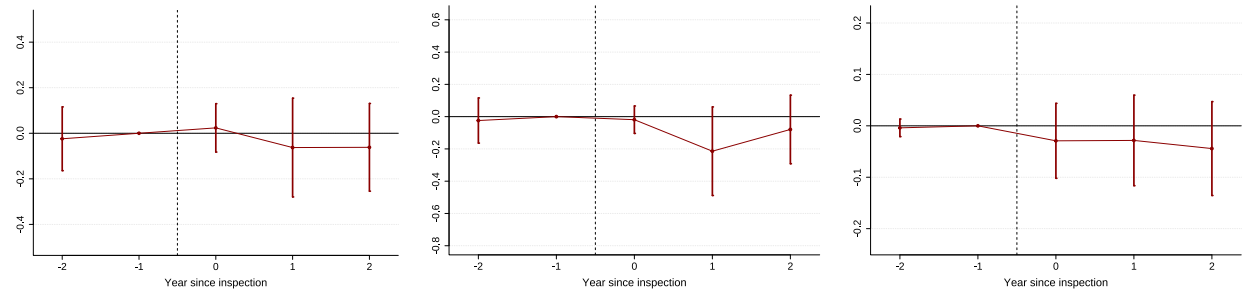
Figure A.25: Effect on Total Labor Costs, Plant Capacity Used and Outsourcing



(a) Log Total Labor Costs (b) Share of plant capacity used (c) Share of workers outsourced

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:11]$. The outcome variable is total labor costs in panel (a), percent of plant capacity used in panel (b), and share of workers outsourced in panel (c). Regressions are estimated based on a balanced sample of firms from EMIM. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those that are never treated or not yet treated until H_{max}. Standard errors are clustered at the firm level.

Figure A.26: Effect on Profits over Value Added

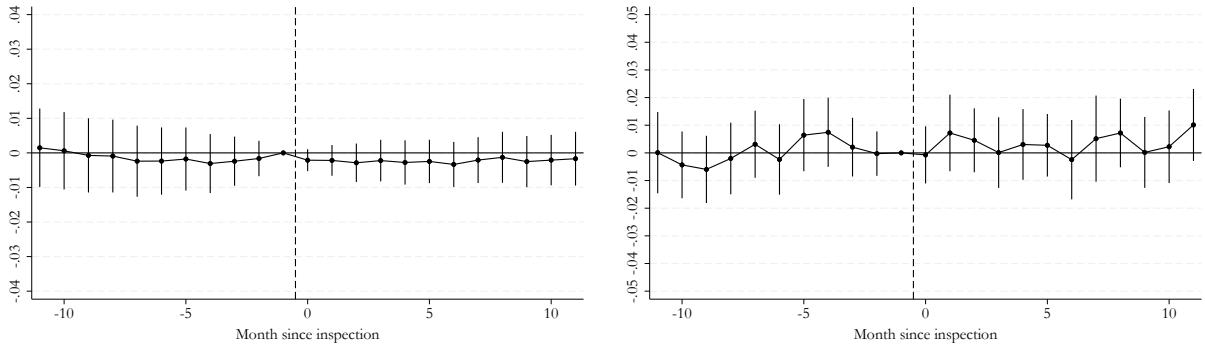


All firms Violating firms Large firms

Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the yearly level for $h \in [-2:2]$. The outcome variable is yearly profits divided by yearly value-added. Regressions are estimated based on a balanced sample of firms from EAIM between 2017 and 2020. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Control firms are those untreated until period h. Standard errors are clustered at the firm level. Panel (b) restricts the treated firms to those found violating during the inspection. In Panel (c) we restrict the sample to large firms with above-median total workers in 2017, which tend to have lower profit volatility than small firms (Herskovic et al., 2020). [Back to Section 6.3.4]

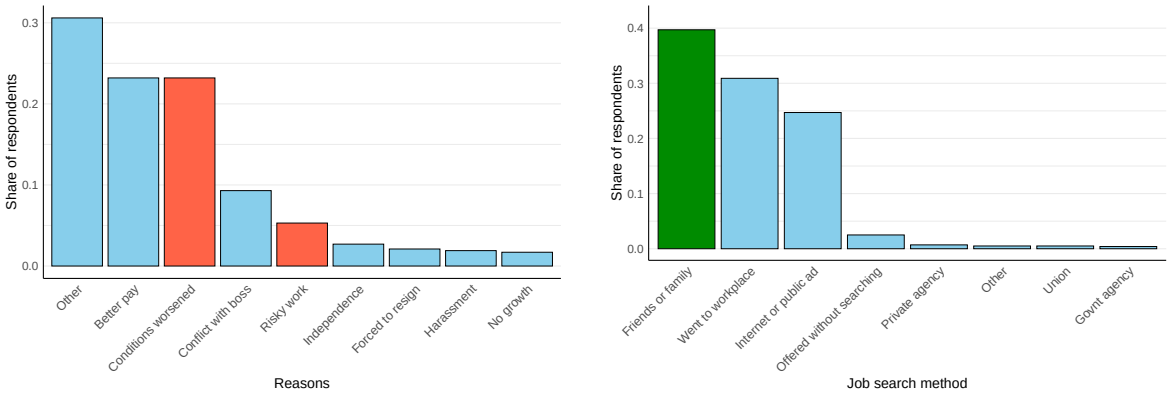
Figure A.27: Additional Worker-level event-study estimates around inspections

(a) Worker Transitions into and out of IMSS (b) Log Wage for Workers Employed at Baseline Firm



Notes: This figure shows estimates of β^h and 95% confidence intervals from estimating Equation (11) at the worker-month level for $h \in [-12, 11]$. In panel (a), the outcome is transitioning from out of IMSS in periods before t-1 and leaving IMSS in periods after t-1. In Panel (b) the outcomes is conditional formal daily wages if employed at the same firm as in t-1. Regressions are estimated using a balanced monthly panel of workers ever employed between 2017 and 2020 in one of the establishments in the experimental sample. Treated workers are those employed in the month before inspection at an establishment receiving its first inspection in the following month between February 2018 and December 2019. Control workers are employed in the same calendar month at establishments in the same randomization group that have not yet been inspected over the relevant event-study horizon. Workers not observed in IMSS in a given month are coded as not formally employed, and unconditional wages are set to zero in those months. Standard errors are clustered at the baseline establishment level.

Figure A.28: Job Quits and Search



(a) Reasons for Quitting Previous Job (b) Search Method for Current Job

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

A.2 Tables Appendix

Table A.1: Descriptive Statistics for Main EMIM Outcomes

	(1) All	(2) Ever inspected 2017–2022m6	(3) Ever random insp. 2017–2022m6	(4) First random insp. 2018–2019
Total Workers	357.57	416.31	426.19	416.12
	(590.99)	(596.25)	(607.88)	(596.19)
Avg. Real Daily Wage	464.55	514.14	515.17	491.96
	(1026.66)	(1004.96)	(1000.16)	(791.44)
Avg. Daily Hours Worked	10.56	10.01	9.98	9.96
	(3.50)	(1.87)	(1.63)	(1.40)
Share Production Workers	0.77	0.77	0.77	0.76
	(0.18)	(0.18)	(0.18)	(0.18)
Share Outsourced Workers	0.22	0.19	0.16	0.16
	(0.39)	(0.37)	(0.35)	(0.35)
Labor Market Share (2-digit Industry) x Municipality	0.05	0.05	0.06	0.06
	(0.12)	(0.12)	(0.13)	(0.13)
Labor Market Share (3-digit Industry) x Municipaliy	0.14	0.15	0.16	0.16
	(0.23)	(0.22)	(0.23)	(0.23)
Share of workers provided training	0.32	0.39	0.42	0.41
	(0.39)	(0.40)	(0.40)	(0.40)
Monthly Revenue (Thousands of Real MXN)	56061	66064	68246	60935
	(127645)	(136870)	(140479)	(127113)
Monthly Revenue per worker (Thousands of Real MXN)	251	281	322	290
	(2829)	(3620)	(4799)	(3134)
Time period	2017	2017	2017	2017
Observations	8,864	4,823	2,552	979
Establishments	8,864	4,823	2,552	979

Notes: This table reports establishment-level means and standard deviations for main establishment characteristics in the EMIM data. Standard deviations are reported in parentheses below the corresponding means. All columns use establishment-level averages in 2017. Column (1) includes all establishments. Column (2) restricts the sample to establishments inspected at least once. Column (3) restricts the sample to establishments inspected at least once randomly. Column (4) restricts to establishments whose first random inspection occurred in 2018–2019. Monetary variables are measured in real Mexican pesos normalized to 2018.

Table A.2: Descriptive Statistics for Main IMSS Outcomes

	(1) All	(2) Ever inspected 2017–2022m6	(3) Ever random insp. 2017–2022m6	(4) First random insp. 2018m2–2019m12	(5) First random insp. 2018m2–2019m12
Establishment Age in Month since 2004m11	119.52	125.69	133.16	133.49	147.86
	(48.14)	(44.80)	(38.65)	(38.53)	(38.70)
Northern Zone Region	0.18	0.19	0.20	0.23	0.23
	(0.38)	(0.39)	(0.40)	(0.42)	(0.42)
Total Workers	385.47	480.71	528.62	537.01	569.39
	(730.60)	(852.11)	(945.14)	(864.90)	(930.92)
Log Total Workers	5.18	5.44	5.52	5.56	5.58
	(1.20)	(1.17)	(1.17)	(1.18)	(1.23)
Avg. Real Daily Wage	401.97	413.21	420.36	423.73	437.05
	(233.98)	(211.54)	(210.34)	(214.04)	(219.05)
Avg. Log Real Daily Wage	5.81	5.86	5.89	5.89	5.98
	(0.53)	(0.48)	(0.46)	(0.47)	(0.46)
Share Minimum Wage (Bottom Coded)	0.01	0.01	0.00	0.00	0.01
	(0.07)	(0.05)	(0.04)	(0.04)	(0.05)
Share Top Coded Wage	0.02	0.02	0.02	0.02	0.02
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
Share Women	0.32	0.32	0.32	0.32	0.32
	(0.20)	(0.21)	(0.21)	(0.21)	(0.21)
Share Permanent Workers	0.84	0.84	0.84	0.85	0.84
	(0.28)	(0.27)	(0.26)	(0.26)	(0.26)
Avg. Worker Age in Years	36.31	36.34	36.64	36.74	37.29
	(3.97)	(3.89)	(3.81)	(3.77)	(3.75)
Avg. Worker Tenure in Months	47.17	50.34	54.10	54.43	57.93
	(27.52)	(26.92)	(25.95)	(25.93)	(26.06)
Avg. Worker Experience in Months	89.85	91.65	93.92	94.54	101.08
	(22.32)	(21.52)	(20.78)	(20.88)	(22.21)
Avg. Worker Year of IMSS Registration	2000.80	2000.69	2000.37	2000.26	2001.06
	(3.99)	(3.92)	(3.80)	(3.76)	(3.78)
Total Separations since Last Month	21.19	25.30	26.25	26.16	28.99
	(58.52)	(68.22)	(72.29)	(63.29)	(66.24)
Total Hires	23.33	27.95	28.60	28.46	31.73
	(71.59)	(81.56)	(83.37)	(70.24)	(80.20)
Time period	2017	2017	2017	2017	Prev. month
Observations	112,992	69,240	34,356	12,828	1,069
Establishments	9,416	5,770	2,863	1,069	1,069

Notes: This table reports establishment-level means and standard deviations for main establishment and their workers' characteristics in the IMSS data. Standard deviations are reported in parentheses below the corresponding means. Columns (1)–(4) report establishment-level averages across all months in 2017. Column (1) includes all establishments. Column (2) restricts the sample to establishments inspected at least once between 2017 and 2022m6. Column (3) restricts the sample to establishments inspected at least once randomly between 2017 and 2022m6. Column (4) restricts to the main sample of establishments whose first random inspection occurred between 2018m2 and 2019m12, measured using their monthly observations in 2017. Column (5) reports the same sample as column (4), measured in the month before the first random inspection instead of all months in 2017. Monetary variables are measured in real Mexican pesos normalized to July 2018.

