CIDE

LATIN AMERICAN ECONOMIC REVIEW

2026 | Volume 37 | Article 8

Does the Enforcement of Labor Regulations Reduce Informality? The Case of Peru *

María Florencia Pinto‡ Yulia Valdivia Rivera† Hernan Winkler§

Submission received: August 29, 2025
Final version received: May 5, 2026
Accepted: June 29, 2026
Published: July 3, 2026

Abstract

This paper examines the effects of strengthened labor regulation enforcement on labor market outcomes in Peru from 2010 to 2019. In 2013, the Peruvian government established a national labor inspection agency, which was progressively rolled out nationwide. This reform led to a substantial increase in the frequency and severity of fines imposed on formal firms. Despite this heightened enforcement, our analysis using extended two-way fixed effects models finds no significant effects on overall employment levels. Moreover, there is no evidence of changes along either the *intensive* margin—informal employment within formal firms—or the *extensive* margin—the share of employment in informal firms. These findings suggest that increased enforcement of labor regulations did not lead to measurable shifts in labor informality or employment outcomes during this period.

Keywords: labor informality, enforcement, regulation, inspections, fines.
JEL codes: H26, J3, O23, O17.

*We would like to thank participants of the LACEA-LAMES 2022 Annual Meeting and the 2022 Jobs and Development Conference, Carlos Rodríguez-Castelán, Daniel Barco, Tanja K. Goodwin, and Luciana De la Flor, for valuable suggestions and comments. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors and do not necessarily represent the views of The World Bank, its executive directors, or the governments of the countries they represent.

‡CEDLAS - IIE - Universidad Nacional de La Plata. Corresponding author.
Email: mflorenciapinto@econo.unlp.edu.ar.
†University of Chicago, Harris School of Public Policy.
Email: yvaldiviarivera@uchicago.edu.
§World Bank.
Email: hwinkler@worldbank.org.

1. Introduction

Labor informality is widespread in the developing world. Despite some progress during the last three decades, informality still accounts for more than 70 percent of total employment and one-third of total output in developing economies (Ohnsorge and Yu, 2022). While informality is often a symptom of an underdeveloped economy, it can also prevent people from moving out of poverty.

Addressing labor informality remains a critical challenge for developing nations. As the informal sector operates outside the legal and regulatory framework, informal workers are often excluded from social insurance, job-related benefits such as paid-time-off and sick leave, and other publicly-provided services (Loayza, 2018). Furthermore, informality challenges the collection of contributions and taxes, reducing the fiscal capacity of governments. Finally, informality is often more prevalent among the poor and vulnerable, and it could prevent them from improving their living conditions.

In response to these challenges, governments around the developing world have often relied on strengthening the enforcement of labor regulations to increase the formalization of firms and workers (Jessen and Kluve, 2021). This policy increases the relative cost of hiring informal workers, as firms face a higher risk of being detected and penalized. Therefore, stricter enforcement is expected to reduce informality. However, in practice, the final impact will depend on other factors. For example, the effectiveness of enforcement depends on the capacity of governments to supervise firms, which could be challenging in developing countries where informality is pervasive and compliance with regulations is extremely low (Almeida and Carneiro, 2012). At the same time, stricter labor inspections could have unintended consequences similar to those of other labor regulations, where the increased cost of labor may lower firms' growth, increase their exit rates, and reduce formal employment (see, for example, Bossavie et al. 2019; Kahn 2007; Betcherman 2015).

In this paper, we study the effect of increasing the enforcement of labor inspections on labor market outcomes in Peru, a country with one of the highest levels of labor informality worldwide.[1] Estimating such an effect is often challenged by the fact that inspections are not randomly assigned. For example, sectors or districts where intrinsic violations of the labor law are more prevalent could be more likely to be targeted by inspections, inducing an upward bias in the estimation. At the same time, non-compliance detection may be higher in areas with better technology or capacity to conduct labor inspections, which is likely correlated with labor outcomes, thereby adding bias to the point estimates. To account for this potential non-random assignment of labor inspections, we exploit the creation and geographic roll-out of SUNAFIL, the National Labor Inspection Office, as a source of exogenous variation in the level of public enforcement. SUNAFIL is in charge of promoting, supervising, and verifying compliance with labor regulations throughout Peru.

We find that the opening of SUNAFIL's regional offices dramatically increased the enforcement of labor regulations, measured by the number and size of fines issued per 1,000 workers in the formal sector. We then estimate impacts using a Two-Way Fixed Effects (TWFE) model, which suggests that opening a SUNAFIL regional office did not affect total employment or employment in the formal sector. An essential contribution of this paper is to measure the effects of enforcement on both the *extensive* and *intensive* margins of informality. The former is the share of employment concentrated in informal or unregistered firms. The latter is the share of informal jobs within the universe of formal or registered firms. This is relevant because enforcement measures are typically directed towards the latter group, even though informality is more concentrated among unregistered or informal businesses. The standard TWFE model suggests that the policy reduced informality along both margins, but the impacts were modest. However, while TWFE models are widely used in empirical research, recent literature has shown that they can produce biased results when the treatment is introduced at different times across units and if treatment effects are heterogeneous (de Chaisemartin and D'Haultfœuille

[1] According to the ILO's Statistics on the informal economy (https://ilostat.ilo.org/topics/informality/, accessed on July 25 2025), among 92 economies with 2019 data, Peru ranked in the 24th position in terms of labor informality.

2020; Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Borusyak et al. 2024). Therefore, to address these concerns, we complement the baseline analysis by estimating the extended two-way fixed effects (ETWFE) estimator proposed by Wooldridge (2021a). The analysis reveals no statistically significant effects of the policy on total employment or labor informality, whether at the extensive or intensive margin.

This paper contributes to a large body of literature investigating the impacts of labor regulations. The conclusions from empirical studies are far from conclusive. Almeida and Carneiro (2012) explore the effects of labor inspections on labor market outcomes in Brazil, and find that a rise in inspections led to an increase in the share of the population in formal employment, an increase in non-employment, and a reduction of wages in the formal sector. Likewise, De Andrade et al. (2016) conducted a field experiment in Belo Horizonte, Brazil, to assess the relative effectiveness of policies that make it easier and cheaper for informal businesses to formalize versus policies that raise the costs of remaining informal by increasing the enforcement of existing regulations. The authors find that inspections are more effective than incentives, i.e., firms do not choose formality unless forced to do so. The authors interpret this finding as a sign of the low private benefits of formality to firms. Also, for the case of Brazil, Meghir et al. (2015) find that enforcement allowed the allocation of workers toward higher-productivity jobs. Other studies, however, find that enforcement has unintended impacts on labor market outcomes or no impacts at all. For instance, Bhorat et al. (2012) explore an increase in government enforcement in South Africa and find that it did not affect minimum wage compliance, whereas Almeida and Poole (2017) show that, after a trade shock, plants facing stricter labor-law enforcement experience a slowdown in job creation and productivity. Accordingly, Almeida and Poole (2017) find that the positive effects of a trade shock increase employment and output, and reduce informality in Brazil; however, firms in areas that experience stricter enforcement of labor regulations experience weaker positive impacts.

Building on this international evidence, research specific to Peru has also examined enforcement effectiveness. Viollaz (2018b) studies the effects of enforcement on compliance with four labor standards (pension system enrollment, minimum wage, maximum weekly working hours, and written employment contract) in Peru from 2008 to 2013. She exploits labor regulations and penalties variation according to firm size and studies whether firms adjust by downsizing to benefit from lower fines and less stringent regulations. The empirical findings indicate that enforcement efforts have little effect on the degree of compliance or the size of firms. In sum, the empirical evidence on labor enforcement effectiveness remains inconclusive, with studies documenting positive, null, and negative effects depending on context and methodology. This heterogeneity in findings highlights the importance of rigorous identification strategies and comprehensive outcome measurement when evaluating enforcement interventions.

The remainder of the paper is organized as follows. Section 2 describes the labor inspection process in Peru and the creation of SUNAFIL. Section 3 presents the data sources used in the analysis, and Section 4 describes the empirical analysis used. Section 5 discusses the main results, and Section 6 concludes.

2. Background

2.1. Labor informality in Peru and the creation of SUNAFIL

Peru has experienced a constant and significant decline in labor informality since 2007, primarily due to rapid formal job creation in a context of strong output growth over the last two decades. However, this downward trend slowed down in 2016, and informality rates grew again, even before the COVID-19 pandemic. The incidence of this phenomenon remains high: in 2019, labor informality reached 73 percent of total employment, a rate that increased to almost 77 percent in 2021, the highest level in 11 years (see Figure A1, Panel

a). In terms of output, the informal sector produces only around 19 percent of total output, reflecting its lower productivity compared to the formal sector (INEI, 2019).

In 2013, Law 29981 created the National Labor Inspection Administration (SUNAFIL, the acronym for *Superintendencia Nacional de Fiscalización Laboral*), whose primary purpose is to verify the compliance of labor market and social security regulations in the private sector.[2] Before the creation of SUNAFIL, labor inspections were conducted only by the Ministry of Labor and by regional offices distributed throughout the country. Even though the *de jure* regulation based on the labor law was universal, there was high heterogeneity in effective enforcement across the territory, not only due to regional differences in technical capacity (human resources and equipment) for conducting inspections but also because of significant disparities in the criteria applied when an infraction was detected. Thus, a firm operating in two regions and violating the same labor standard faced different inspection risks and could receive completely different penalties.

These regional disparities in enforcement led to the creation of SUNAFIL, a central authority in the labor inspection system, attached to the Ministry of Labor, whose objective was to consolidate all labor inspections throughout the country and standardize the criteria applied when dealing with labor law infractions. SUNAFIL is responsible for promoting, supervising, and verifying that employers comply with labor obligations nationwide. The labor inspections conducted by SUNAFIL, as it generally happens in other countries as well, are focused on firms in the formal sector only, i.e., firms that are registered with the tax authority (SUNAT – *Superintendencia Nacional de Aduanas y Administración Tributaria*).[3] In addition, SUNAFIL targets small and large firms, while micro firms (that is firms with no more than five employees) continue to be under the orbit of the regional inspection agencies.[4] This is a crucial point since labor inspections miss a large share of labor informality, given that the informal sector employs most informal workers.