Table A.3: Randomization Check: Ordinary Inspections

Panel A: EMIM

	Total workers	Avg hs worked	Inhouse workers	Revenue	Revenue p.w.	Avg wage	Share blue collar	Share outsourced
Panel A1: Without randomization group fixed effects								
Inspected	66.56**	-0.0090***	88.30***	12,785.9*	57.43	0.5938	0.0031	-0.0612***
	(28.44)	(0.0015)	(26.35)	(7,751.0)	(82.06)	(1.286)	(0.0056)	(0.0124)
Observations	285,841	284,564	284,564	281,093	281,057	239,368	284,564	284,564
Panel A2: With randomization group fixed effects								
Inspected	25.77	-0.0009	18.63	6,169.9	95.15	-0.3118	0.0005	-0.0013
	(30.21)	(0.0010)	(27.25)	(6,951.2)	(109.7)	(0.4736)	(0.0054)	(0.0045)
Observations	255,428	254,029	254,029	250,927	250,891	214,173	254,029	254,029

Panel B: IMSS

	Total workers	Share women	Share temporary	Total separations	Hires	Avg daily wage	P10 salaries	P50 salaries
Panel B1: Without randomization group fixed effects								
Treatment	128.0***	-0.00781	-0.00950	1.019	0.548	21.36***	9.450**	14.62**
	(40.58)	(0.00607)	(0.00734)	(2.096)	(1.819)	(6.680)	(4.098)	(6.448)
Observations	277,776	277,776	277,776	277,776	277,776	277,776	277,776	277,776
Panel B2: With randomization group fixed effects								
Treatment	18.50	-0.000645	-0.0121	-2.772	-4.494*	2.215	1.236	1.075
	(43.00)	(0.00510)	(0.00812)	(2.837)	(2.554)	(6.089)	(3.423)	(5.878)
Observations	245,825	245,825	245,825	245,825	245,825	245,825	245,825	245,825

Panel C: Economic Census

	Total workers	Avg hs worked	Revenue	Revenue p.w.	Avg labor cost	Profit	Share outsourced	VA p.w.
Panel C1: Without randomization group fixed effects								
Inspected	-13.85	-0.0049	-17,365.8	82.49	-3.029	-31,378.8	0.0032	6.700
	(11.38)	(0.0109)	(134,126.7)	(107.3)	(2.009)	(47,402.6)	(0.0060)	(39.25)
Observations	137,520	137,520	137,520	137,520	137,520	137,520	137,520	137,160
Panel C2: With randomization group fixed effects								
Inspected	-5.971	-0.0037	31,754.7	-8.692	-0.7778	-18,611.7	0.0013	-35.09
	(11.70)	(0.0126)	(131,407.1)	(94.52)	(1.779)	(49,595.7)	(0.0027)	(45.51)
Observations	125,820	125,820	125,820	125,820	125,820	125,820	125,820	125,496

Notes: This Table shows the results from estimating a regression using the sample of firms from EMIM (Panel A), IMSS (Panel B), and the sample of inspected firms from the Economic Census (Panel C), 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. Inspections from 2019 to 2021m6 are considered to correctly account for 'months since last inspection' control. The regressions in panel A1,B1,C1 include state-by-year-by-month fixed effects. The regressions in panel A2,B2,C2 include randomization-group-by-year-by-month fixed effects. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

Table A.4: EMIM Placebo Randomization Check: Extraordinary Inspections

	Total workers	Avg hs worked	Inhouse workers	Revenue	Revenue p.w.	Avg wage	Share blue collar	Share outsourced
Panel A: Without randomization group fixed effects								
Treatment	147.9***	-0.0112***	169.4***	19,264.3***	-58.49**	0.4381	0.0062.	-0.0679***
	(18.22)	(0.0013)	(17.53)	(3,999.3)	(28.13)	(0.4564)	(0.0036)	(0.0080)
Observations	285,841	284,564	284,564	281,093	281,057	239,368	284,564	284,564
Panel B: With randomization group fixed effects								
Treatment	70.63***	-0.0010	63.40***	8,900.3***	-28.99	0.5049**	0.0033	0.0066***
	(13.38)	(0.0007)	(12.33)	(2,655.6)	(20.62)	(0.2239)	(0.0020)	(0.0020)
Observations	255,428	254,029	254,029	250,927	250,891	214,173	254,029	254,029

Notes: This Table shows the results from estimating a regression using the sample of firms from EMIM 2019 to 2022, where we regress a firm-level outcome measured in month $t-1$ indicated in the different columns on a binary variable indicating an extraordinary inspection in month t. The regressions in panel A include state-by-date fixed effects. The regressions in panel B include randomization-group-by-date fixed effects. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

Table A.5: Census Placebo Randomization Check: Extraordinary Inspections

	Total workers	Avg hs worked	Revenue	Revenue p.w.	Avg labor cost	Profit	Share outsourced	VA p.w.
Panel A: Without randomization group fixed effects								
Treatment	64.03***	0.0165***	267,949.5**	154.8***	7.511***	76,980.9**	-0.0184***	40.42**
	(9.393)	(0.0062)	(106,266.4)	(45.62)	(1.209)	(27,995.6)	(0.0037)	(19.07)
Observations	266,616	266,616	266,616	266,616	266,616	266,616	266,616	265,716
Panel B: With randomization group fixed effects								
Treatment	48.42***	0.0127**	180,852.7*	84.55*	4.790***	57,106.5*	0.0005	21.09
	(7.521)	(0.0061)	(106,026.9)	(43.33)	(1.065)	(27,467.0)	(0.0013)	(18.02)
Observations	251,424	251,424	251,424	251,424	251,424	251,424	251,424	250,452

Notes: This Table shows the results from estimating a regression using the sample of firms from the Economic Census, 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. Inspection from 2019 to 2022 are considered. The regressions in panel A include state-by-date fixed effects. The regressions in panel B include randomization-group-by-date fixed effects. Standard errors are clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

Table A.6: Correlations between model-selected variables and violation probability. Robustness

Dep Var:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Violation						Violation + Sanction			
1st set of empirical proxies for model parameters										
Profits per worker	-0.037	-0.031	-1.513	0.032	0.042	-0.781	-0.064**	-0.047*	0.006	0.030
	(0.066)	(0.045)	(1.024)	(0.087)	(0.058)	(0.699)	(0.028)	(0.028)	(0.030)	(0.033)
Share women	-0.032***	-0.031***	-0.277*	-0.036***	-0.037***	-0.106***	-0.028***	-0.029***	-0.032***	-0.034***
	(0.010)	(0.010)	(0.142)	(0.011)	(0.011)	(0.034)	(0.009)	(0.010)	(0.010)	(0.011)
Firm Market Share in LLM	0.021**	0.016	0.008	0.020**	0.014	0.079**	0.026***	0.020**	0.028***	0.021**
	(0.009)	(0.010)	(0.059)	(0.009)	(0.010)	(0.034)	(0.009)	(0.009)	(0.009)	(0.010)
Continuous inspection prob	-	-0.044	-	-	-0.047	-	-	-0.040	-	-0.047
		(0.053)			(0.054)			(0.049)		(0.050)
Q2 inspection prob	-0.034	-	-	-0.032	-	-	-0.007	-	-0.007	-
	(0.032)			(0.032)			(0.029)		(0.029)	
Q3 inspection prob	-0.026	-	-	-0.025	-	-	-0.011	-	-0.011	-
	(0.032)			(0.032)			(0.029)		(0.029)	
Q4 inspection prob	-0.069**	-	-	-0.068**	-	-	-0.046*	-	-0.047*	-
	(0.031)			(0.032)			(0.028)		(0.028)	
Q5 inspection prob	-0.052*	-	-	-0.049	-	-	-0.032	-	-0.032	-
	(0.032)			(0.032)			(0.029)		(0.029)	
Observations	1,997	1,997	1,815	1,987	1,987	1,805	1,812	1,812	1,803	1,803
Double-Lasso controls	No	No	No	Yes	Yes	Yes	No	No	Yes	Yes
2nd set of empirical proxies for model parameters										
VA per worker	-0.033	-0.034	-0.063	0.023	0.027	-0.137	-0.059**	-0.046	0.003	0.020
	(0.056)	(0.039)	(0.947)	(0.072)	(0.046)	(0.496)	(0.026)	(0.027)	(0.025)	(0.027)
Share women	-0.030***	-0.029***	-0.273*	-0.033***	-0.036***	-0.104***	-0.025***	-0.027***	-0.029***	-0.032***
	(0.010)	(0.010)	(0.143)	(0.011)	(0.012)	(0.035)	(0.009)	(0.010)	(0.010)	(0.011)
LLM Herfindahl Index	0.021**	0.017	0.027	0.020**	0.015	0.043	0.027***	0.021**	0.026***	0.021*
	(0.010)	(0.011)	(0.083)	(0.010)	(0.011)	(0.044)	(0.009)	(0.011)	(0.010)	(0.011)
Continuous inspection prob.	-	-0.042	-	-	-0.044	-	-	-0.039	-	-0.043
		(0.052)			(0.053)			(0.048)		(0.049)
Q2 inspection prob	-0.033	-	-	-0.031	-	-	-0.005	-	-0.005	-
	(0.032)			(0.032)			(0.029)		(0.029)	
Q3 inspection prob	-0.023	-	-	-0.022	-	-	-0.006	-	-0.007	-
	(0.032)			(0.032)			(0.029)		(0.029)	
Q4 inspection prob	-0.067**	-	-	-0.066**	-	-	-0.045	-	-0.045	-
	(0.031)			(0.032)			(0.028)		(0.029)	
Q5 inspection prob	-0.050	-	-	-0.045	-	-	-0.029	-	-0.028	-
	(0.032)			(0.032)			(0.029)		(0.029)	
Observations	1,997	1,997	1,815	1,987	1,987	1,805	1,812	1,812	1,803	1,803
Double-Lasso controls	No	No	No	Yes	Yes	Yes	No	No	Yes	Yes
Probability re-weighting	No	No	Yes	No	No	Yes	No	No	No	No
State and Sector FE	No	Yes	No	No	Yes	No	No	Yes	No	Yes

Notes: This table shows the coefficients of multivariate regressions of a binary variable indicating whether an inspected firm was found violating on a firm characteristic. In Columns 1 to 6, the outcome variable is whether the firm was found in violation. In Columns 7 to 10, the outcome is 1 if the firm was found violating and a sanction procedure was initiated (firms violating without a sanction procedure are excluded from this estimation). The first panel uses a first set of empirical proxies for model parameters, while the second panel replaces these with alternative proxies. The regressions are using a sample of manufacturing firms from the 2018 Economic Census with more than 15 workers that received an ordinary inspection between 2019 and 2021. Firm characteristics correspond to the values measured during the 2018 Economic Census. Columns 4-6 and 9-10 include control variables selected via post selection double lasso. Columns 1,4,7,9 control for predicted inspection probability quintile fixed-effects rather than continuous p. Columns 2,5,8,10 control for state and 2-digit NAICS sector FE. Columns 3 and 6 estimate the regression using the inverse of the probability of inspection as a weight (Hirano et al., 2003). *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$. [Back to Section 5]