Informality can take place under two different margins. On one hand, firms can decide whether to register with the tax authorities and comply with taxes and labor regulations. If they do, they become formal; if they do not, firms remain informal and small, not to be detected by tax authorities. This is known as the *extensive margin* of informality, which differentiates the formal and informal sectors of the economy. On the other hand, there is an *intensive margin* of informality that takes place within the formal sector of the economy. Registered firms can be partially informal across different dimensions: they can maintain part of their workforce unregistered, pay part of the salary of registered workers off the books, or fail to comply with other labor regulations such as those related to working days, holidays, and leave, etc. (Ulyssea, 2018; Perry et al., 2007). The intensive margin of informality accounts for a large share of total informal employment in Latin American countries: In Brazil and Mexico, 40 and 44 percent of informal workers are employed by formal firms, respectively (Ulyssea, 2018). In the case of Peru, about 23 percent of informal workers are employed in the formal sector (INEI, 2020).

SUNAFIL began to operate in practice in 2014, gradually opening offices in each Peruvian region. Figure 1 below displays the creation of regional offices by year. The Headquarters (Lima) and a few regional offices initiated their inspection operations in 2014. Other regional offices were launched later, between 2015 and 2019. There are five regions that by 2019 had not yet established a SUNAFIL labor inspection office.[5] This gradual rollout induced significant variation in the level of enforcement for firms located in different geographical areas and over time, which we exploit in the empirical strategy.

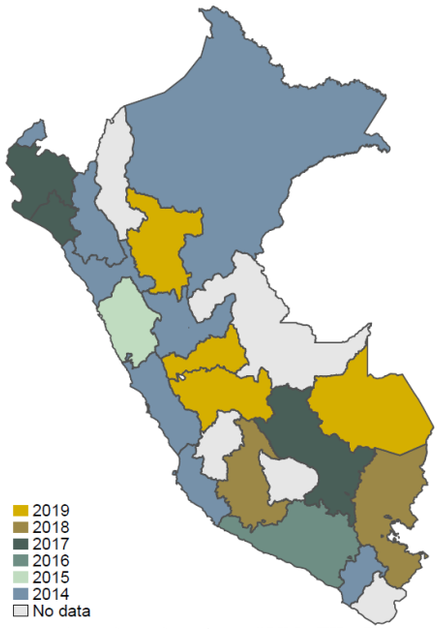
[2]Strictly speaking, it verifies compliance with labor regulations both in the private and public sector, as long as the labor relationship takes place under the private labor regime (SUNAFIL, 2022).

[3]SUNAFIL also conducts information campaigns and provides guidance regarding labor market regulations.

[4]In 2018, Law N° 30814 established that for eight years, SUNAFIL would temporarily supervise microenterprises, a competence that until then only regional governments had. However, it was only in June 2021 that a Ministerial Resolution (N° 100-2021-TR) allowed the creation of commissions to transfer powers from regional governments to SUNAFIL. In practice, only three regions have fully completed this process to date.

[5]Those regions are Amazonia, Apurímac, Huancavelica, Tacna, and Ucayali, where SUNAFIL opened its first regional office in the late 2020s.

Figure 1: Year of creation of SUNAFIL regional offices



Note: Authors' own elaboration based on data from SUNAFIL. The grey boundaries represent regions.

2.2. The labor inspection process

The enforcement process starts with the inspection itself, which can be triggered either by an employee's complaint about the employer not complying with some regulation, or by one of SUNAFIL's intelligence operations. About 80 percent of inspections in 2014 corresponds to the first case.[6] During the inspection, the SUNAFIL agent issues an inspection order and verifies labor obligations' (non-) compliance. If no violation is detected, the procedure ends. Otherwise, the inspector grants the firm a period to comply with the regulation. If the firm regularizes its status within that period, the case is closed and no fine is issued. If the firm fails to comply, the violation is classified as minor, serious, or very serious depending on the labor standard violated, and SUNAFIL proposes the fine amount based on the severity of the infraction and the number of employees affected. This initiates the administrative procedure, as a result of which SUNAFIL's sanctioning body issues the penalty resolution (*resolución sancionadora*), which includes the amount of the fine to be paid by the firm.[7]

The creation of SUNAFIL's regional offices led to an increase in the number of labor inspectors and, consequently, in the number of inspections conducted and fines issued. Importantly, labor inspectors were assigned to a given SUNAFIL regional office and inspected firms in that region.[8] Our data confirm that cross-regional inspections are rare in practice: only 2 percent of fines are issued by an inspection agency located in

[6]According to information reported in the Annual Labor Inspection Plan (SUNAFIL, 2022).

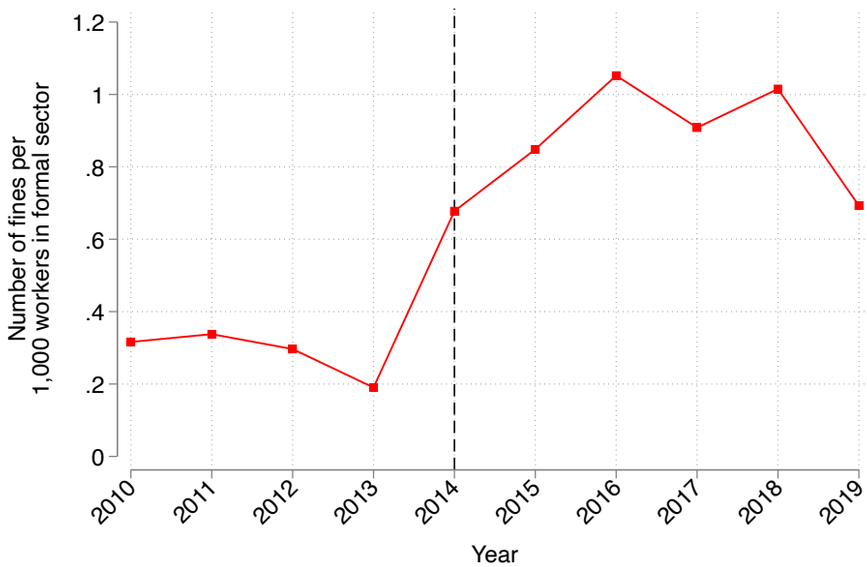
[7]During the administrative procedure, the employer is given the right to present a defense (gather the requested information, present additional evidence, etc.), and an examining body conducts additional revisions. Also, there might be additional instances where the employer can ask for reconsideration, for instance, if additional evidence was not presented in the first place.

[8]This regional jurisdiction is grounded in Peru's labor inspection legal framework, which establishes that labor inspectors shall exercise their functions within the territorial scope of the body to which they are assigned, and that each Regional Intendency is a decentralized unit responsible for directing and executing inspection activities within its respective territorial jurisdiction (see Article 22, "Territorial Distribution of Competencies", of the General Labor Inspection Law 28806, and Article 43 of SUNAFIL's Organizational Rules D.S. 007-2013-TR). In exceptional cases, SUNAFIL's central authority may temporarily assign inspection personnel to a different territorial jurisdiction.

a different region, and 77 percent of those cases correspond to inspections initiated by the Metropolitan Lima headquarters. This evidence supports our assumption that treatment is effectively assigned at the regional level.

Figure 2 presents the evolution in the number of fines issued per 1,000 workers in the formal sector over the period 2010–2019.[9] Until 2013, the average number of violations detected was low —about 0.2 fines for every 1,000 workers—, reflecting the limited enforcement capacity of the regional inspection agencies that operated prior to SUNAFIL's creation.[10] A clear break in this trend appears in 2014, coinciding with the establishment of SUNAFIL and the opening of its first regional offices. The number of fines issued per 1,000 workers in the formal sector increased steeply from that point onward, reaching approximately one fine per 1,000 workers during 2016–2018, an increase of more than 400 percent relative to the pre-SUNAFIL period.

Figure 2: Number of fines issued per 1,000 workers in the formal sector by year



Note: The number of fines refers to the number of inspections that resulted in the issuance of a fine. Data about workers in the formal sector is from ENAHO and refers to individuals working in firms registered with the tax authority (SUNAT), regardless of the worker's own (in)formality condition.

Given that SUNAFIL's offices were gradually rolled out across Peruvian regions, the increase in enforcement was not homogeneous throughout the country. Figure 3 gives an idea of this geographic variation. Each shaded area corresponds to a different district (the smallest territorial unit), and the grey boundaries represent the 25 regions.[11] In 2014, the first year of SUNAFIL's operation (panel a), there were very few districts where inspections that resulted in fines were conducted. One was Metropolitan Lima, home of the headquarters office, which initiated about 84 percent of the 2,800 inspections conducted that year. In addition, the purple dots represent the location of a SUNAFIL regional office and reveal that, overall, the level of enforcement of labor regulations was higher in districts located in regions with a SUNAFIL office. Likewise, panels b and c report the number of inspections that detected violations and the location of SUNAFIL offices in 2017

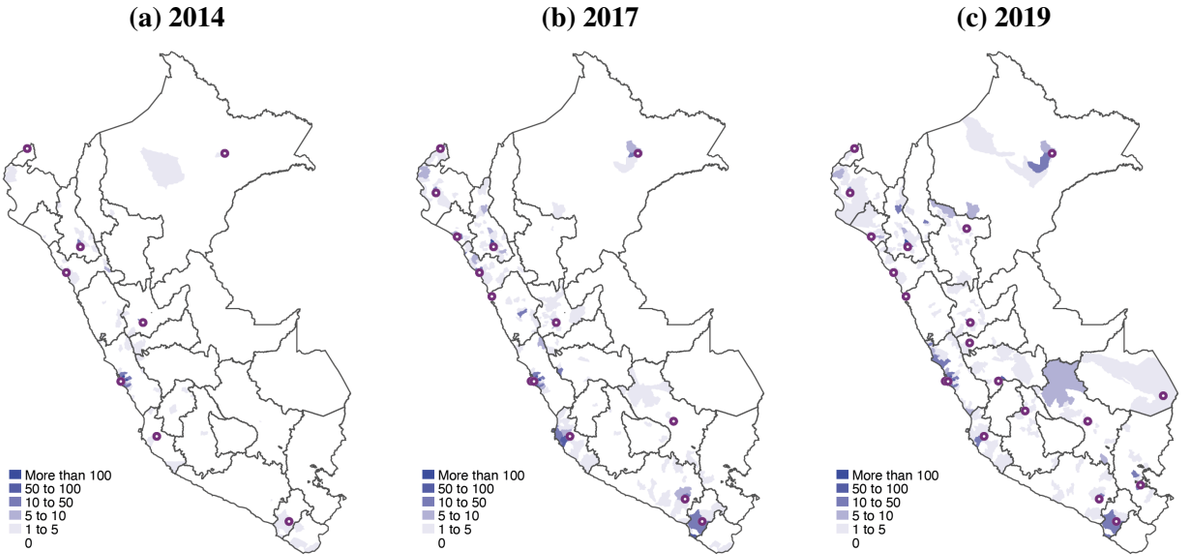
[9]This information refers to the number of fines issued after the detection of a labor law violation, but it does not necessarily reflect fines collected.

[10]Moreover, as pointed out by Díaz et al. (2018), most labor inspectors were concentrated in the regions of Lima and Callao at the time.

[11]There are about 1,800 districts, each of them belonging to one of 196 provinces in 24 *departamentos* and the Constitutional Province of Callao. For conciseness, we refer to those 25 areas as regions.

and 2019. The pattern is clear: there was considerable heterogeneity in the level of enforcement across the territory. In particular, districts in a region with an operating SUNAFIL agency were significantly more likely to be inspected and penalized.[12]

Figure 3: Number of fines issued by SUNAFIL by district



Note: The shaded areas correspond to different districts, the grey boundaries represent regions, and the purple dots represent the location of the corresponding SUNAFIL office in the region. There were 2872, 3925, and 3063 initiated inspections that resulted in fines in 2014, 2017, and 2019, respectively.

In sum, the gradual roll-out of SUNAFIL agencies across different regions induced considerable disparities in the likelihood that a firm was inspected depending on its location. In particular, firms in a region with no inspection agency faced a much lower likelihood of inspection and penalization.

3. Data

We use several sources of data. First, we use administrative records on all labor inspection orders that resulted in penalties for 2010-2020, obtained from SUNAFIL's publicly available online query system.[13] These data cover penalties issued by the entire labor inspection system, including SUNAFIL dependencies and other regional offices.[14] For each penalty resolution, the system provides: the case file number and corresponding resolution number; the issuing inspection agency and the administrative instance (first or second); the date the resolution was issued and its status (e.g., final or appealed); the fine amounts disaggregated by severity of the infraction — minor, serious, and very serious — as well as the total fine amount; the inspection order number and the date of the corresponding infraction report; and the economic sector of the inspected firm,

[12]This can be expected since inspectors go from the regional office to the location of each firm either by car or public transportation (Viollaz, 2018a). Some SUNAFIL offices have only one vehicle, which further restricts the displacement of inspectors throughout the territory. Further, there is also little availability of human, logistical, and financial resources for the various functions that SUNAFIL oversees (SUNAFIL, 2019).

[13]The portal is maintained by SUNAFIL as part of Peru's public transparency framework and is accessible at https://aplicativosweb5.sunafil.gob.pe/si.consultaResoluciones/. The data were retrieved through systematic queries covering all penalty resolutions issued between 2010 and 2020.

[14]Some regional offices under the orbit of the Ministry of Labor that were still operational.

classified by ISIC code.[15] Importantly, the geographic location of each penalty is recorded at the department, province, and district level, which allows us to match each penalty to the corresponding SUNAFIL regional office. We restrict our period of analysis to years up to 2019 since the arrival of the Covid-19 pandemic at the beginning of 2020 affected not only the level of economic activity but also the intensity and modality of labor inspections.[16]

Table 1: Summary statistics of penalty resolutions

	2010-2013	2014-2019
Total number of penalties	4,535	22,208
Number of penalties issued by SUNAFIL	0	21,447
Number of districts that received penalties	185	1,385
% of minor penalties	3.40%	2.30%
% of serious penalties	21.40%	6.60%
% of very serious penalties	75.30%	91.10%
Average amount of minor penalties	104	783
Average amount of serious penalties	6,356	13,282
Average amount of very serious penalties	5,738	35,381
Average penalty amount	11,377	42,894
Average length of process (months)	23.5	20.3
Number of penalties per 1,000 workers in formal sector	0.058	0.438

Note: The penalties are measured in S./ of 2019.

We observe a total of 4,500 penalties issued before the creation of SUNAFIL (until 2013), and about 22,000 after (2014-2019), most of these resulting from inspections conducted by SUNAFIL (see Table 1). The number of districts that received penalties for violating labor standards increased from 185 before 2014 to almost 1,400 afterward. Moreover, the share of penalties considered "very serious" increased from 75 to 91 percent. Table 1 also presents the average penalty amount, depending on the seriousness of the infraction: for the period 2014-2019, the average amount for minor penalties is almost 783 soles, while for severe infractions (which account for more than 90 percent of issued infractions) the average fine is 35,381 soles. This is economically significant: it represents at least 5.5 percent of microenterprises' sales.[17] The length of the inspection process upon the detection of an infraction (i.e., from the issuance of the inspection order until the issuance of a penalty resolution) was reduced, on average, by three months after 2014, revealing an effort on the part of SUNAFIL to speed up the process and reduce burdensome and lengthy procedures. The average number of fines issued increased: for every 1,000 workers in the formal sector, the number of fines went from 0.058 before the creation of SUNAFIL to 0.438 afterward. In addition, we have data on the exact launch date of each SUNAFIL regional office and the exact physical address, which we matched to our dataset of penalties.[18]

[15] In the vast majority of cases, labor inspections in a given region are conducted by the inspection agency from that region. However, in our data, there are 2 percent of cases where the inspection is conducted by an agency from a different region; most of these cases correspond to inspections initiated by the Metropolitan Lima inspection agency.

[16] Due to social distancing measures imposed during the pandemic, labor inspections were conducted online, and prioritized for certain types of violations, such as non-compliance with health and sanitary measures, over others such as labor obligations.

[17] Under the law, firms with sales of about 700,000 soles or less are microenterprises

[18] We exclude from our analysis the five agencies created after 2019: Amazonas, Apurímac, Huancavelica, Tacna, and Ucayali.

Table 2: Summary statistics of outcome and control variables

	2010-2013	2014-2019
Outcome variables:		
Employment rate	70.6	69.6
Participation of formal sector (% of total employment)	29.4	28.2
Informality rate (% of total employment)	75.3	73.1
Informality rate in formal sector	31.8	27.5
Additional control variables:		
Paved roads (% of total roads in the region)	19.1	20.4
Government expenditure executed (%)	68.0	65.2
Regional GDP	24.0	29.2
Share of agriculture in regional GDP (%)	10.6	10.1
Share of manufacture in regional GDP (%)	12.8	11.4
Share of services in regional GDP (%)	19.5	20.3

Note: Gross Domestic Product (GDP) is measured in constant 2007 thousands of soles.

Second, we use data at the individual level from the *Encuesta Nacional de Hogares* (ENAHO), the main National Household Survey to measure individual- and household-level socioeconomic outcomes in Peru, from 2010 to 2019. ENAHO contains information about individuals' reported employment status, including workers' informality condition, as well as their earnings, hours of work, and economic sector of activity.[19] Workers report whether the firm (or person) that employs them is registered with the Tax Authority (SUNAT), either as a legal or an individual entity (with an associated taxpayer number), allowing us to distinguish between workers in the formal and the informal sector of the economy.[20] This last piece of information is of particular interest to us, since it allows us to identify workers employed in the formal sector of the economy, which is the one targeted by labor inspections.[21] Our main measures of labor outcomes are employment, employment in the formal sector (i.e., registered with the SUNAT), informality status, and informality status within the formal sector (i.e. informal workers employed by a firm registered with SUNAT).[22] The sample of interest is restricted to individuals aged 15 or older. Importantly, the ENAHO is designed to be representative at the regional level, which is the geographical unit of analysis in this study, ensuring that the regional variation exploited by our identification strategy is not driven by sampling artifacts. In addition, the ENAHO is the only nationally representative household survey in Peru that systematically collects detailed information on labor informality. Alternative labor data sources in Peru (e.g., administrative records) typically cover only formal workers, and therefore cannot be used to construct comparable measures of informality.

[19]The INEI defines informal employment as those jobs that meet the following conditions: i) employers and self-employed workers whose productive unit belongs to the informal sector, ii) wage earners without social security financed by their employer, and iii) unpaid family workers, independently of the formal or informal nature of the productive unit where they work.

[20]Interestingly, being registered with SUNAT does not necessarily imply compliance with the tax legislation, since some firms registered with the tax authority do not file taxes (Díaz et al., 2018).

[21]Workers also report whether the firm maintains accounting records. Interestingly, 2.6 percent of workers reporting that their firms are registered with SUNAT also report that they do not keep accounting records, which may be due to reporting errors. Our results, however, are robust to using this alternative variable to define the formal sector.

[22]Figure A.2 in the Appendix presents a diagram showing the different components of employment, which helps understand the various outcomes analyzed. Thus, "employed" is a dummy variable equal to one if the individual is employed and zero otherwise, while "employment in the formal sector" is equal to one only if the individual is employed in the formal sector (box 3 in the diagram) and zero if employed in the informal sector (box 6). "Informal" is a dummy variable that equals one for informal workers (boxes 5 and 6 in the diagram) and zero for formal workers (box 4), and "informality in the formal sector" considers, for the universe of workers in the formal sector (boxes 4 and 5), whether the worker is informal (box 5) or formal (box 4).

Table 2 presents basic descriptive statistics of the outcomes measured with ENAHO for our sample. The table shows a slight decline in the prevalence of labor informality, from about 75 percent in 2010-2013 to 73 percent in 2014-2019. Within the formal sector, the informality rate decreased as well, from 32 to 28 percent on average, even though the participation of the formal sector in total employment decreased slightly.

Third, we use administrative data on district and regional-level characteristics. As a measure of institutional quality at the district level, we use the execution of public investment in projects as a percentage of the budget from the Ministry of Economy (MEF). Further, we use information on regional development indicators, including the gross added value and the share of GDP accounted for by the agriculture, manufacturing, and service sectors at the regional level from the Instituto Nacional de Estadística e Informática (INEI). We also use the Ministry of Transport and Communications' information on the percentage of paved regional roads to measure accessibility.