Table A.7: Correlation between firm characteristics and violation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable						
	Violation				Violation & sanction		
	Control for propensity score			Prop score weighting	No controls	Control for prop score	Prop score weighting
Regressions with firm specific characteristics							
Firm age	0.0030	-0.0016	0.0034	-0.0016	-0.0008	-0.0006	-0.1329**
	(0.0094)	(0.0088)	(0.0093)	(0.0088)	(0.0095)	(0.0091)	(0.0578)
Firm size	0.0093	0.0002	0.0077	-0.0010	0.0207	0.0129	-0.5901*
	(0.0283)	(0.0270)	(0.0285)	(0.0272)	(0.0291)	(0.0282)	(0.3239)
Capital	0.0127	0.0200	0.0122	0.0200	0.0177	0.0200	-0.4474*
	(0.0205)	(0.0201)	(0.0205)	(0.0200)	(0.0207)	(0.0199)	(0.2329)
Revenue	-0.0048	-0.0017	-0.0050	-0.0018	-0.0030	-0.0018	-0.0715
	(0.0038)	(0.0039)	(0.0039)	(0.0039)	(0.0039)	(0.0039)	(0.0615)
Profit	-0.0085**	-0.0057*	-0.0082**	-0.0054*	-0.0057	-0.0044	-0.1738
	(0.0034)	(0.0030)	(0.0034)	(0.0030)	(0.0037)	(0.0032)	(0.1170)
Profit per worker	-0.0314	-0.0607**	-0.0292	-0.0588**	-0.0295	-0.0481*	-1.450
	(0.0672)	(0.0267)	(0.0691)	(0.0257)	(0.0456)	(0.0274)	(1.148)
Value added per worker	-0.0225	-0.0518**	-0.0211	-0.0508**	-0.0288	-0.0447*	0.4096
	(0.0597)	(0.0231)	(0.0614)	(0.0225)	(0.0395)	(0.0252)	(1.534)
Export share	0.0004	-0.0006	0.0007	-0.0004	0.0034	-0.0004	0.0934
	(0.0059)	(0.0053)	(0.0059)	(0.0054)	(0.0066)	(0.0061)	(0.0782)
Informal competition is a problem	0.0213*	0.0060	0.0225*	0.0070	0.0090	0.0024	-0.0029
	(0.0121)	(0.0111)	(0.0121)	(0.0111)	(0.0119)	(0.0114)	(0.0836)
Regressions with worker characteristics							
Training costs per worker	-0.0056	-0.0063*	-0.0060	-0.0066*	-0.0028	-0.0042	-0.0115
	(0.0055)	(0.0034)	(0.0057)	(0.0034)	(0.0049)	(0.0037)	(0.0072)
Share workers receiving training	-0.0250***	-0.0272***	-0.0246***	-0.0270***	-0.0280***	-0.0250***	-0.0667
	(0.0091)	(0.0081)	(0.0091)	(0.0081)	(0.0091)	(0.0083)	(0.1002)
Avg worker age	0.0099	0.0046	0.0105	0.0049	0.0033	0.0010	0.0897
	(0.0116)	(0.0104)	(0.0116)	(0.0104)	(0.0120)	(0.0110)	(0.0927)
Share informal workers	-0.0120	0.0006	-0.0122	0.0004	-0.0114	0.0005	0.0014
	(0.0127)	(0.0117)	(0.0127)	(0.0117)	(0.0125)	(0.0116)	(0.0418)
Share workers women	-0.0324***	-0.0280***	-0.0326***	-0.0284***	-0.0306***	-0.0284***	-0.2757*
	(0.0101)	(0.0092)	(0.0101)	(0.0092)	(0.0104)	(0.0095)	(0.1428)
Share workers with higher education	0.0040	0.0104	0.0039	0.0106	0.0032	0.0112	-0.1436
	(0.0118)	(0.0106)	(0.0118)	(0.0106)	(0.0119)	(0.0108)	(0.1296)
Average wage	-0.0035	-0.0107	-0.0028	-0.0102	-0.0094	-0.0149*	-0.2292*
	(0.0094)	(0.0076)	(0.0094)	(0.0076)	(0.0100)	(0.0079)	(0.1225)
Average hours worked	-0.0025	-0.0016	-0.0021	-0.0012	-0.0017	0.0040	0.6066**
	(0.0207)	(0.0192)	(0.0207)	(0.0192)	(0.0208)	(0.0194)	(0.2406)
Share outsourced workers	0.0467***	0.0498***	0.0463***	0.0501***	0.0539***	0.0495***	0.0458
	(0.0111)	(0.0108)	(0.0111)	(0.0108)	(0.0107)	(0.0106)	(0.0906)
Regressions with local labor market characteristics							
Employment concentration in LLM (total worker HHI)	0.0236**	0.0283***	0.0235**	0.0288***	0.0190*	0.0234**	0.0682
	(0.0095)	(0.0093)	(0.0095)	(0.0093)	(0.0110)	(0.0107)	(0.0900)
Wage-bill concentration in LLM (wagebill HHI)	0.0206**	0.0234***	0.0210***	0.0242***	0.0160	0.0195**	0.0803
	(0.0081)	(0.0078)	(0.0081)	(0.0078)	(0.0100)	(0.0095)	(0.0614)
Labor market share in the LLM	0.0211**	0.0257***	0.0209**	0.0259***	0.0157	0.0201**	0.0061
	(0.0087)	(0.0086)	(0.0086)	(0.0085)	(0.0096)	(0.0094)	(0.0774)
Wage-bill share in the LLM	0.0159**	0.0196***	0.0162**	0.0201***	0.0084	0.0121	-0.0371
	(0.0078)	(0.0076)	(0.0078)	(0.0076)	(0.0090)	(0.0087)	(0.0886)
Share of informal workers in LLM	0.0090	0.0025	0.0098	0.0035	0.0017	-0.0037	0.1312
	(0.0133)	(0.0125)	(0.0134)	(0.0126)	(0.0193)	(0.0186)	(0.1229)
Control for $\hat{p}$	Yes	No	Yes	No	Yes	No	Yes
$\hat{p}$ quintile FE	No	Yes	No	No	No	Yes	No
Probability re-weighting	No	No	No	Yes	No	No	No
State and Sector FE	No	No	Yes	No	No	No	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 4, the outcome variable is whether the firm was found in violation (N observations = 1,998). In Columns 5 to 7, the outcome is 1 if the firm was found violating and a sanction procedure was initiated (firms violating without a sanction procedure are excluded from this estimation N observations = 1,813). The regressions are estimated using a sample of manufacturing firms from the 2018 Economic Census with more than 15 workers that received an ordinary inspection between 2019 and 2021. Firm characteristics correspond to the values measured during the 2018 Economic Census. Columns 4 estimates the regression using the inverse of the estimated probability of inspection as a weight (Hirano et al., 2003). *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$. Back to Section 5

Table A.8: Summary Statistics - Firm Survey on Regulatory Quality and Government Impact on Firms (ENCRIGE) 2020

Variable	Mean
N	2666
Number of workers	505
Inspected by STPS in 2020	0.311
Ever inspected by STPS	0.630
Labor Regulations are a main regulatory obstacle	0.035
STPS Labor Inspections are a main regulatory obstacle	0.009
Annual compliance cost per worker (MXN)	5556
Share of staff hired for compliance	0.036
Among firms inspected by STPS in 2020	
N	830
Number of inspections received in 2020	1.512
Forced to close after inspection	0.002
License revoked after inspection	0.006
Given warning & time limit to comply after inspection	0.08
Faced corruption during inspection	0.034
Paid bribe	0.020
Average bribe amount (MXN)	7384

Notes: This table reports summary statistics from the 2020 ENCRIGE firm survey. The sample is restricted to manufacturing firms with over 15 workers. "Inspected by STPS in 2020" is an indicator equal to one if the firm reports being inspected by the labor ministry (STPS) in 2020; "Ever inspected by STPS" indicates whether the firm has ever been inspected. The following rows include indicators for whether labor regulations or STPS inspections are reported as a main regulatory obstacle. "Annual compliance cost per worker" measures reported annual expenditures on regulatory compliance per employee, and "Share of staff hired for compliance" captures the fraction of workers dedicated to compliance activities. The bottom panel restricts the sample to firms inspected by STPS in 2020 and includes questions only asked to this subgroup of firms. "Paid bribe" indicates whether the firm reports paying a bribe, and "Average bribe amount" reports the mean conditional on paying a bribe. Monetary values are expressed in 2019 Mexican pesos (MXN). [Back to Section 6.2]

Table A.9: Summary Statistics of Survey Responses by Salary Category

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

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

B Appendix: Time-varying AKM Model

This appendix describes the construction of the worker-establishment panel used to estimate the time-varying AKM model following Lachowska et al. (2023) and provides several validity checks.

Data construction. The sample covers workers aged 17 to 64 over the period 2015–2024. We keep the employment spell within a year that has the highest earnings in that year. Then, we aggregate monthly observations to the worker-year level, leaving us with the average daily real wage, linked to the corresponding establishment. Table B.1 summarizes the sample size before and after restricting to the largest connected set. This set is defined as the largest sample in which all the firm-by-year observations are connected by worker moves.

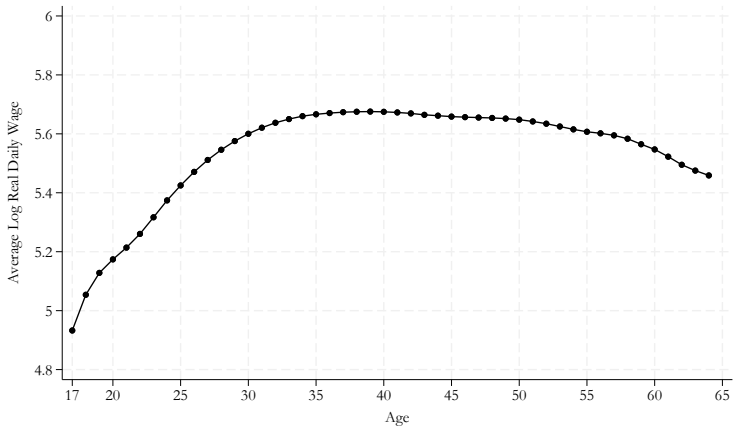
Table B.1: Original Sample and Largest Connected Set

	Original sample	Largest connected set
Worker-year observations	254,117,421	251,946,424
Unique workers	47,862,869	47,331,325
Unique establishments	2,351,120	2,040,990
Establishment-year observations	10,965,308	9,459,951
Mean log wage	5.547	5.552
Variance of log wage	0.5079	0.5076
R^2	—	0.9369
Adjusted R^2	—	0.9186

Notes: The table reports summary statistics for the full sample and the largest connected set used for estimation. This set is defined as the largest sample in which all the firm-by-year observations are connected by worker moves. Mean log wage refers to the mean of log real daily wages across worker-year observations. There is one observation per person and year for individuals aged 17 to 64 in the corresponding year. The reported R^2 and adjusted R^2 measure the explanatory power of the estimated time-varying AKM model and are computed from the regression specified in Equation (10), estimated on the largest connected set. The period is 2015-2024.

Moving to the largest connected set reduces the sample by around 2.17 million worker-year observations, or about 1 percent of the full sample. This corresponds to a reduction of approximately 0.5 million workers (1%), 0.3 million establishments (13%), and 1.5 million establishment-year observations (13%). The largest connected set therefore covers almost all workers. The model further achieves a large adjusted R^2 of 0.9186.

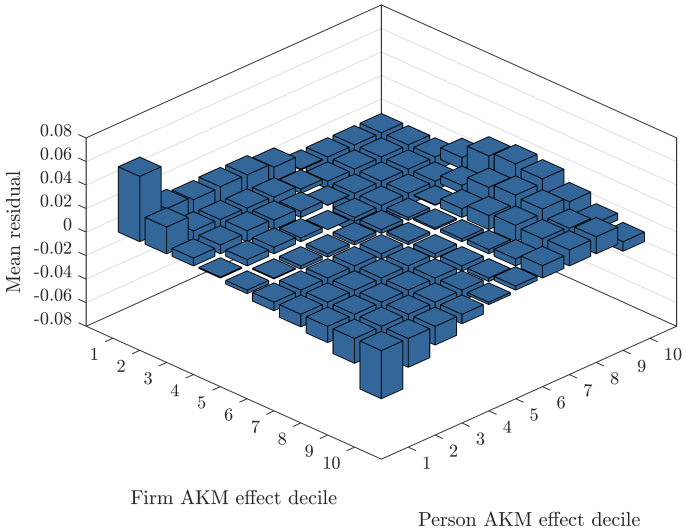
Figure B.1: Average Log Daily Wage by Age



Notes: The figure plots the average log daily real wage by age for workers aged 17–64 in IMSS over the period 2015–2024. The underlying unit of observation is a worker-month. Age is defined as the difference between the calendar year and year of birth. The number of observations is 254,117,421 for 47,862,869 workers.

Validation checks. A key assumption of the AKM framework is additivity, namely that worker and firm fixed effects enter linearly in the wage equation, without worker–firm-specific match effects. Following Card et al. (2013), we assess this assumption by plotting residuals from the TV-AKM estimated on the pooled 2015–2024 sample. The figure shows residuals for all 251 million worker-year observations in the largest connected set, grouped by 10 deciles of worker fixed effects and 10 deciles of firm fixed effects. If worker and firm effects were complementary, we would expect positive residuals for high worker fixed-effect individuals employed at high firm fixed-effect establishments. We do not observe such a pattern. However, we observe that residuals for high worker fixed-effect individuals in medium firm fixed-effect establishments are somewhat elevated, which may reflect that these workers, such as managers, receive wages above what the AKM predicts. Overall, these residuals are small in magnitude. Workers with the lowest worker fixed effects have positive residuals in low to medium firm fixed-effect establishments, which is likely driven by institutional constraints such as the minimum wage in Mexico, which increase low-skilled worker rents at low paying firms.

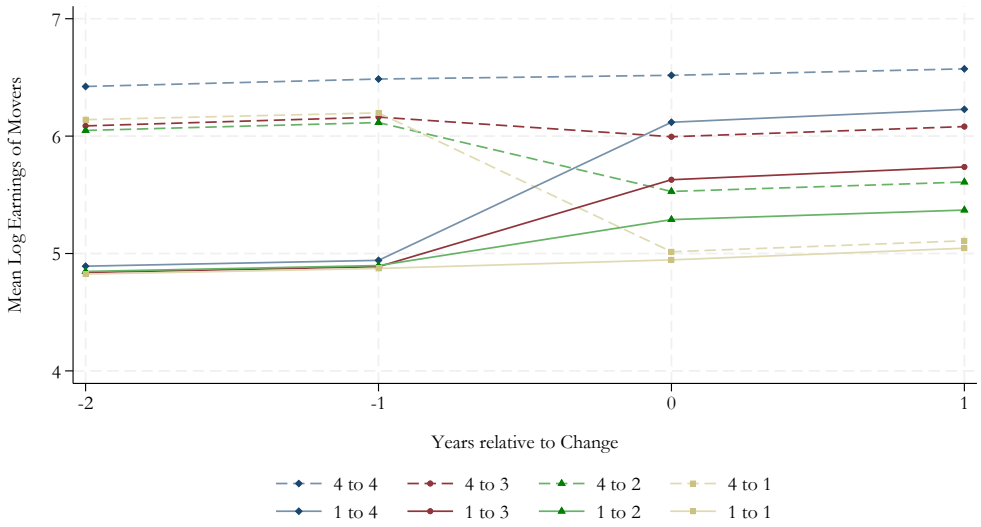
Figure B.2: Mean Residuals by Person-Establishment Deciles



Notes: The figure shows mean residuals from the TV-AKM model by deciles of worker and firm fixed effects. Residuals are computed pooling all years 2015–2024. Each cell corresponds to the average residual for a given combination of worker and firm fixed-effect deciles.

Additive separability implies that workers moving upward and downward in the firm distribution experience proportional wage increases and declines. Following Card et al. (2013), we therefore use the symmetry of wage changes for upward and downward movers as a diagnostic for additive separability and limited endogenous mobility. We keep workers observed in the same establishment for two years before the move and two years after the move in the new establishment, yielding 11.2 million moves. We classify the last and next firms of a worker by the quartile of their estimated firm effects. The event-study in Figure B.3 shows that the increase in log daily wages for workers who move from a quartile 1 establishment to a quartile 4 establishment is positive and very similar in magnitude to the decline in log daily wages for workers who move from a quartile 4 establishment to a quartile 1 establishment. In addition, the absence of pronounced wage movements immediately before or after the move indicates that transitory wage shocks are limited.

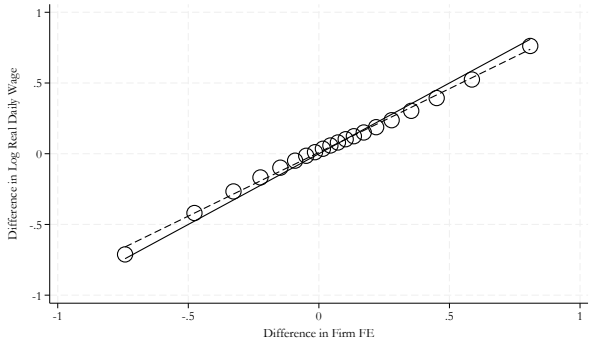
Figure B.3: Mean Log Real Daily Wage of Movers Classified by Quartile of Origin and Destination Firm Effects



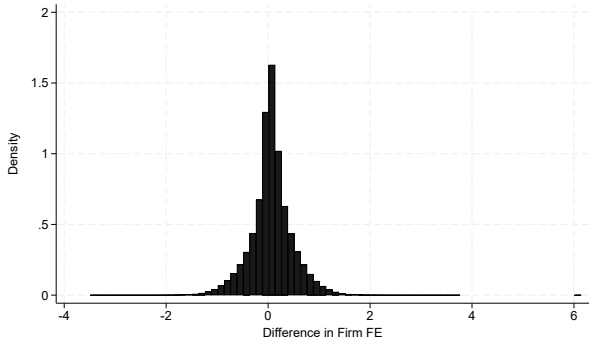
Notes: The figure shows average log real daily wage around job-to-job transitions, grouping movers by quartiles of origin and destination firm effects. The sample includes workers observed for at least two years before and after a move. Based on 11.2 mio. moves 2015-2024.

Next, we show evidence that the firm fixed effect captures firm-specific variation in worker wages. Similar to Sorkin (2018), we find that changes in wages for movers are almost one-to-one with changes in estimated firm effects. Figure B.4a plots this relationship. The in-sample estimated coefficient is 0.903, with a standard error of 0.0001, based on around 54 million observations. The distribution of the change in firm fixed effects for movers is also approximately normal, again based on the same around 54 million observations, shown in Figure B.4b.

Figure B.4: Change in Log Real Daily Wage by and Distribution of Change in Firm Effects of Movers



(a) Change in Log Real Daily Wage by Change in Firm Effects of Movers

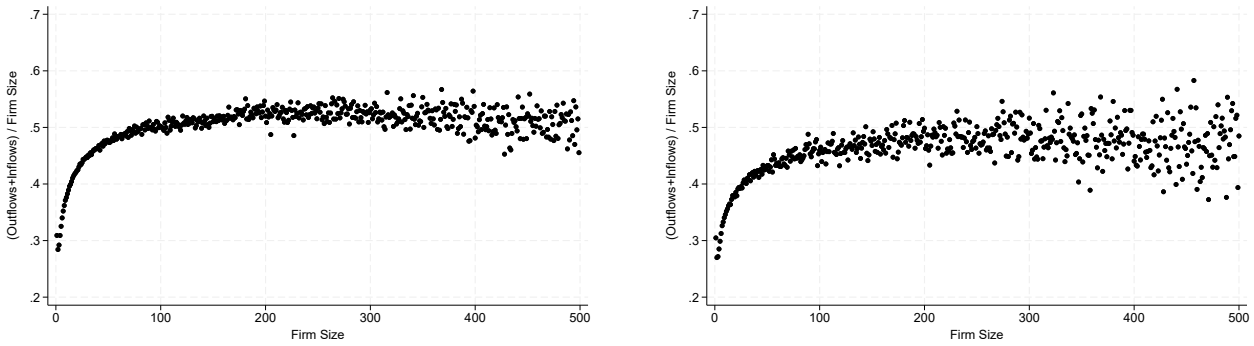


(b) Distribution of Changes in Firm Effects of Movers

Notes: The figure shows in Panel (a) the relationship between changes in log real daily wage and changes in firm effects for movers. The slope is 0.903 (SE = 0.0001). Panel (b) shows the distribution of changes in firm effects for the same sample of movers. Based on 54 mio. moves 2015-2024.

Because the distribution of firm size is highly right-skewed in Mexico, we next show that even small firms have a substantial share of movers per year over the entire period 2015–2024 as shown in Figure B.5a, which looks similar for sub-periods like 2015-2017 as shown in Figure B.5b. We plot inflows plus outflows divided by firm size, and we show that even small firms have mover shares around 30 percent per year. Each bin represents the share for a given firm-size category. Inflows are defined as workers who were in a different establishment in the previous year, while outflows are defined as workers who are in a different establishment in the next year. Reassuringly, 67.18 percent of observations belong to individuals who ever move between firms during 2015–2024. Across all 47,331,325 workers, the median number of moves is 1 and the mean is 1.19. Among movers, the median number of moves is 2 and the mean is 2.28.

Figure B.5: Share of Movers by Firm Size



(a) 2015–2024 (b) 2015–2017

Notes: The figure shows the share of worker flows (inflows plus outflows) relative to firm size across firm-size bins of size 1 up to firm size of 500 workers. Inflows are defined as workers employed at a different establishment in the previous year, and outflows as workers employed at a different establishment in the following year. Panel (a) covers the years 2015–2024, and panel (b) covers the years 2015–2017.

Overall, the evidence indicates that the TV-AKM model provides a well-fitting and internally consistent decomposition of wages. The high explanatory power, the absence of systematic interaction patterns in the residuals, and the symmetry and magnitude of wage changes around job-to-job transitions all support the additivity and mobility-based identification assumptions. Moreover, the close to one-to-one relationship between wage changes and changes in firm effects, together with the substantial mobility observed across firms of all sizes, suggests that the estimated time-varying firm effects capture economically meaningful differences in wage-setting policies. Taken together, these results validate the use of the TV-AKM framework to decompose the changes in average firm wages into firm-specific wage-setting and worker composition components.

C Appendix: Matching Process

Matching Inspections and establishment data. Our original inspections data includes information on the firm and the state in which the inspection took place, but does not identify the specific establishment inspected. As a result, for multi-establishment firms operating within the same state,

we cannot determine which establishment was visited. In contrast, the establishment survey data is at the establishment level. To merge these datasets, we aggregate the establishment survey to the firm-by-state level. All regressions based on EMIM are conducted at this level of analysis. Under this approach, a firm classified as inspected should be interpreted as having had at least one of its establishments in a given state inspected. Over 90% of firms in our sample have only one establishment per state. Therefore, this firm-by-state aggregation does not affect most observations. One potential concern of this approach is that tagging all establishments of a firm within a state as inspected could cause the estimated effect to capture both direct and spillover effects across these establishments. However, we find no evidence on spillover effects when analyzing the impact of an inspection on another establishment located in a different state but belonging to the same firm. We demonstrate that our main results are robust when focusing exclusively on single-establishment firms in Appendix Figure A.16.

Table C.1: Comparison between Original and Matched Datasets

	(1)	(2)	(3)	(4)	(5)
	Inspections	Inspections matched w. DENUE		Inspections matched with DNE	
	Share	Share	$\lvert(2)-(1)\rvert$	Share	$\lvert(4)-(1)\rvert$
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. Although EMIM sampling is primarily based on firm revenue, this variable is not observed in the IMSS data. We therefore use the

establishment-level wage bill as a proxy. Table C.2 shows that the wage bill is a stronger predictor of firm revenue than total employment.

Table C.2: Revenue Prediction Regressions

	(1) Revenue	(2) Revenue
Total workers	933.7	
	(648.8)	
Total wage bill		5.549**
		(2.274)
R^2	0.062	0.087
Within R^2	0.033	0.059

Notes: This table reports the results of OLS regressions of firm level revenue on total workers and total wage will and 3-digits NAICS sector fixed effects. The sample used is all firms from the 2018 Economic Census N=4800157.

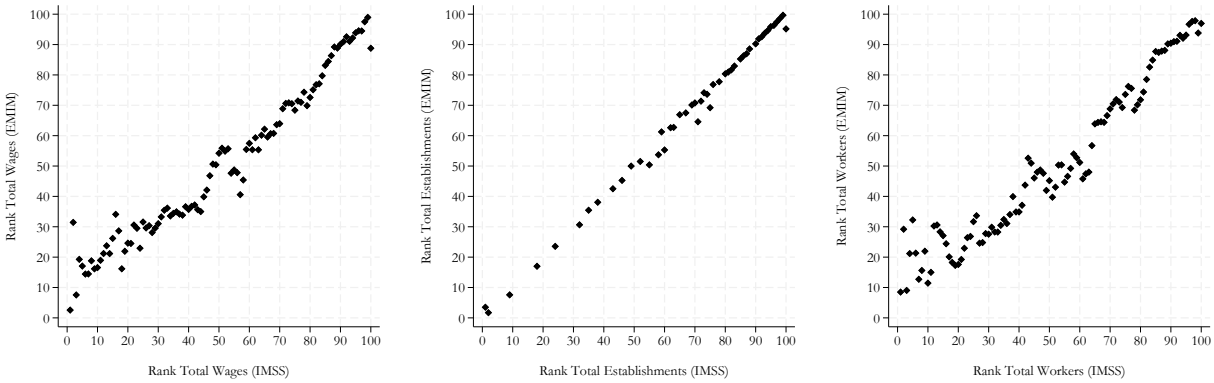
To construct a comparable IMSS sample, we proceed in two steps. First, we identify manufacturing establishments that either belonged to the top 80% of their sector's wage bill distribution in every month of 2017 or employed more than 250 workers in any month of 2017. This criterion closely mirrors the EMIM sampling strategy, which targets large and economically significant establishments. The threshold of 250 employees is commonly used to distinguish large firms from small and medium-sized enterprises (SMEs). We use 2017 as the reference year, because it predates the inspections analyzed in our main sample, which were conducted in 2018 and 2019. This first step yields approximately 6,000 establishments.