In the following section, we present two empirical strategies to estimate the impact of an increased enforcement of labor regulations on labor informality. The first, widely used in the literature, is based on standard Two-Way Fixed Effects (TWFE) models. The second, more recent and robust, uses an extended estimator that avoids the usual biases in studies with a staggered treatment adoption, as in our study. This comparison allows us to evaluate to what extent the empirical results depend on the underlying assumptions or not.

4. Empirical strategy

This paper seeks to estimate the causal effect of increased enforcement of labor regulations on the degree of compliance with labor law, measured through labor informality outcomes. The main empirical challenge is that labor inspections and fines are not randomly allocated. Instead, it is likely that sectors or districts with higher intrinsic non-compliance may be more likely to be inspected and penalized, potentially biasing naïve estimates upward. Conversely, regions with greater institutional capacity may be more effective in enforcing labor regulations and experience different labor market dynamics, inducing additional sources of endogeneity.

To identify the impact of interest, we exploit the fact that SUNAFIL was established as a central labor inspection authority in 2014, while its different regional offices began operations at various points in time, as illustrated in Figure 1. This gradual rollout generated a staggered difference-in-differences setting, in which regions were exposed to increased enforcement intensity at different points in time. For instance, a firm located in the Ica region faced a higher risk of detection in 2014, when the Ica Regional Office opened, whereas a comparable firm in the Cusco region experienced a similar increase only in 2017, following the opening of the Cusco Regional Office.

Given that recent methodological advances have shown that standard two-way fixed effects (TWFE) estimators may produce biased estimates when treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon, 2021),[23] accounting for such heterogeneity is particularly important in our context in light of the institutional features of SUNAFIL's roll-out — which plausibly allow treatment effects to evolve with exposure length. Accordingly, our main specification relies on the Extended Two-Way Fixed Effects (ETWFE) estimator proposed by Wooldridge (2021b), which addresses the problems mentioned above by estimating separate treatment effects for each cohort-period combination, ensuring that only appropriate com-

[23] Goodman-Bacon (2021) shows that the treatment effect estimate obtained from a TWFE model is a weighted average of all possible 2×2 difference-in-differences comparisons between groups of units treated at different points in time, including comparisons between earlier and later treated units. When treatment effects are homogeneous across treated groups and across time, the TWFE estimator is consistent for the average treatment effect on the treated (ATT). Conversely, when treatment effects are heterogeneous, these weights can be negative or non-convex, potentially biasing the overall estimate.

parisons —between treated units and those not yet or never treated— inform the estimates.[24]

Let regions be indexed by r and years by t=2010,...,2019. Treatment occurs when a SUNAFIL office begins operating in a region; let q denote the region's treatment year. Our benchmark ETWFE specification is given by:

$$\mathrm{Y}_{irt} = \eta + \sum_{k=q}^{T}\sum_{s=k}^{T} \tau_{ks}\left(w_{rt}\cdot d_{rk}\cdot fs_t\right) + \sum_{k=q}^{T}\lambda_k d_{irk} + \sum_{s=2}^{T}\theta_s fs_t + u_{irt}, \tag{1}$$

where $Y_{irt}(k)$ is the observed labor market outcome of individual i living in region r at exposure length t, w_{rt} is an indicator taking the value 1 if region r is treated in year t, d_{rk} is an indicator taking the value 1 if region r belongs to treatment cohort k, fs_t are year fixed effects (i.e. $fs_t = 1$ if $s = t$ and 0 otherwise), and u_{irt} is the error term. This specification fully interacts cohort and year indicators to estimate cohort-specific dynamic treatment effects.

The parameters τ_{ks} capture the average treatment effect for cohort k in year s. In Section 5, we report both the average of the different group-time average treatment effect estimates, i.e. $\overline{\widehat{\tau}} = \frac{1}{(T-q+1)(T-q+2)/2}\sum_{k=q}^{T}\sum_{s=k}^{T}\widehat{\tau}_{ks}$, as well as the dynamic effects by length of exposure. This specification allows treatment effects to vary flexibly across cohorts and exposure duration, ensuring consistent estimation of average treatment effects under staggered treatment adoption.

Identification of the ETWFE specification relies on two assumptions: no anticipation and parallel trends. The no anticipation assumption requires that outcomes are not affected by future treatment before it actually occurs. The parallel trends assumption requires that, in the absence of treatment, regions in which a SUNAFIL office opened at different points in time would have experienced similar trajectories in employment and informality outcomes relative to regions that were not yet or never treated within our period of study. Table 3 shows that the year of creation of a SUNAFIL office is largely uncorrelated with regional characteristics at the time of opening, including the informality rate, urban share, average age, road infrastructure, and sectoral composition. Moreover, the event-study estimates presented in Section 5 show no evidence of differential pre-trends for any of the outcomes analyzed, providing direct support for the parallel trends assumption.

It is important to mention that our identification strategy shares a common characteristic with most of the existing literature: we measure enforcement at the region level rather than at the firm level. This is not a limitation unique to our study and it reflects the data constraints common to this body of literature. The vast majority of studies in this literature construct enforcement measures at some geographic level and relate them to labor market outcomes measured from household surveys, censuses, or administrative records, without observing whether any specific firm was inspected.[25] An exception is De Andrade et al. (2016), who conduct a randomized experiment in Brazil, in which individual firms are directly assigned to receive

[24] A number of alternative estimators have been developed with the same objective, including Callaway and Sant'Anna (2021), Sun and Abraham (2021), de Chaisemartin and D'Haultfœuille (2020), and Borusyak et al. (2024). As discussed by Roth et al. (2023) and de Chaisemartin and D'Haultfœuille (2023), these estimators share the same fundamental identification strategy and, under comparable assumptions, target the same parameters; the key differences are practical. We choose the ETWFE estimator over other heterogeneity-robust alternatives. Unlike event-study-type estimators (Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020), which require parallel trends to hold unconditionally, ETWFE readily accommodates time-invariant covariates, allowing them to have cohort-specific effects through interactions with cohort and time indicators. While Callaway and Sant'Anna (2021) also allow for conditional parallel trends, they do so through inverse probability weighting in which covariate effects do not vary across cohorts. In addition, ETWFE exploits parallel trends across the entire pre-treatment period rather than only the period immediately before treatment, yielding more precise estimates — an important consideration given the small number of treatment cohorts in our setting. While the imputation estimators proposed by Borusyak et al. (2024) and Gardner (2022) are closely related to ETWFE, they differ mostly in implementation. As shown by Wooldridge (2021b), the ETWFE and Callaway-Sant'Anna regression-adjustment estimators are numerically equivalent without covariates and using the never-treated group as the comparison.

[25] For example, Almeida and Carneiro (2012) and Almeida and Poole (2017) for Brazil, Viollaz (2018a) and Viollaz (2018b) for Peru and Argentina, and Bhorat et al. (2012) for South Africa

Table 3: Correlation of timing of implementation and regional indicators

	Year of creation of SUNAFIL office
Informality rate	0.009
	(1.07)
Average age	-0.078
	(-0.55)
Urban	-0.005
	(-0.40)
Public investment execution (%)	-3.058*
	(-2.75)
Share of paved roads	0.007
	(0.35)
Agricultural share	0.006
	(0.86)
Manufacturing share	-0.019
	(-1.83)
Services share	0.0008
	(0.11)
Observations	20

Note: Each row is a separate OLS regression of the year of SUNAFIL office creation on a given regional characteristic at the time of opening. The informality rate, average urban rate, and average age refer to regional averages calculated from ENAHO (INEI). Public investment execution measures the public investment expenditure as a share of approved budget (from *Ministry of Economy*). The share of paved roads is obtained from the *Ministry of Transport and Communications*, and sectoral shares come from INEI regional accounts. *t* statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

inspections or incentives, allowing them to identify the direct effect of enforcement on the treated firm. The inability to observe firm-level inspections and outcomes means that our estimates —like those of most studies in the literature— capture the average response of all firms in a geographic area to a change in the general enforcement environment, rather than the direct effect of an inspection on the inspected firm. As a result, the estimated effects reflect a combination of direct effects on firms that are actually inspected and indirect deterrence effects on firms that may alter their behavior in response to the perceived increase in the probability of detection. To the extent that the increase in enforcement intensity is diluted across all firms in a region, the region-level estimates will tend to be attenuated toward zero relative to the effect that enforcement would have on the firms actually targeted. We acknowledge this limitation while noting that our event study design, which exploits the staggered roll-out of SUNAFIL offices, provides a credible source of temporal and geographic variation in enforcement intensity that is arguably exogenous to contemporaneous labor market conditions in each region.

As a benchmark, we also estimate the effects using a standard TWFE specification, whose results are reported in Appendix B. While this comparison is informative, we caution that TWFE estimates in our setting may be contaminated by the biases described above, and therefore do not constitute our preferred estimates.

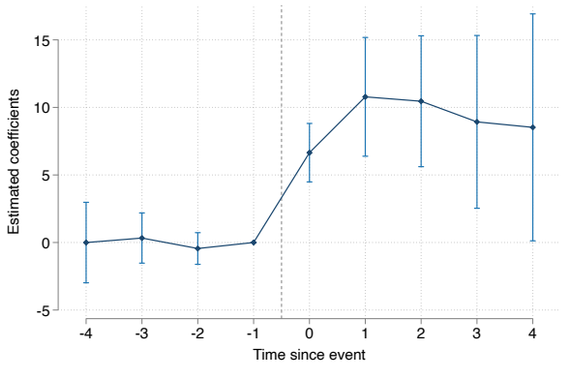
5. Main results

5.1. Effect on measures of enforcement

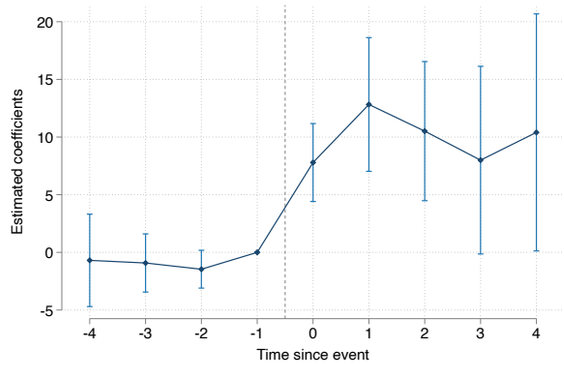
Before examining the effect on labor market outcomes, we first examine whether the opening of a SUNAFIL regional office led to a measurable increase in the enforcement of labor regulations. Figure 4a displays the estimated coefficients from an event-study specification on the log of the number of fines issued per 1,000 workers in the formal sector for different years relative to the opening of each regional office, controlling for calendar-year and region fixed effects. The trend in fines issued changed dramatically following the opening of a SUNAFIL office: relative to the last pre-event period, the number of fines per 1,000 workers increased by more than 600 percent in the first year of SUNAFIL's operations, and continued to rise thereafter, with all post-event coefficients statistically significant at least at the 95% level (see Table A.2 in the Appendix). Likewise, Figure 4b plots the log of the total monetary amount of fines issued per 1,000 workers in the formal sector (measured in soles of 2019), and shows an equally steep increase: the total amount of fines rose by approximately 700 percent in the first year of SUNAFIL's operations, and by more than 900 percent on average over the four years following the office opening. Importantly, none of the estimated coefficients are statistically significant for periods before $j = 0$, confirming the absence of pre-existing trends in enforcement activity. Overall, these results show that opening a SUNAFIL regional office implied a sharp and significant increase in the enforcement of labor regulations, measured by the number and size of fines issued, providing a strong first stage for the analysis that follows.

Figure 4: Impact of the opening of the SUNAFIL offices on penalties issuance

(a) Log of number of fines per 1,000 workers in the formal sector



(b) Log of total amount of fines per 1,000 workers in the formal sector



Note: The dots and bars represent the coefficients and 95 percent confidence intervals based on standard errors clustered at the regional level. The full regression output is shown in Table A.2 in the Appendix.

5.2. Effect on labor market outcomes

This section presents the main findings of the paper. Table 4 presents the aggregate ATT estimates from the ETWFE specification in Equation 1. Column 1 reports results using never-treated regions as the comparison group, while Column 2 uses not-yet-treated regions.[26] The results show no statistically significant effect of

[26]While never-treated regions are often considered the cleanest control group -minimizing potential contamination across units- not yet treated regions may offer a more relevant counterfactual, as they are on a similar treatment trajectory, therefore exhibit more comparable pre-trends with treated units.

the opening of a regional SUNAFIL agency on total employment or employment in the formal sector of the economy, suggesting that SUNAFIL offices roll-out did not lead to unintended job losses. Furthermore, we find no statistically significant effects of SUNAFIL's presence on overall informality or on informality within the formal sector. These null results hold regardless of whether the comparison group consists of never-treated or not-yet-treated regions.

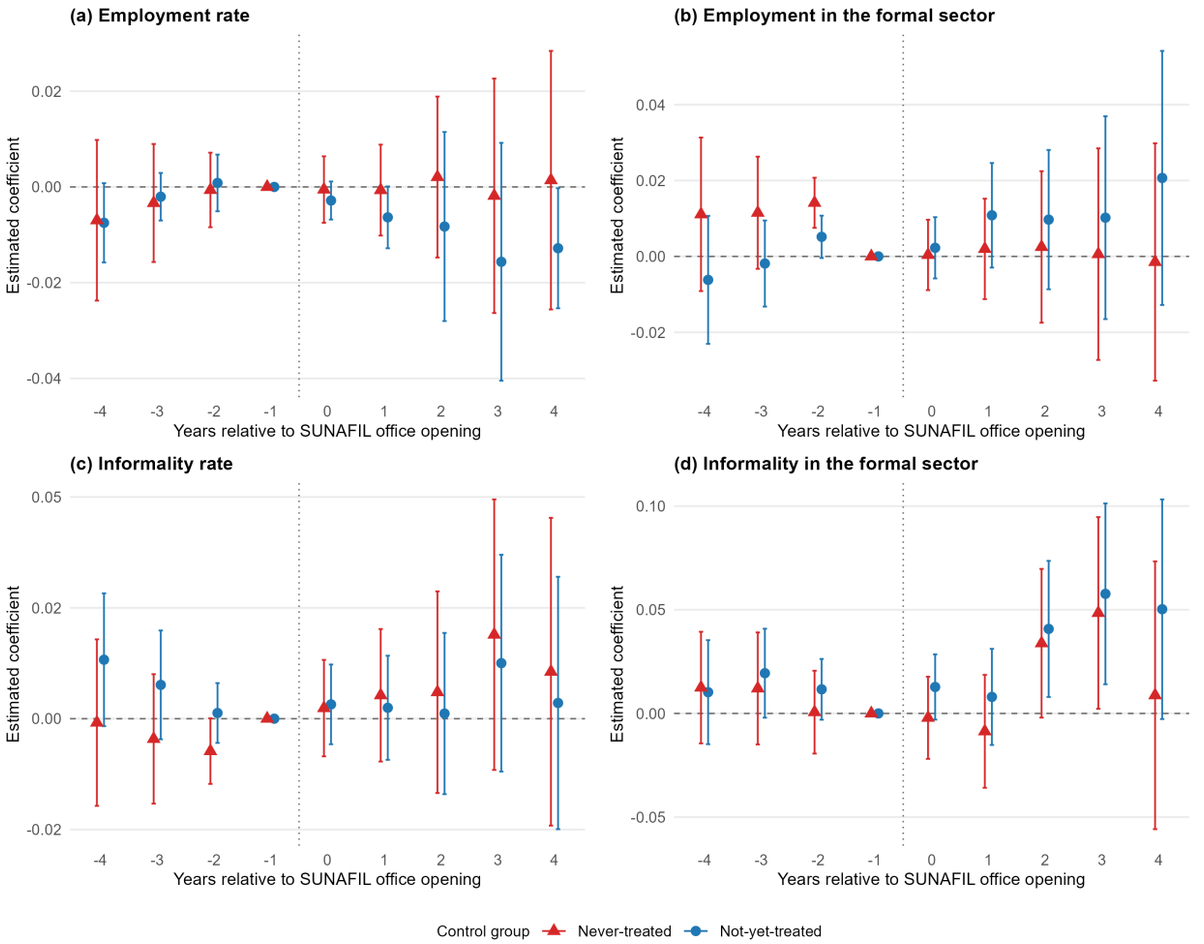
Table 4: Extended TWFE estimates of the impact of the opening of a SUNAFIL office on employment and informality

	(1)	(2)
Employment	0.004	0.001
	(0.007)	(0.004)
Employment, formal sector	0.002	0.008
	(0.006)	(0.005)
Informality	0.001	-0.002
	(0.006)	(0.004)
Informality, formal sector	-0.004	-0.002
	(0.012)	(0.016)
Comparison group	Never treated	Not yet treated

Note: All regressions also include cohort and year fixed effects. The estimations are population-weighted, and the standard errors are clustered at the regional level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

In addition, Figure 5 presents the dynamic treatment effects from the ETWFE specification in Equation 1, using never-treated (red) and not-yet-treated (blue) regions as comparison groups. Reassuringly, we find no evidence of differential pre-trends for any of the outcomes analyzed: the estimates are consistently close to zero and statistically indistinguishable from zero before the opening of a regional SUNAFIL agency for all outcomes analyzed, supporting the validity of the primary parallel trends assumption. In the post-treatment periods, no statistically significant effects on informality or employment outcomes emerge at any horizon.

Figure 5: Impact of the opening of a SUNAFIL office on employment and informality, ETWFE without covariates



Note: The dots and bars represent the coefficients and 95 percent confidence intervals based on standard errors clustered at the regional level. Red triangles use never-treated regions as the comparison group; blue circles use not-yet-treated regions.

Overall, our results indicate that a SUNAFIL regional office did not lead to measurable changes in labor market outcomes, regardless of the length of exposure to treatment. These findings stand in contrast to what a standard TWFE specification would suggest. As reported in Appendix B, the TWFE estimates indicate modest but statistically significant eductions in informality following the opening of a SUNAFIL office (a decline of about 1.4 percentage points in the overall informality rate and 1.8 percentage points within the formal sector). However, as discussed in Section 4, TWFE estimates in staggered adoption settings can be biased when treatment effects are heterogeneous, due to implicit comparisons in which earlier-treated units serve as controls for later-treated ones. The disappearance of these effects under the ETWFE estimator is consistent with this concern, and underscores the importance of using estimators that are robust to treatment effect heterogeneity in staggered settings such as ours. We thus interpret the TWFE results as a useful benchmark, while relying on the ETWFE as our preferred specification.

5.3. Robustness checks

SUNAFIL inspectors might face financial constraints when traveling throughout the territory to conduct inspections. Given these restrictions regarding physical and human capital and accessibility challenges in some areas– for instance, due to dense jungle terrain or heavy rainfall– it is possible that SUNAFIL's inspections are not reaching the entire country. If this were the case, we might expect the estimated effects to be downward biased. To test this hypothesis, Table 5 reports the results of the ETWFE estimation but excluding from the sample the districts where no inspection order or fine has been recorded during our period of study. These findings confirm the absence of effect of SUNAFIL's rollout, both overall and within the formal sector. We observe a marginally significant positive impact on formal employment when using never-treated regions as the comparison group. Still, it disappears when not-yet-treated units are used as the comparison group.