Second, we ensure geographic representativeness by matching the municipal distribution of establishments observed in EMIM. Using aggregate information on the number of establishments by municipality in EMIM, we supplement the sample with additional establishments selected based on their 2017 wage bill until the corresponding municipal targets are reached in any month of 2017. After applying these selection rules, we further retain only establishments observed in every month from 2017 through 2020. The resulting IMSS sample contains 9,416 establishments and is balanced over the period 2017–2020.

To assess the comparability of the two datasets, Appendix Figure C.1 compares aggregate outcomes across cells defined by year, month, and municipality. For each cell, we compute the total wage bill, the number of establishments, and the number of workers, rank the cells within each dataset, and present rank-rank plots. The analysis is based on 11,499 municipality-month cells spanning 246 municipalities over the 2017–2020 period. These cells include approximately 9,000 establishments (an average of 37 per municipality) and 3.4 million workers (roughly 14,000 per municipality). Although EMIM includes both formal and informal employment and the two datasets differ in their wage definitions, the distributions of all three aggregate outcomes are highly similar across datasets, supporting the comparability of our constructed IMSS sample. We obtain similar results when cells are instead defined by year, month, and sector, or by the more granular combination of year, month, sector, state, and municipality.

Figure C.1: Sample Comparability: EMIM and IMSS

(a) Total Wage Bill (b) Number of Establishments (c) Number of Workers



Notes: This figure compares rank-rank relationships across the EMIM and IMSS samples for municipality-year-month cells. Panel (a) compares the ranks of the total wage bill, panel (b) compares the ranks of the number of establishments, and panel (c) compares the ranks of the number of workers. Diamonds represent the average of 100 EMIM ranks by 100 IMSS ranks. The analysis is based on 11,499 municipality-month cells spanning 246 municipalities over the 2017–2020 period.

D 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 (2) using a static discrete choice framework, as is common in the monopsony literature (Card et al., 2018; Berger et al., 2022). The indirect utility of worker i for working in firm j is:

$$U_i(w_j, a_j) = \beta_w \cdot log(w_j) + \beta_a \cdot log(a_j) + \epsilon_{ij}, \tag{15}$$

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

$$n^s(w_j, a_j) = \left(w_j^{\beta_w} \cdot a_j^{\beta_a} \right)^{\theta_j}, \tag{16}$$

where the elasticity of labor supply with respect to the wage is captured by $\eta_{j;n,w} \equiv \frac{\partial n_j}{\partial w_j} \frac{w_j}{n_j} = \beta_w \theta_j$ and the elasticity with respect to the non-wage benefit by $\eta_{j;n,a} \equiv \frac{\partial n_j}{\partial a_j} \frac{a_j}{n_j} = \beta_a \theta_j$. While we do not explicitly model θ_j in this framework, Berger et al. (2022) show that in general equilibrium the labor supply elasticity faced by the firm, θ_j, will depend negatively on its labor market share. This result motivates using labor market share, or labor market concentration in the empirical analysis as the relevant proxy for a low firm-level labor supply elasticity.[82]

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

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

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

D.1 Optimal firm decision before enforcement

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

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

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

$$(w_j) : z_j = w_j \left(1 + \frac{1}{\eta_{j;n,w}}\right) + ka_j + p \cdot \phi(a_{min} - a_j), \tag{17}$$

$$(a_j) : z_j = ka_j \left(1 + \frac{1}{\eta_{j;n,a}} \left(1 - p \cdot \frac{\phi}{k}\right)\right) + w_j + p \cdot \phi(a_{min} - a_j). \tag{18}$$

If we join both FOCs, we get:

$$\frac{w_j}{\eta_{j;n,w}} = \frac{ka_j}{\eta_{j;n,a}} \left(1 - p \cdot \frac{\phi}{k}\right). \tag{19}$$

Replacing on the right-hand side of (17), the marginal cost of labor in this scenario is:

$$ka_j \left(1 + \frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a}\right) \left(1 - p \cdot \frac{\phi}{k}\right) + p \cdot \phi \cdot a_{min}. \tag{20}$$

Using (18) and (19) we obtain the following expressions for a_j and w_j:

$$a_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p \cdot \phi)}, \tag{21}$$

$$w_j^v = \frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)}. \tag{22}$$

Using (21) and (22) we obtain the following expression for total employment:

$$n_j^v = \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right)}\right)^{\theta_j \beta_w} \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p \cdot \phi)}\right)^{\theta_j \beta_a}. \tag{23}$$

Profits will be given by:

$$\Pi^v = \frac{1}{\eta_{j;n,w}} \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{\left(\frac{1}{\theta_j \beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}\right)^{\theta_j \beta_w + 1} \left(\frac{z_j - p \cdot \phi \cdot a_{min}}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)\left(1 - p \cdot \frac{\phi}{k}\right)}\right)^{\theta_j \beta_a}. \tag{24}$$

Finally, note that the firm will only be in this scenario if the expression of a_j in Equation 21 is lower than a_{min}. This simplifies to:

$$z_j < \underbrace{ka_{min}\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right) - p \cdot \phi \cdot a_{min}\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a}\right)}_{\Omega_0}. \tag{25}$$

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

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

$$(w_j) : z_j = w_j \left(1 + \frac{1}{\eta_{n,w}}\right) + ka_j, \tag{26}$$

$$(a_j) : z_j = ka_j \left(1 + \frac{1}{\eta_{n,a}}\right) + w_j. \tag{27}$$

If we join both FOC we get:

$$\frac{w_j}{ka_j} = \frac{\eta_{n,w}}{\eta_{n,a}}, \tag{28}$$

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:

$$ka_j \left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right) = w_j \left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1\right). \tag{29}$$

The optimal a_j and w_j are:

$$a_j^{uc} = \frac{z_j}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}, \tag{30}$$

$$w_j^{uc} = \frac{z_j}{\left(\frac{1}{\theta_j \beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}. \tag{31}$$

Employment and profits are given by:

$$n_j^{uc} = \left(\frac{z_j}{\left(\frac{1}{\theta_j \beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}\right)^{\theta_j \beta_w} \left(\frac{z_j}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}\right)^{\theta_j \beta_a}, \tag{32}$$

$$\Pi^{uc} = \frac{1}{\eta_{n,w}} \left(\frac{z_j}{\left(\frac{1}{\theta_j \beta_w} + 1 + \frac{\beta_a}{\beta_w}\right)}\right)^{\theta_j \beta_w + 1} \left(\frac{z_j}{k\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}\right)^{\theta_j \beta_a}, \tag{33}$$

Finally, note that the firm will only be in this scenario if the expression of a_j in Equation (30) is greater than a_{min}. This simplifies to:

$$z_j > \underbrace{ka_{min}\left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1\right)}_{\Omega_1}. \tag{34}$$

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

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

$$w_j \left(1 + \frac{1}{\theta_j \beta_w}\right) + ka_{min}. \tag{35}$$

The resulting wage, labor, and profits are:

$$w_j^{cc} = \frac{z_j - ka_{min}}{1 + \frac{1}{\theta_j \beta_w}}, \tag{36}$$

$$n_j^{cc} = \left(\frac{z_j - k a_{min}}{1 + \frac{1}{\theta_j \beta_w}} \right)^{\beta_w \theta_j} (a_{min})^{\beta_a \theta_j}, \tag{37}$$

$$\Pi^{cc} = \frac{1}{\eta_{n,w}} \left(\frac{z_j - k a_{min}}{1 + \frac{1}{\theta_j \beta_w}} \right)^{\theta_j \beta_w + 1} (a_{min})^{\theta_j \beta_a}. \tag{38}$$

D.1.4 Derivation of Prediction 1: when do firms violate regulations

Using Equations (25) and (34), we arrive at the following expressions for when the firm will decide to violate, stated in Prediction 1 of the model.

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

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

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

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

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

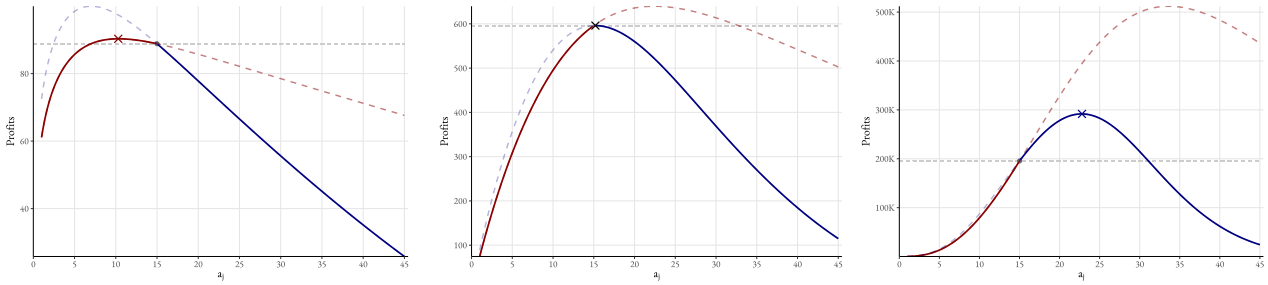
$$a_j^{uc} = \frac{z_j}{k \left(\frac{1}{\theta_j \beta_a} + \frac{\beta_w}{\beta_a} + 1 \right)}, \qquad w_j^{uc} = \frac{z_j}{\left(\frac{1}{\theta_j \beta_w} + \frac{\beta_a}{\beta_w} + 1 \right)}.$$

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

In Figure C.2 we report illustrative examples of firm profits as a function of working conditions a_j for different values of θ_j, our main parameter of interest, keeping all other parameters constant. We show the three cases: Non-compliance, constrained compliance, and compliance. In general, this

figure illustrates that the degree of labor market power θ_j, can shift otherwise similar firms from non-compliance to constrained compliance to compliance.

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



(a) Non-compliance is optimal (b) Constrained compliance optimal (c) Compliance is optimal

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

D.2 Derivation of Prediction 2: effect of enforcing compliance

In this section, we examine the impact of enforcing compliance on a firm that would otherwise choose $a_j < a_{min}$. For a firm whose optimum before enforcement is non-compliance, enforcement will always move it to the constrained compliance scenario. This occurs because a non-compliant firm's optimal choice without enforcement satisfies $z_j < \Omega_0$ in Equation (25). Since $\Omega_0 < \Omega_1$, condition (34) cannot be satisfied, and the optimal a_j under enforced compliance will never exceed a_{min}. Consequently, the firm will provide $a_j = a_{min}$. Furthermore, firm profits will weakly decrease under enforced compliance, since the firm's newly required bundle was already feasible before enforcement, but the firm had previously chosen a different bundle to maximize profits.

D.2.1 Prediction 2a - Effect on wages

The proportional change in wages is given by Equation (39), which is the ratio of the optimal wage under constrained compliance (Equation (36)) to the optimal wage under violation (Equation (22)):

$$\Delta\%w_j = \frac{z_j - ka_{min}}{z_j - p\phi a_{min}} \left(1 + \frac{\beta_a}{\frac{1}{\theta_j} + \beta_w}\right), \tag{39}$$

which is lower than 1 for all previously non-compliant firms.[83]

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

D.2.2 Prediction 2b - Effect on employment

The proportional change in employment is given by Equation (40), which is the ratio of Equation (37) to Equation (23)—that is, firm size under constrained compliance relative to firm size under violation:

$$\Delta\%n_j = \left(\underbrace{\frac{a_{min}\left(\frac{1}{\theta_j\beta_a} + \frac{\beta_w}{\beta_a} + 1\right)(k - p\phi)}{z_j - p\phi a_{min}}}_{>1} \right)^{\theta_j\beta_a} \left(\underbrace{\Delta\%w_j}_{<1} \right)^{\theta_j\beta_w} \lesseqgtr 1 \tag{40}$$

This expression highlights two opposing forces. The first term captures the direct effect of enforcing higher amenities, which tends to increase employment by raising the value of the job. The second term captures the wage adjustment, which works in the opposite direction: firms partially offset higher amenities by lowering wages. Employment increases when for the firm it is optimal to not fully offset the increase in job value from higher amenities through lower wages. In that case, the total value of the job rises, inducing a movement along the firm-specific labor supply curve (2). As a result, more workers are willing to supply labor to the firm, leading to an increase in employment.