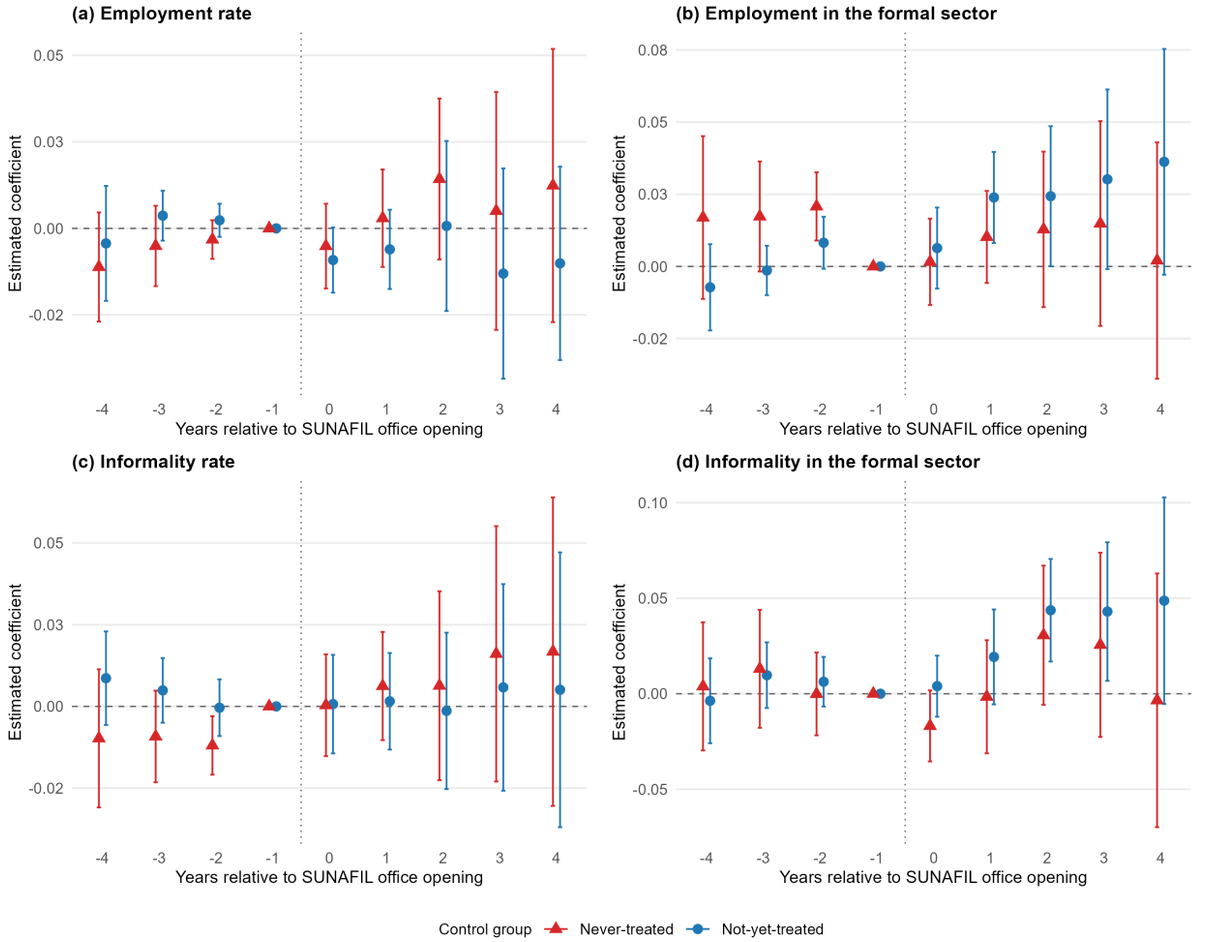
Table 5: Robustness to the exclusion of districts never inspected, ETWFE

Comparison group	Employment	Employment, formal sector	Informality	Informality, formal sector
Never treated	-0.003	0.012*	-0.005	0.002
	(0.006)	(0.007)	(0.009)	(0.015)
Not yet treated	0.001	0.008	-0.002	-0.002
	(0.004)	(0.005)	(0.004)	(0.016)

Note: The estimations are population-weighted, and the standard errors are clustered at the regional level.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Figure 6 presents the corresponding event-study estimates. The results confirm the absence of significant effects on employment or informality at any horizon, and show no evidence of differential pre-trends, reinforcing the conclusion that the null findings are not driven by the inclusion of districts that were never effectively reached by enforcement.

Figure 6: Impact of the opening of a SUNAFIL office on employment and informality, ETWFE restricting to inspected districts



Note: Sample restricted to districts where at least one SUNAFIL inspection was conducted during the period of study. The dots and bars represent the coefficients and 95 percent confidence intervals based on standard errors clustered at the regional level. Red triangles use never-treated regions as the comparison group; blue circles use not-yet-treated regions.

5.4. Discussion of results

Several factors may explain the limited effects of SUNAFIL's enforcement efforts on labor informality. First, from a methodological standpoint, the fact that our treatment variable captures exposure to enforcement at the regional level rather than actual enforcement at the firm level may lead to attenuation bias in our estimated impacts. This measurement concern should be kept in mind when interpreting our results.

Beyond this methodological consideration, there are other reasons that could account for the absence of measurable effects. One possibility is that, despite the significant increase in inspections following the creation of SUNAFIL, enforcement capacity remains insufficient. As is often the case in developing countries with high levels of informality, inspection work is constrained by the lack of inspectors, insufficient infrastructure, equipment, and vehicle units. In addition, the institutional framework limits the scope of enforcement, with most inspections being reactive rather than preventive. Data from the World Bank Enterprise Surveys are consistent with this hypothesis: the share of firms visited or required to meet with tax officials in Peru (22.1

percent) is lower than both the Latin American and Caribbean average (27.7 percent) and significantly below the global average (39.1 percent), suggesting that the level of inspection activity in Peru remains comparatively low (see Table A.3).

A second possibility is that corruption in the inspection process undermines the effectiveness of enforcement, as firms may resort to bribes to avoid official fines and circumvent compliance. Yet Peru does not appear to be an outlier along this dimension: the share of firms expected to give gifts in meetings with tax officials (8.1 percent) is higher than the regional average (4.8 percent) but in line with the global average, and other corruption indicators such as bribery incidence and bribery depth are below both the regional and global benchmarks (see Table A.3).

Third, if the monetary value of the fines is sufficiently low relative to the cost of compliance, firms would have weak incentives to adjust their behavior. As reported in Section 3, the average fine for serious and very serious infractions is approximately 35,000 soles. While this is economically significant for the smallest firms under SUNAFIL's jurisdiction — representing about 5.6 percent of annual sales for firms just above the microenterprise threshold — it amounts to less than 0.5 percent of annual sales for firms at the upper bound of the small-firm category, and an even smaller share for medium and large firms.[27] This suggests that, for a large share of firms under SUNAFIL's purview, the expected penalty may be too low to deter non-compliance.

Finally, even with adequate enforcement capacity, an appropriate institutional framework, and meaningful penalties, there is likely an upper bound to the potential impacts of enforcement on informality given the low levels of productivity that often characterize informal jobs. Forcing the formalization of such jobs could lead to unintended consequences such as job destruction or employment precarization (see, for example, Almeida et al. 2022, and Bossavie et al. 2019 for the case of minimum wages).

6. Conclusion

This paper studies the impact of increasing the enforcement of labor inspections on labor market outcomes in Peru for 2010-2019. We exploit the decentralization of SUNAFIL–the National Labor Inspection Office–as a source of variation in the level of public enforcement and take advantage of its gradual rollout in Peruvian regions. This decentralization was aimed at consolidating all labor inspections and standardizing the criteria applied when dealing with labor law infractions. Our findings point out a large and significant increase in the number and size of fines in response to the roll-out of the SUNAFIL's regional offices. However, we do not find any impacts on total employment or informality. Several factors may help account for this, including the fact that our treatment variable captures regional exposure to enforcement rather than firm-level enforcement as well as persistent constraints on inspection capacity.

Our paper contributes to the literature that studies the effectiveness of labor enforcement regulations through a large-scale intervention. Evaluating labor inspections as instruments to reduce informality *vis-à-vis* other policies is essential, especially considering the cost of such intervention. Other policies to reduce informality include information campaigns (De Andrade et al., 2016), tax reductions and bureaucracy simplification policies (Monteiro and Assunção, 2012), reductions in payroll taxes (Bernal and Eslava, 2017), or simplification of labor registration procedures (Ronconi and Colina, 2011).

[27] Under Peruvian law (Law 30056), firm size categories are defined by annual sales in Tax Units (UIT). Using the 2019 UIT value of 4,200 soles, the microenterprise ceiling is 150 UIT (630,000 soles) and the small-firm ceiling is 1,700 UIT (7.14 million soles).

References

Almeida, Rita and Pedro Carneiro (2012), "Enforcement of labor regulation and informality." *American Economic Journal: Applied Economics*, 4, 64–89.

Almeida, Rita K, Lourenço S Paz, and Jennifer P Poole (2022), "Precarization or protection? The role of trade and labour policies on informality." *The Journal of Development Studies*, 58, 1416–1435.

Almeida, Rita K and Jennifer P Poole (2017), "Trade and labor reallocation with heterogeneous enforcement of labor regulations." *Journal of Development Economics*, 126, 154–166.

Bernal, Raquel and Marcela Eslava (2017), "Switching from Payroll Taxes to Corporate Income Taxes."

Betcherman, Gordon (2015), "Labor market regulations: What do we know about their impacts in developing countries?" *The World Bank Research Observer*, 30, 124–153.

Bhorat, Haroon, Ravi Kanbur, and Natasha Mayet (2012), "Estimating the Causal Effect of Enforcement on Minimum Wage Compliance: The Case of S outh A frica." *Review of Development Economics*, 16, 608–623.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess (2024), "Revisiting Event-Study Designs: Robust and Efficient Estimation." *The Review of Economic Studies*, 91, 3253–3285.

Bossavie, Laurent, Ayşenur Acar Erdoğan, and Mattia Makovec (2019), "The impact of the minimum wage on firm destruction, employment and informality." *World Bank Policy Research Working Paper*.

Callaway, Brantly and Pedro H.C. Sant'Anna (2021), "Difference-in-Differences with multiple time periods." *Journal of Econometrics*, 225, 200–230, URL https://linkinghub.elsevier.com/retrieve/pii/S0304407620303948.

De Andrade, Gustavo Henrique, Miriam Bruhn, and David McKenzie (2016), "A helping hand or the long arm of the law? Experimental evidence on what governments can do to formalize firms." *The World Bank Economic Review*, 30, 24–54.

de Chaisemartin, Clément and Xavier D'Haultfœuille (2023), "Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey." *The Econometrics Journal*, 26, C1–C30.

de Chaisemartin, Clément and Xavier D'Haultfœuille (2020), "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110, 2964–2996.

Díaz, Juan José, Juan Chacaltana, Iamele P Rigolini, and Claudia Ruiz Ortega (2018), "Pathways to formalization: going beyond the formality dichotomy–the case of Peru." *World Bank Policy Research Working Paper*.

Gardner, John (2022), "Two-Stage Differences in Differences.", URL https://arxiv.org/abs/2207.05943. Working Paper.