Intuition for Prediction 2b. The intuition behind this result is that imposing a_{min} on a non-compliant firm distorts it away from its optimal relationship between w_j and a_j (defined by (19)). Once constrained, the firm can no longer achieve its optimal $\frac{a_j}{w_j}$ ratio. As a result, its best response may not involve fully offsetting the increase in amenities with lower wages to keep job value constant. Instead, the firm may optimally only partially offset the increase in amenities with lower amenities therefore increasing the total job value, $V_j = a_j^{\beta_a} w_j^{\beta_w}$. Because V_j governs the firm's labor supply, this leads to an expansion in employment that is optimal for the firm under constrained compliance.

Proof of Prediction 2b. We now prove this result analytically. For enforcement to increase employment, it must be optimal for the firm to expand employment in the new constrained-compliance equilibrium. The firm's optimal employment choice is characterized by equating marginal cost of labor to marginal (revenue) product, $MC_j(n_j) = z_j$. Therefore, to derive how the firm's optimal employment level changes after enforcement, we derive the marginal cost under non-compliance and constrained compliance, and then compare the two to assess when enforcement will lead to a marginal cost reduction.

Marginal costs under non-compliance. We begin by deriving marginal cost under non-compliance. In this scenario, the first-order conditions of the firm imply that firm chooses the optimal combination of wages defined by:

$$\frac{w_j}{a_j} = (k - p\phi)\frac{\beta_w}{\beta_a}. \tag{$\star$}$$

Total labor costs under non-compliance are:

$$TC^v = w_j n_j + (k - p\phi)a_j n_j + p\phi a_{\min} n_j, \tag{41}$$

where n_j is defined by the labor supply function (2). Marginal costs are:

$$MC^v = \frac{dTC^v}{dn_j} = \frac{\partial TC^v}{\partial n_j} + \frac{\partial TC^v}{\partial w_j}\frac{dw_j}{dn_j} + \frac{\partial TC^v}{\partial a_j}\frac{da_j}{dn_j}. \tag{42}$$

$$MC^v = \underbrace{w_j + (k - p\phi)a_j + p\phi a_{\min}}_{\text{direct effect}} + \underbrace{\frac{\partial TC^v}{\partial w_j}\frac{dw_j}{dn_j}}_{\text{wage adjustment}} + \underbrace{\frac{\partial TC^v}{\partial a_j}\frac{da_j}{dn_j}}_{\text{amenity adjustment}}. \tag{43}$$

The direct effect captures the cost of hiring an additional worker holding wages and amenities fixed, where the second and third parts of this term include the cost of providing amenities and the expected fine for under-provision. The adjustment term reflects that expanding employment requires adjusting both wages and amenities for all workers, including incumbents. This raises the wage bill and amenity bill of the firm, increasing inframarginal costs. Using the optimal wage–amenity ratio in the non-compliance regime ($\star$), we can write:

$$\frac{da_j}{dn_j} = \frac{\beta_a}{(k - p\phi)\beta_w}\frac{dw_j}{dn_j}.$$

Substituting for $\frac{da_j}{dn_j}$ and $\frac{\partial TC^v}{\partial a_j}$, $\frac{\partial TC^v}{\partial w_j}$, and rearranging terms we obtain an expression for the marginal cost of labor conditional on the optimal wage-amenity ratio ($\star$) holding.

$$\boxed{MC^v(n_j) = MC^v(n_j \mid (\star)) = \underbrace{[w(n_j) + (k - p\phi)a(n_j) + p\phi a_{\min}]}_{\text{direct effect}} + \underbrace{n_j \frac{dw_j}{dn_j}\left(1 + \frac{\beta_a}{\beta_w}\right)}_{\text{wage + amenity adjustment}}} \tag{44}$$

We can use (2) and ($\star$) to obtain expressions for the inverse labor supply curves under non-compliance with labor regulations, $w(n_j)$ and $a(n_j)$, conditional on the firm choosing the optimal combination of wages and amenities.

$$a_j(n_j) = n_j^{1/\theta_j}\left[(k - p\phi)\frac{\beta_w}{\beta_a}\right]^{-\beta_w} \qquad w_j(n_j) = \left[(k - p\phi)\frac{\beta_w}{\beta_a}\right]^{\beta_a} n_j^{1/\theta_j}. \tag{45}$$

Therefore the marginal cost can be expressed as:

$$MC^v(n_j) = \underbrace{\left[(k - p\phi)\frac{\beta_w}{\beta_a}\right]^{\beta_a}\frac{1}{\beta_w}n_j^{1/\theta_j} + p\phi a_{\min}}_{\text{direct effect}} + \underbrace{\left[\left((k - p\phi)\frac{\beta_w}{\beta_a}\right)^{\beta_a}\frac{1}{\theta_j}n_j^{1/\theta_j}\right]\left(1 + \frac{\beta_a}{\beta_w}\right)}_{\text{wage + amenity adjustment}} \tag{46}$$

Rearranging:

$$MC^v(n_j) = \left[(k - p\phi)\frac{\beta_w}{\beta_a}\right]^{\beta_a}\frac{1 + 1/\theta_j}{\beta_w}n_j^{1/\theta_j} + p\phi a_{\min}. \tag{47}$$

Marginal costs under constrained compliance Under constrained compliance, amenities are

fixed at $a_{\min}$. Total costs and marginal costs are:

$$TC^{cc}(n_j) = w_j(n_j)\, n_j + k a_{\min} n_j. \tag{48}$$

$$MC^{cc} = \frac{dTC^{cc}}{dn_j} = \frac{\partial TC^{cc}}{\partial n_j} + \frac{\partial TC^{cc}}{\partial w_j}\frac{dw_j}{dn_j} \tag{49}$$

$$\boxed{MC^{cc}(n_j) = MC^{cc}(n_j \mid a = a_{min}) = \underbrace{w(n_j) + k a_{\min}}_{\text{direct effect}} + \underbrace{n_j \frac{dw_j}{dn_j}}_{\text{wage adjustment}}} \tag{50}$$

As in the non-compliance scenario, the direct effect consists of costs of hiring an additional worker keeping wages fix and providing minimum amenities. As amenities are fixed at a_{min}, the marginal costs does not include a term representing the amenity adjustment to incumbents because these are already paid a_{min}. The firm only adjusts wages of incumbents when expanding employment, represented in the wage adjustment term. The inverse firm-specific labor supply function under constrained compliance following (2) is:

$$w_j(n_j) = n_j^{1/(\theta_j \beta_w)} a_{\min}^{-\beta_a/\beta_w}. \tag{51}$$

Therefore:

$$MC^{cc}(n_j) = \underbrace{n_j^{1/(\theta_j \beta_w)} a_{\min}^{-\beta_a/\beta_w} + k a_{min}}_{\text{direct effect}} + \underbrace{n_j^{1/(\theta_j \beta_w)} \frac{1}{\theta_j \beta_w} a_{\min}^{-\beta_a/\beta_w}}_{\text{wage adjustment}} \tag{52}$$

Rearranging terms we obtain an expression for the marginal cost of employment as a function of n_j under constrained compliance.

$$MC^{cc}(n_j) = \left(1 + \frac{1}{\theta_j \beta_w}\right) n_j^{1/(\theta_j \beta_w)} a_{\min}^{-\beta_a/\beta_w} + k a_{\min}. \tag{53}$$

Effect of enforcement on marginal cost of labor

In both the non-compliance and constrained compliance scenarios, the firm chooses employment n_j^* such that marginal cost equals marginal (revenue)product, $MC_j(n_j^*) = z_j$. Enforcement increases employment if, evaluated at the non-compliant optimum n_j^{v*}, marginal cost falls in the constrained compliant case:

$$MC^{cc}(n_j^{v*}) < MC^{v}(n_j^{v*}). \tag{54}$$

In this case, enforcement leads to a reduction in the marginal cost of an additional worker (below z_j), inducing the firm to expand employment. Using the inequality (54), and the expressions for the marginal cost under non-compliance (44) and compliance (50), enforcement increases employment

iff:

$$\underbrace{ka_{\min} - [(k-p\phi)a^v(n_j^{*v}) + p\phi a_{\min}]}_{\Delta \text{ direct effect } a_j \ (>0)} + \underbrace{w^{cc}(n_j^{*v}) - w^v(n_j^{*v})}_{\Delta \text{ direct effect } w_j \ (<0)} + \underbrace{n_j^{*v}\left(\frac{dw_j^{cc}}{dn_j} - \frac{dw_j^v}{dn_j}\right)}_{\Delta \ w_j \text{ adjustment } (<0)} - \underbrace{n_j^{*v}\frac{dw_j^v}{dn_j}\frac{\beta_a}{\beta_w}}_{\Delta \ a_j \text{ adjustment}} < 0 \tag{55}$$

As the expression in (55) shows, enforcement affects the marginal cost through four channels:

1. **Direct effect of a_j.** The direct effect of a_j will rise, as the amenity level rises, $a_{min} > a_j^v$, therefore $ka_{min} > (k-p\phi)a_j^v + p\phi a_{\min}$. This term is positive and will therefore **increase** marginal costs in the constrained compliant in comparison to the non-compliant case.

2. **Direct effect of w_j.** As the firm now provides higher a_j, for any given level of n_j, the firm can provide lower wages. The change in the direct effect of w_j will be negative and therefore **decrease** marginal costs in the constrained compliant in comparison to the violation case.

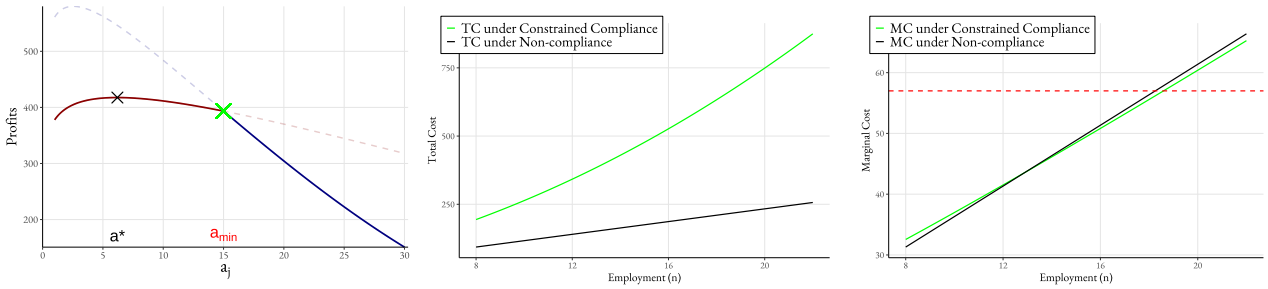
3. **Wage adjustment.** When $\beta_a > 0$, $\frac{dw}{dn}$ is decreasing in amenities, meaning that as higher level of a_j expanding employment requires smaller wage increases. This means that enforcement will lead to a reduction in the wage adjustment component. This will therefore **decrease** marginal cost in the constrained compliant in comparison to the violation case.

4. **Amenity adjustment.** Under non-compliance, hiring an additional worker requires adjusting amenities for all workers, increasing inframarginal costs. Under constrained compliance, as amenities are fixed at $a_{\min}$, the marginal cost of hiring an additional worker does not include this rise in a_j for incumbents (who already earn a_{min}). Therefore, the a_j adjustment margin term of the marginal cost is not present in MC^{cc}, which will therefore **decrease** marginal cost in the constrained compliant in comparison to the violation case.

In summary, enforcement can reduce marginal costs by eliminating amenity adjustments and reducing wage adjustments through higher amenity provision. When these indirect effects dominate the increase in direct costs, marginal cost fall and employment increases. Notice that if $\beta_a = 0$, amenities do not affect labor supply. In this case, channels (2)–(4) disappear and enforcement unambiguously increases marginal cost, reducing employment.

Figure C.3 illustrates an example of a setting in which enforcement increases employment. Panel (a) shows profits as a function of a_j, with the optimal non-compliant choice (black X) and constrained compliance outcome (green X). Panel (b) plots total costs as a function of n_j under non-compliance and constrained compliance derived from from (41) and (48). Panel (c) plots marginal cost curves derived from (47) and (53). It can be seen that while total costs are higher under constrained compliance for all n_j in this example, the marginal cost under constrained compliance lies below that under non-compliance at the the intersection with z_j, leading to a higher intersection with z_j and thus higher employment.

Figure C.3: Profits, total costs and marginal costs under constrained compliance and non-compliance

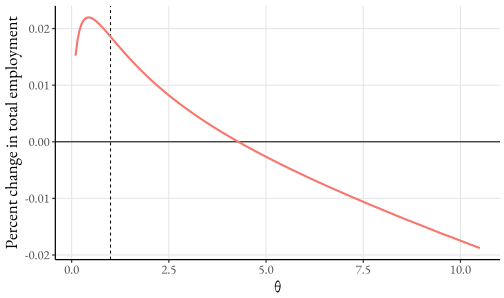


(a) Profits (b) Total Costs (c) Marginal Costs

Notes: This figure illustrates simulated profits, total costs, and marginal costs under non-compliance and constrained compliance for a representative firm. Panel (a) plots profits as a function of amenities a_j, showing the optimal non-compliant choice a_j^* (black X) and the constrained compliance level $a_{\min}$ (green X). Panel (b) shows total labor costs as a function of employment n_j, indicating that total costs are higher under constrained compliance for all n_j. Panel (c) plots the marginal cost of labor under non-compliance (Equation (47)) and constrained compliance (Equation (53)). In this example, marginal cost under constrained compliance is lower over the relevant range, leading to a higher optimal level of employment. The figure is generated using the following parameter values: $z_j = 57$, $\theta_j = 1$, $a_{\min} = 15$, $\beta_w = 0.9$ $\beta_a = 0.1$, $k = 1.12$, $\phi = 1.5$, $p = 0.5$

Figure C.4 shows how this result depends on labor supply elasticity θ. When labor supply is sufficiently inelastic (low θ, high monopsony power), employment increases. In contrast, when labor supply is sufficiently elastic, the direct effects of enforcement on marginal costs dominate and employment declines.

Figure C.4: Effect on total employment under optimization in CC and V for different θ



D.3 Model solution with alternative labor supply

In this section, we solve the model under three specific cases of the labor supply function in 16. First, we analyze a setting where the firm faces an upward-sloping labor supply curve that depends solely on the wage ($\beta_a = 0$), as in conventional monopsony models (Card et al., 2018). Second, we assume that firms set w_j and a_j in a perfectly competitive labor market, where firms take the value of a job as given ($\theta \to \infty$) (Dube et al., 2022; Mas, 2025). Third, we analyze the case where the firm faces an upward-sloping labor supply curve that depends solely on the non-wage conditions ($\beta_w = 0$).

We obtain two key results. First, in the alternative scenario where $\beta_a = 0$ such that firms do not have amenity setting power, if the price per unit of amenity k and the fine per worker ϕ are constant across firms, the only factor driving heterogeneity in a firm's decision to violate regulations is the

probability of inspection p. Second, in both alternative scenarios considered, enforcing a minimum level of amenities $a_{\min}$ on a non-compliant firm always leads to a reduction in employment. The intuition behind this result is as follows. In the scenario where the labor supply curve is upward sloping in the wage, the increase in amenities due to enforcement raises the marginal cost of labor but does not increase labor supply, resulting in a decline in employment. In the competitive environment, firms minimize marginal cost of labor $w_j + a_j$ subject to providing a level of worker utility they take as given. An increase in enforcement causes firms to deviate from this cost-minimizing allocation, causing a decrease in employment.

D.3.1 Labor supply depending only on w_j

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

$$n^s = w^{\theta_j \beta_w}. \tag{56}$$

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:

$$n_j^{violate} = (z_j - p\phi a_{min})^{\theta_j \beta_w} \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w}, \tag{57}$$

$$\Pi_j^{violate} = \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w} \left(\frac{1}{1 + \theta_j \beta_w} \right) (z_j - p\phi a_{min})^{1 + \beta_w \theta_j}. \tag{58}$$

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

$$n_j^{comply} = (z_j - k a_{min})^{\theta_j \beta_w} \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w}, \tag{59}$$

$$\Pi_j^{comply} = \left(\frac{\theta_j \beta_w}{1 + \theta_j \beta_w} \right)^{\theta_j \beta_w} \left(\frac{1}{1 + \theta_j \beta_w} \right) (z_j - k a_{min})^{1 + \beta_w \theta_j}. \tag{60}$$

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 (57) and (59), we can see that in the case that a firm does not comply with a_{min} ($k > p\phi$), then enforcement of a_{min} will reduce employment n_j.

D.3.2 Perfectly competitive labor market

In a perfectly competitive labor market, the elasticity of the labor supply function in Equation (16), $\theta_j \to \infty$. The utility of total compensation $\bar{U}$, is determined in the competitive market and taken as given by the firm.

$$w_j^{\beta_w} \cdot a_j^{\beta_a} = \bar{U}. \tag{61}$$

Using Equation 61, 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., $f''(n_j) < 0$.[84] When the firm does not comply with minimum amenities requirements ($a_j < a_{min}$), the first-order conditions with respect to w_j and n_j are:

$$(w_j): \quad w_j = \left((k - p\phi) \bar{U}^{\frac{1}{\beta_a}} \frac{\beta_w}{\beta_a} \right)^{\frac{\beta_a}{\beta_a + \beta_w}}, \tag{62}$$

$$(n_j): \quad z_j f'(n_j) = w_j + (k - p\phi) \underbrace{\bar{U}^{\frac{1}{\beta_a}} w_j^{-\frac{\beta_w}{\beta_a}}}_{a_j} + p \cdot \phi a_{min}. \tag{63}$$

The firm will only be in the non-compliance scenario if $a_j < a_{min}$. Substituting the expression for a_j into 62, this implies:

$$\bar{U}^{\frac{1}{\beta_a}} \cdot \left((k - p\phi) \bar{U}^{\frac{1}{\beta_a}} \frac{\beta_w}{\beta_a} \right)^{\frac{-\beta_w}{\beta_a + \beta_w}} < a_{min}. \tag{64}$$

When the firm complies with workplace regulations by providing exactly $a_j = a_{min}$ (constrained compliance), we can use Equation (61) and the FOC of the profit function with respect to n_j to derive the expressions for wages and labor:

$$w_j = \left(\frac{\bar{U}}{a_{min}^{\beta_a}} \right)^{\frac{1}{\beta_w}}, \tag{65}$$

$$z_j f'(n_j) = \left(\frac{\bar{U}}{a_{min}^{\beta_a}} \right)^{\frac{1}{\beta_w}} + k a_{min}. \tag{66}$$

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 (61). 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 63, 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

[84] 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.

the marginal cost of labor. This increase in the marginal cost of employment causes the amount of labor hired, defined by Equation (66), 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.

D.3.3 Labor supply depending only on a_j

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

$$n^s = a_j^{\theta_j \beta_a}. \tag{67}$$

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:

$$a_j = \frac{z_j - \tilde{w} - p\phi a_{min}}{\left(1 + \frac{1}{\theta_j \beta_a}\right)(k - p\phi)}, \tag{68}$$

while labor is given by:

$$n_j = \left(\frac{z_j - \tilde{w} - p\phi a_{min}}{\left(1 + \frac{1}{\theta_j \beta_a}\right)(k - p\phi)} \right)^{\theta_j \beta_a}. \tag{69}$$

The firm will be in the non-compliance scenario if the expression in Equation (68) is lower than a_{min}. This translates into:

$$z_j - \tilde{w} < k a_{min} \left(\frac{1}{\theta_j \beta_a} + 1 \right) - p \cdot \phi \cdot a_{min} \frac{1}{\theta_j \beta_a}. \tag{70}$$

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 (67) and given by:

$$a_{min}^{\theta_j \beta_a}. \tag{71}$$

Notably, in this scenario, Predictions 1 and 2 from the benchmark model are confirmed. In contrast to the alternative case where labor supply depends solely on wages, Equation (70) 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 (69) and (71), 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.

E Appendix: Mechanisms and Comparison with Prior Evidence on Inspection Effects

This section considers alternative mechanisms that could account for the empirical patterns documented in Section 6, with a particular focus on the observed increase in firm size. It then compares our findings with those in the existing literature on inspections and provides empirical evidence on the mechanisms that can explain the differences in estimated effects.

E.1 Alternative mechanisms

In this section, we provide empirical evidence against four main alternative mechanisms that could account for the positive effects of inspections on firm employment observed in Section 6. Specifically, 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 (iv) changes in managerial structure of the firm. We provide empirical evidence suggesting that these alternative explanations are inconsistent with empirical evidence or unlikely to fully account for the magnitude of the employment effects.

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

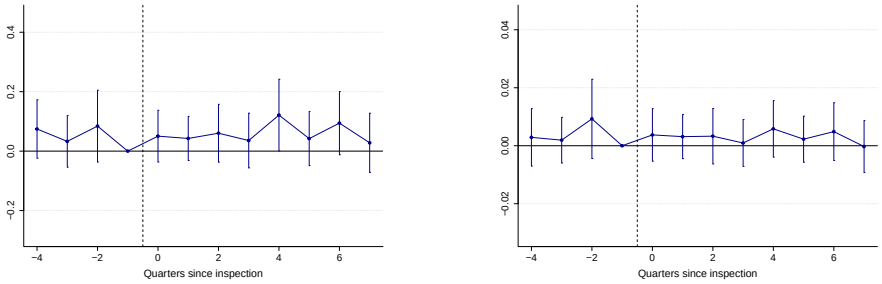
First, the monthly manufacturing survey EMIM is designed to capture information on the total workforce, including both formal and informal workers. Consequently, we should not expect to see an increase in total employment if firms only formalize informal workers.[85] Even if firms were inclined to misreport their informal workers, the firms in our sample are unlikely to have large shares of informal workers. The manufacturing sector has the lowest share of informal workers among all sectors in Mexico (see Figure A.3), and our sample focuses on the largest and most productive manufacturing firms, which are even less likely to employ informal workers.[86] Additionally, the estimated employment effects are remarkably similar in EMIM and IMSS, suggesting that the increase in IMSS employment is unlikely to be driven by formalization.

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 real production, reported in Figure 12 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 C.5, we find no significant effect of inspections on social security contributions per worker nor on the ratio of contributions to average wages (at the 5% level).

[85] 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.

[86] 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 C.5: Effect on Average Social Security Contributions



(a) Social Sec. Contrib. per Worker (b) Social Sec. Contrib. over Average Wage

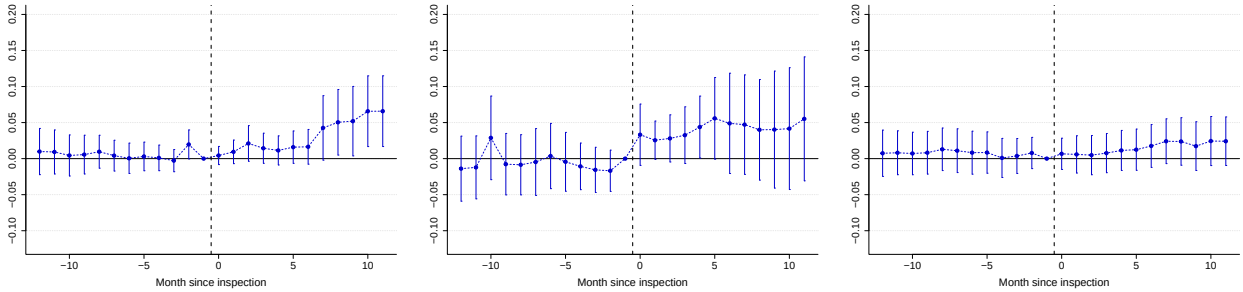
Notes: This Figure shows estimates of β^h and 95% confidence intervals from estimating Equation 7 at the quarterly level for $h \in [-12:11]$. The outcome variables measured at the firm level are social security contributions per worker and average social security contributions per worker divided by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

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

Fourth, as shown in Figure 6, employment rises gradually following an inspection. If the effect were primarily driven by formalization, we would instead expect an immediate jump in employment after the inspection, similar to the pattern documented by Brotherhood et al. (2024). Moreover, the increase in employment is partly explained by lower separations and greater hiring from other formal firms, margins that cannot be attributed to formalization.