Goodman-Bacon, Andrew (2021), "Difference-in-differences with variation in treatment timing." *Journal of econometrics*, 225, 254–277.

INEI (2019), "Producción y empleo informal en el Perú." *Cuenta Satélite de la Economía Informal 2007-2018*.

INEI (2020), *Producción y Empleo Informal en el Perú: Cuenta Satélite de la Economía Informal 2007-2019*. Instituto Nacional de Estadística e Informática, Lima, URL dehttps://www.inei.gob.pe/media/MenuRecursivo/publicaciones_digitales/Est/Lib1764/libro.pdf.

Jessen, Jonas and Jochen Kluve (2021), "The effectiveness of interventions to reduce informality in low-and middle-income countries." *World Development*, 138, 105256.

Kahn, Lawrence M (2007), "The impact of employment protection mandates on demographic temporary employment patterns: International microeconomic evidence." *The Economic Journal*, 117, F333–F356.

Loayza, Norman (2018), "Informality: why is it so widespread and how can it be reduced?" *World Bank Research and Policy Briefs*.

Meghir, Costas, Renata Narita, and Jean-Marc Robin (2015), "Wages and informality in developing countries." *American Economic Review*, 105, 1509–1546.

Monteiro, Joana CM and Juliano J Assunção (2012), "Coming out of the shadows? Estimating the impact of bureaucracy simplification and tax cut on formality in Brazilian microenterprises." *Journal of Development economics*, 99, 105–115.

Ohnsorge, Franziska and Shu Yu (2022), *The long shadow of informality: Challenges and policies*. The World Bank, Washington.

Perry, Guillermo, William Maloney, Omar Arias, Pablo Fajnzylber, Andrew Mason, and Jaime Saavedra-Chanduvi (2007), *Informality: Exit and Exclusion*. World Bank Latin American and Caribbean Studies, The World Bank, Washington.

Ronconi, Lucas and Jorge Colina (2011), "Simplification of labor registration in Argentina: Achievements and pending issues." Technical report, IDB Working Paper Series.

Roth, Jonathan, Pedro H.C. Sant'Anna, Alyssa Bilinski, and John Poe (2023), "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature." *Journal of Econometrics*, 235, 2218–2244.

Sun, Liyang and Sarah Abraham (2021), "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects." *Journal of Econometrics*, 225, 175–199.

SUNAFIL (2019), "Informe Anual de Inspección del Trabajo 2019." *Informes Anuales*.

SUNAFIL (2022), *Plan Anual de Inspección del Trabajo*. Superintendencia Nacional de Fiscalización Laboral.

Ulyssea, Gabriel (2018), "Firms, Informality, and Development: Theory and Evidence from Brazil." *American Economic Review*, 108, 2015–2047.

Viollaz, Mariana (2018a), "Are labour inspections effective when labour regulations vary according to the size of the firm? Evidence from Peru." *International Labour Review*, 157, 213–242.

Viollaz, Mariana (2018b), "Enforcement of labor market regulations: heterogeneous compliance and adjustment across gender." *IZA Journal of Labor Policy*, 7, 2.

Wooldridge, Jeffrey M. (2021a), "Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators.", URL http://dx.doi.org/10.2139/ssrn.3906345. Available at SSRN: https://ssrn.com/abstract=3906345.

Wooldridge, Jeffrey M. (2021b), "Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators." *The Econometrics Journal*, 24, C1–C31.

Appendix

A. Tables and figures

Table A.1: Impact of the opening of a SUNAFIL office on employment and informality

Dependent variable:	Employment			Employment in formal sector			Informality			Informality in formal sector		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Four years prior	0.001	0.002	-0.005	-0.006	-0.007	-0.002	0.009	0.013**	0.008	0.006	0.014	0.019
	(0.007)	(0.007)	(0.007)	(0.013)	(0.011)	(0.010)	(0.006)	(0.005)	(0.006)	(0.012)	(0.012)	(0.013)
Three years prior	-0.000	-0.000	-0.005	0.005	0.002	0.005**	-0.001	0.004	0.001	0.009	0.013	0.017*
	(0.005)	(0.005)	(0.005)	(0.008)	(0.007)	(0.007)	(0.004)	(0.004)	(0.005)	(0.011)	(0.010)	(0.009)
Two years prior	0.006	0.006	0.003	0.008	0.007	0.009***	-0.002	-0.001	-0.003	0.010	0.013	0.015
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.011)	(0.011)	(0.011)
One year prior	-	-	-	-	-	-	-	-	-	-	-	-
Implementation	-0.006*	-0.007**	-0.006*	0.014***	0.011**	0.010**	-0.019***	-0.016***	-0.015***	-0.021**	-0.020**	-0.021**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)	(0.008)	(0.008)	(0.008)
One year after	-0.004	-0.006	-0.002	0.019**	0.018***	0.016**	-0.019***	-0.020***	-0.017***	-0.037***	-0.037***	-0.040***
	(0.005)	(0.005)	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.005)	(0.013)	(0.012)	(0.011)
Two years after	0.000	-0.002	0.005	0.020**	0.020**	0.015*	-0.030**	-0.033***	-0.028***	-0.030**	-0.032**	-0.037***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)	(0.008)	(0.011)	(0.010)	(0.008)	(0.014)	(0.013)	(0.012)
Three years after	0.003	-0.000	0.009	0.024	0.023*	0.017	-0.024**	-0.028***	-0.021*	-0.031	-0.035*	-0.042*
	(0.008)	(0.009)	(0.009)	(0.016)	(0.014)	(0.014)	(0.012)	(0.010)	(0.010)	(0.020)	(0.019)	(0.021)
Four years after	-0.002	-0.005	0.008	0.025	0.027	0.018	-0.028**	-0.034***	-0.025**	-0.054**	-0.058**	-0.067**
	(0.009)	(0.010)	(0.010)	(0.019)	(0.017)	(0.017)	(0.013)	(0.012)	(0.012)	(0.025)	(0.023)	(0.027)
Control variables:												
Individual and household-level controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Public investment execution	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Percentage of paved roads	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Regional GDP	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
% of regional GDP in agriculture, manufacture, and services	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	829,131	829,034	829,034	567,838	567,779	567,779	594,820	594,757	594,757	137,416	137,396	137,396

Note: All regressions also include district and year fixed effects. The coefficients correspond to different leads and lags of SUNAFIL office openings in the region. In particular, each variable takes a value of 1 if the individual lives in a region four years before the opening of a SUNAFIL office in that region, two years prior, the year of opening, one year after, two years after, three years after, and four years after. The estimations are population-weighted, and the standard errors are clustered at the regional level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

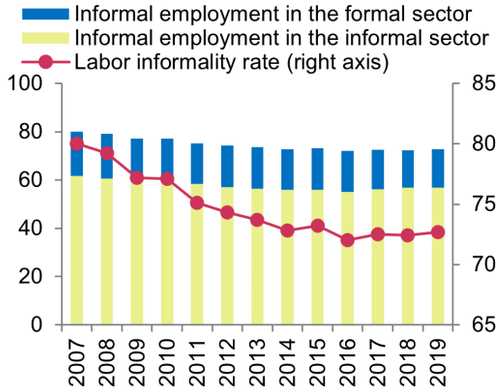
Table A.2: Impact of the opening of SUNAFIL regional offices on enforcement

Dependent variable:	Log (fines per 1,000 workers in formal sector)	Log (fine amount per 1,000) workers in formal sector)
Four years prior	-0.00397	-0.693
	(1.443)	(1.941)
Three years prior	0.329	-0.921
	(0.900)	(1.222)
Two years prior	-0.445	-1.461*
	(0.568)	(0.794)
One year prior	-	-
Implementation	6.650***	7.791***
	(1.048)	(1.638)
One year after	10.78***	12.82***
	(2.130)	(2.809)
Two years after	10.46***	10.51***
	(2.347)	(2.922)
Three years after	8.929***	7.996*
	(3.099)	(3.943)
Four years after	8.524**	10.40**
	(4.071)	(4.980)
Observations	828,273	830,089
District and year FE	Yes	Yes

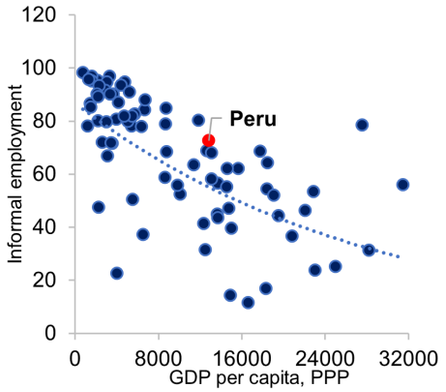
Note: All regressions include district and year fixed effects. The coefficients correspond to different leads and lags of SUNAFIL office openings in the region. In particular, each variable takes a value of 1 if the individual lives in a region four years before the opening of a SUNAFIL office in that region, two years prior, the year of opening, one year after, two years after, three years after, and four years after. Standard errors clustered at the regional level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Figure A.1: Labor informality in Peru

(a) Labor informality, 2007-2019 (% of total employment)



(b) Labor informality and GDP per capita (% of total employment, constant 2017 international $)



Source: Own elaboration based on data from ENAHO (Panel a) and ILO, World Bank (Panel b).
Note: Both figures measure informality as a percentage of total employment. GDP per capita (PPP) in Panel b is measured in constant 2017 international dollars.