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



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

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

Mechanical increase in labor for compliance. One potential alternative explanation for the post-inspection increase in employment is that firms are mechanically required to hire additional personnel to comply with newly enforced regulations—for example, by employing workers specialized in safety or regulatory compliance. To assess the plausibility of this mechanism, we reviewed the complete set of inspection protocols, which detail the specific standards subject to enforcement during inspections.[87] While the protocols do not contain explicit directives requiring firms to hire more workers as an automatic consequence of inspection, certain provisions may indirectly encourage firms to increase staffing levels to comply with regulations or correct deficiencies. For instance, provision 002-STPS-2010 stipulates that firms classified as having a high risk of fire must create fire response brigades with specific training and simulation requirements. If the existing workforce is insufficient to meet these requirements across all shifts, additional personnel may need to be hired. Similarly, provision 009-STPS-2011 states that if inspections reveal deficiencies in the handling of machinery, hazardous materials, or work at heights, the firm must assign authorized, trained personnel to operate and maintain the machinery. If existing staff are insufficient or unqualified, new hires may be necessary to comply. However, 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% increase in employment documented in Figure 6.

Moreover, a mechanical increase in the number of workers employed solely to comply with the regulation would be unlikely to raise firms' total production and therefore cannot explain the positive effects on revenues, material costs, production costs, and plant capacity used as presented in Section 6.3.4. This mechanism is also inconsistent with the reduction in voluntary worker separations from the firm documented in Section 6.4.

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

[87] 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

a_{min} can be expressed as $q \cdot \tilde{w}$ where $\tilde{w}$ is an exogenous wage paid to the specialized workers. Firm profits under compliance can be expressed as:

$$\Pi_j = z_j n_j - w_j \underbrace{n_j}_{\substack{\text{productive} \\ \text{employment}}} - \tilde{w} \underbrace{q \cdot n_j}_{\substack{\text{specialized} \\ \text{employment}}} \tag{72}$$

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. However, this would lead to a decrease in our observed measure of revenue per worker $\frac{z}{n(1+q)}$ which is again inconsistent with the evidence in Section 6.3.4.

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 C.6 shows that the positive employment effect is stronger for inspections related to workplace safety, hygiene, and training, suggesting that this type of misinformation is unlikely to be the primary driver of our results.[88]

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

Alternatively, it is possible that firms misperceive the positive labor supply effects of providing good working conditions. In this scenario, firms forced to improve working conditions, through an inspection, may realize that doing so increases their attractiveness to workers. This mechanism is also consistent with a model of monopsony power, in which firms underestimate the labor supply elasticity with respect to amenities—i.e., they perceive $\tilde{\beta}_a < \beta_a$. As a result, firms provide suboptimally 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. Yet under this scenario the rise in total firm employment would also be driven by an expansion in the firms' labor supply, as in our main mechanism. In conclusion, even if inspections operate primarily by conveying information to firms, the observed increase in employment remains aligned with monopsonistic behavior.

Changes in managerial structure. An alternative explanation is that inspections induce changes in firms' managerial structure. Firms' decisions to violate labor regulations may be correlated with managerial quality if poorly managed firms are less likely to be informed about labor regulations or more likely to exploit their labor market power by providing poor working conditions. In this

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

case, an inspection may reveal poor management practices, leading firms to replace their managers. Such managerial turnover could, in turn, affect employment and production if the new managers are not only more compliant with labor regulations but also more productive.

Our setting and evidence are not consistent with this explanation. First, Bloom et al. (2026) show that management quality is higher among large manufacturing firms than among smaller firms. Since our analysis focuses on large manufacturing establishments, there is less scope for improvements in managerial quality following an inspection. Second, although we cannot directly identify managers in our data, we examine whether inspections increase the hiring of workers with top-coded wages in the IMSS, who are likely to occupy managerial positions. We find no effect on this outcome, providing no evidence that inspections induce managerial turnover.

E.2 Comparison to prior evidence on inspections

A growing literature studies how labor inspections affect firm behavior and labor market outcomes in developing countries. While related studies in Brazil and Mexico find that labor inspections *reduce* firm employment (Brotherhood et al., 2024; Samaniego de la Parra and Fernández Bujanda, 2024; do Prado et al., 2025), Section 6.3.1 shows that inspections *increase* employment among large manufacturing firms. In this section we argue that these contrasting findings can be reconciled by differences in sample composition and the mechanisms through which inspections affect firms, and we present empirical evidence consistent with this interpretation.

Specifically, while our analysis focuses on large manufacturing firms, the prior literature instead studies firms across all sectors that are substantially smaller on average. Inspected firms in our sample employ, more than 500 formal workers on average, compared with approximately 100 formal workers in Samaniego de la Parra and Fernández Bujanda (2024). In do Prado et al. (2025), the average inspected firm employs only 19 workers, 12 of whom are formally employed. We argue that these stark differences in firm composition affect two dimensions that are central to firms' responses to inspections: the primary compliance margin affected by inspections and firms' degree of labor market power.

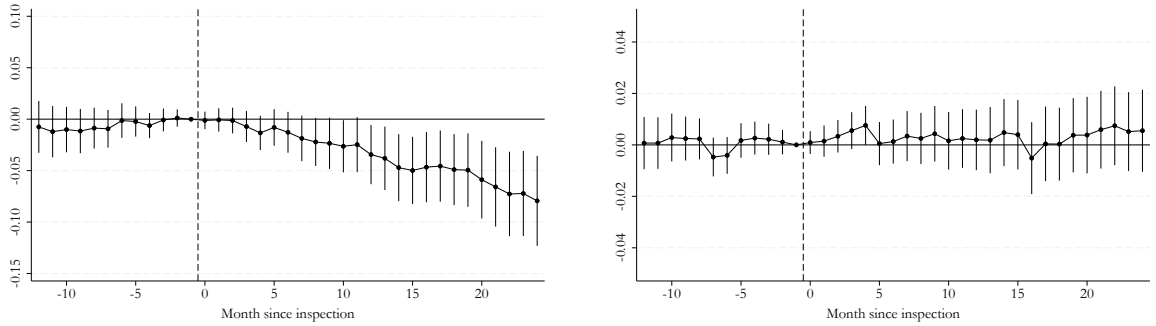
First, the main dimension of compliance studied in the prior literature is the formalization of informal workers. In these settings, inspections induce firms to formalize previously informal workers, raising labor costs and, in turn, reducing employment. As discussed in the previous subsection, worker formalization is unlikely to be the primary margin of adjustment in our setting. The mechanism underlying the negative employment effects in the prior literature is therefore unlikely to account for the employment response documented in our sample.

The second important consequence of these differences in sample composition between this study and previous literature is related to the fact that large firms are more likely to possess labor market power (Berger et al., 2022). As a result, the mechanism emphasized in this paper—whereby firms exercise labor market power over working conditions and inspections increase employment by increasing labor supply—is more likely to operate among large firms. Smaller firms, by contrast, are likely to possess less labor market power and to offer wage–amenity bundles closer to the competitive benchmark. By increasing the cost of labor, inspections may then induce smaller firms to reduce em-

ployment, consistent with the findings of the prior literature. Consequently, the mechanism through which inspections increase formal employment in our setting is less likely to operate in the samples in the prior literature that are composed primarily of smaller firms.

We provide two sets of empirical evidence supporting this interpretation. First, we estimate the effects of random inspections on employment and wages using the full sample of ever-inspected firms in the IMSS data, rather than restricting to large manufacturing firms. Because this broader sample includes smaller firms, it is more comparable to the samples examined in the prior literature. The results are presented in Figure C.7 where it can be seen inspections have a clear negative effect on employment of approximately -7%, in line with the findings of Samaniego de la Parra and Fernández Bujanda (2024). This result suggests that the divergence in findings is unlikely to be driven by differences in the inspection data or empirical design. Instead, it is consistent with differences in the types of firms included in each analysis and, consequently, in the mechanisms driving the results.

Figure C.7: All Ever inspected Firms



(a) Log Total Workers

(b) Log Salary

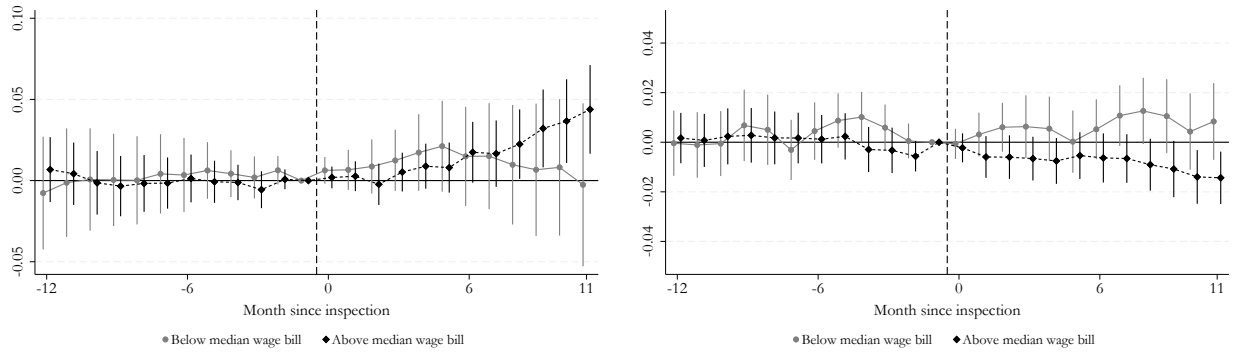
Notes: This figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12:11]$. In panel (a), the outcome variable is the log number of workers. In panel (b), the outcome variable is the log average establishment wage. Regressions are estimated based on a sample of all ever randomly inspected firms in IMSS between 2017 and 2022. Treated firms are those receiving the first ordinary inspection between 2018 and 2019. Standard errors are clustered at the establishment level.

Second, we perform the opposite exercise by focusing on a subsample of firms for which the differences with the prior literature are largest and the mechanisms emphasized in this paper are most likely to be present. Specifically, we examine heterogeneity by establishments' pre-inspection total wage bill. The wage bill is a key determinant of the IMSS sampling procedure that replicates the EMIM sample of large, productive establishments and is strongly correlated with total revenue. Because our main sample already consists of large firms, this exercise compares the very largest establishments to other large establishments. It therefore allows us to assess whether the estimated effects are more closely aligned with the predictions of our model among firms where labor market power is likely to be strongest.

Figure C.8 confirms this prediction by presenting heterogeneous treatment effects by establishments' pre-period wage bill in 2017. Panel (a) shows that the increase in employment is concentrated among establishments with above-median wage bills. For this group, employment rises by about 4 percent one year after inspection, similar in magnitude to the estimate for the full sample but es-

timated more precisely. Panel (b) shows a decline in wages of roughly 1.5 percent. Relative to the baseline estimate of about 1 percent, this effect is larger in magnitude, more precisely estimated, and statistically significant. Overall, the observed rise in employment among high-wage-bill establishments despite larger wage declines is consistent with a stronger version of the main mechanism: enforcement improves working conditions enough that the increase in job value is not fully offset by lower wages, therefore increasing labor supply.

Figure C.8: Heterogeneous Effects by Median Wage Bill 2017



(a) Log Total Workers

(b) Log Average Establishment Wage

Notes: This figure shows estimates of β^h and 95% confidence intervals from estimating Equation (7) at the monthly level for $h \in [-12 : 11]$. The figure reports separate estimates for establishments above and below the median wage bill. Grey circles denote establishments below the median wage bill, while black diamonds denote establishments above the median wage bill. The median wage bill is computed within sector-by-state cells in 2017 using the main sample. In panel (a), the outcome variable is the log number of workers. In panel (b), the outcome variable is the log average establishment wage. 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. Standard errors are clustered at the establishment level.

Taken together, these findings help reconcile our results with the negative employment effects documented in the prior literature. Inspections reduce employment in the broader population of firms but increase employment among the large firms that constitute our primary sample. This heterogeneity is consistent with the mechanism developed in this paper: inspections can expand employment when firms possess labor market power and employment is constrained by labor supply, whereas they reduce employment when firms face more competitive labor markets and employment is primarily constrained by labor demand.