Figure A.2: Components of employment

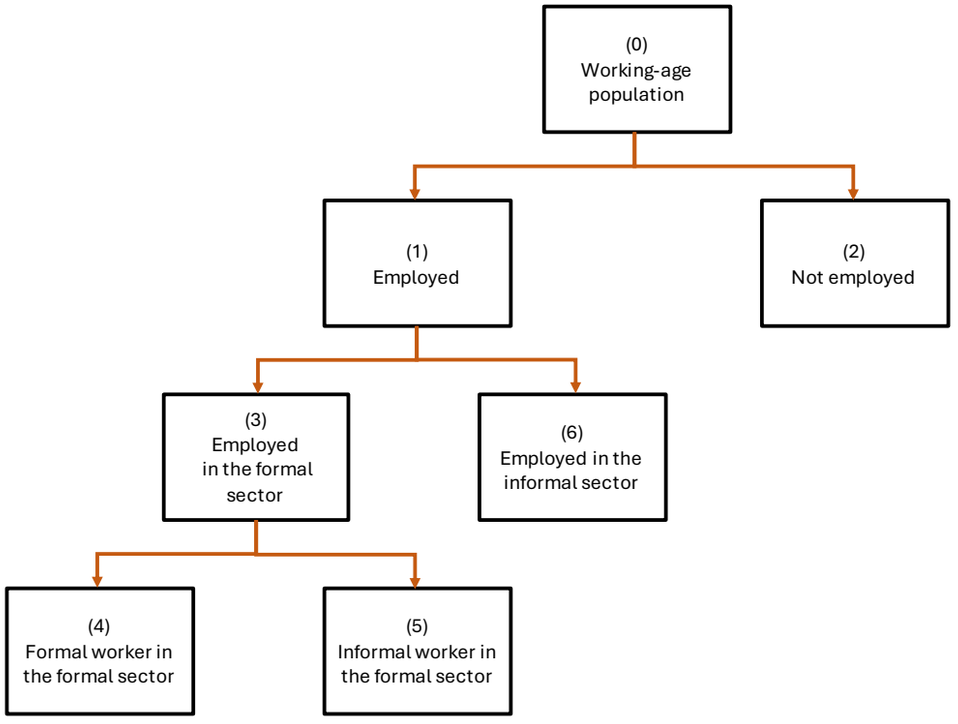


Table A.3: Selected indicators from the World Bank Enterprise Surveys, Peru (2023)

Indicator	Peru (2023)	LAC	All Economies
Inspections			
Percent of firms visited or required to meet with tax officials	22.1	27.7	39.1
If there were visits, average number of visits or required meetings with tax officials	3.2	2.7	2.2
Corruption			
Percent of firms expected to give gifts in meetings with tax officials	8.1	4.8	8.1
Percent of firms expected to give gifts to public officials "to get things done"	6.1	8.8	12
Bribery incidence (percent of firms experiencing at least one bribe payment request)	7.6	8.8	11.1
Average bribery depth (% of public transactions where a gift or informal payment was requested)	4.5	6.2	8.8

Source: World Bank Enterprise Surveys (https://www.enterprisesurveys.org, accessed on February 5th, 2026). LAC = Latin America & Caribbean.

B. Two-Way Fixed Effects (TWFE) model

This appendix reports results from a standard Two-Way Fixed Effects (TWFE) model as a benchmark for the main estimates presented in Section 5. As discussed in Section 4, TWFE estimators can produce biased results in staggered adoption settings when treatment effects are heterogeneous across cohorts or over time, which motivates our reliance on the ETWFE estimator as the preferred specification. The results reported here should therefore be interpreted with that caveat in mind.

The TWFE specification is the following:

$$y_{i,d,r,t} = \beta D_{r,t} + \lambda_d + \lambda_t + \theta \cdot X_{i,d,r,t} + \epsilon_{i,d,r,t} \tag{2}$$

where $y_{i,d,,r,t}$ represents the outcome of interest for individual i, district d, region r, at time t. Our main treatment variable is $D_{r,t}$, a binary variable equal to one if a SUNAFIL agency is operational in region r and year t. We include year fixed effects (λ_t) and district fixed effects (λ_d).

We also include time-varying control variables $X_{i,d,r,t}$. Specifically, we control for each individual's age and age squared, educational level dummies, and an urban/rural indicator. To account for broader differences in institutions, governance, or level of development, we also control for the percentage of government expenditure executed per year, regional GDP, as well as the participation of agriculture, manufacturing, and services in regional GDP. We further control for the percentage of paved roads in the region, given that inspectors need to travel from the inspection agency to the firm's location and inspections are more likely in regions with a better road network.

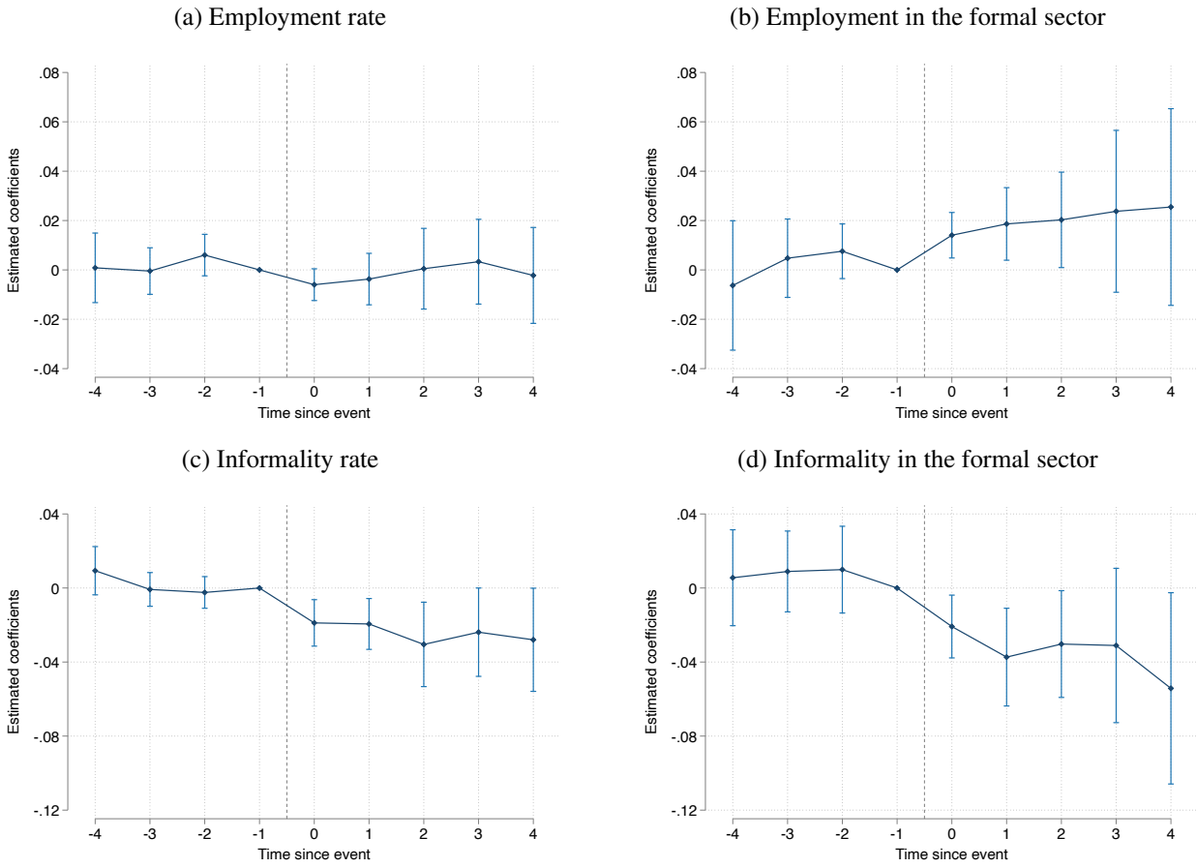
Identification in this specification relies on the parallel trends assumption: in the absence of treatment, regions with and without a SUNAFIL office would have followed similar trajectories in labor market outcomes. To assess this assumption, we estimate a standard event-study specification and test for the presence of differential pre-trends.[28] Figure B.1 presents the results. The estimated coefficients for all pre-event periods are small and statistically indistinguishable from zero across all outcomes, providing support for the parallel trends assumption in this context.

[28] For this, we take the event date as the year of each regional SUNAFIL office opening, and estimate the following standard event study specification:

$$y_{i,d,r,t} = \sum_{j=-5}^{5} \gamma_j \mathbb{1}\{Years\_since\_agency_{r,t} = j\} + \lambda_d + \lambda_t + \epsilon_{i,d,r,t}$$

where the indicator function $\mathbb{1}$ equals one if an inspection agency opened j periods away in region r and period t (j equals zero for the first year under treatment, i.e., the year that SUNAFIL opened in that region, and takes negative –positive– values for years before –after– the event). The λ_d and λ_t represent district and year fixed effects as before. The main coefficients of interest are the γ_j's that measure the effect of an increase in labor law enforcement for each of the j years before and after the event of the SUNAFIL agency opening. Therefore, γ_j's are typically interpreted as measuring the effect of being treated at different exposure lengths.

Figure B.1: Impact of the opening of a SUNAFIL office on employment and informality



Note: The dots and bars represent the coefficients and 95 percent confidence intervals based on standard errors clustered at the regional level. The full regression output is in the Appendix in Table A.1.

Table B.1 presents the estimates of β across three specifications. Column 1 includes only district and year fixed effects. Column 2 adds individual and household-level controls. Column 3 further incorporates regional characteristics, such as public investment execution, road infrastructure, regional GDP, and sectoral composition.

Table B.1: TWFE estimates of the impact of the opening of a SUNAFIL office on employment and informality

	(1)	(2)	(3)
Employment	-0.004	-0.005	-0.004
	(0.003)	(0.003)	(0.003)
Employment, formal sector	0.008	0.006	0.005
	(0.005)	(0.004)	(0.004)
Informality	-0.016**	-0.014**	-0.014**
	(0.007)	(0.007)	(0.006)
Informality, formal sector	-0.018**	-0.015*	-0.018**
	(0.008)	(0.008)	(0.008)
Control variables:			
Individual and household-level controls	No	Yes	Yes
Public investment execution	No	No	Yes
Percentage of paved roads	No	No	Yes
Regional GDP	No	No	Yes
% of regional GDP in agriculture, manufacture, and service	No	No	Yes

Note: All regressions also include district and year fixed effects. The estimations are population-weighted, and the standard errors are clustered at the regional level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The TWFE estimates suggest that the opening a SUNAFIL regional office did not affect the number of jobs, total employment, or employment in the formal sector, as the coefficients are small and never statistically different from zero. They suggest that it led to a statistically significant reduction in labor informality. Specifically, the overall informality rate declines by 1.4 percentage points, a result that is robust across the three specifications. Yet, the effect represents a modest 2.5 percent reduction relative to the national average of 73 percent between 2014 and 2019. The informality rate within the formal sector falls by a somewhat larger margin, around 1.8 percentage points.

As discussed in Section 5, however, these reductions in informality do not survive when the ETWFE estimator is used instead, consistent with the well-documented limitations of TWFE in staggered adoption settings.