

Labor-Law Compliance in an Informal Labor Market

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Abstract

I study the effects of a reform of the system of labor inspection in Colombia, which was requested by the United States as part of the negotiations for a bilateral trade agreement. An additional labor inspector increases labor-law compliance in the formal sector (i.e., among registered firms), while there is no effect in the informal sector. There is also a decrease in the probability of working informally in the formal sector, while overall employment remains unchanged. Complementary evidence from an enterprise survey shows that firms in the formal sector compensate for part of the increase in labor costs by reducing payroll contributions.

JEL classification H10, J38, J58, K31

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1. Introduction

The majority of employment in low- and middle-income economies is in the informal sector of the economy, where workers are not covered by the labor law and do not benefit from the provision of mandated benefits (Bhorat, Kanbur, and Stanwix 2019; ILO 2018). Two interpretations of informality used to prevail in the literature (for a review, see La Porta and Shleifer (2014)). According to the first view, the presence of a large informal sector is the result of a survival strategy by unproductive enterprises that would not be able to operate if they had to face all the costs imposed by the legislation. Alternatively, self-selection into the informal sector reflects comparative advantages as in a Roy (1951) model, so that in equilibrium wages will reflect differences in amenities and workers will be indifferent between sectors. Despite presenting different interpretations, both views imply that informality increases the number of available jobs (Dix-Carneiro et al. 2026). In line with these models, empirical studies have shown that reducing the size of the informal sector through stricter labor-law enforcement can lead to a reduction in employment due to an increase in labor costs (Almeida and Carneiro 2009, 2012).

However, these interpretations of informality are not able to fully explain some of the patterns observed in the data. Studies have revealed the presence of a substantial overlap in the productivity distribution of formal and informal firms, frequent transitions of workers between the formal and the informal sectors, as well as the presence of informal employment in both formal and informal firms (Bosch and Maloney 2010; Botelho and Ponczek 2011; Porta and Shleifer 2008; La Porta and Shleifer 2014). New models have therefore been proposed, which explain informality as the result of the presence of search frictions in the labor market (Meghir, Narita, and Robin 2015) or the consequence of choices made by heterogeneous firms that can operate fully or partially informally (Ulyssea 2018). In these models, the majority of enterprises that operate in the informal sector do so by choice rather than need. This leads to the emergence of an implicit subsidy towards smaller and less productive informal firms that evade taxation. Reducing the size of the informal sector by strengthening public enforcement can thus increase output and employment by encouraging the reallocation of workers to higher productivity jobs.

I contribute to this debate by studying the effects of a reform of the system of labor inspection in Colombia, a country that has undertaken extensive efforts over the last decades to reaffirm the role of the state amid the presence of both organized crime and paramilitary forces. Despite recent progress, non-compliance with the legislation remains high in the country and the levels of interpersonal and institutional trust are among the lowest in the region (OECD 2017).¹ In the labor market, almost one out of two workers in non-agriculture employment has no access to social security and more than one out of three workers earns less than the minimum wage (ILO 2019; Marinakis 2014). There are also systematic reports of violations of collective bargaining rights and violence against trade union representatives (ITUC 2010). In an attempt to strengthen labor-law compliance, the US administration requested the Colombian government to more than double the number of labor inspectors operating in the country between 2011 and 2014.² The request was advanced as a requirement for the approval by the United States (US) Congress of the “US–Colombia Trade Promotion Agreement”.

As a result of the policy change, the number of inspectors in Colombia increased from two to four per 100,000 individuals between 2011 and 2014. The assignment rule used to distribute the new inspectors generated exogenous variations in the strength of law enforcement across departments and years, which I exploit for identification. Specifically, the Colombian Ministry of Labor distributed the vacancies across departments following an allocation rule that was elaborated at the central level, changed in each hiring round, and was meant to capture the risk of non-compliance with the labor legislation (i.e., assigning more inspectors to higher-risk areas). I exploit the fact that this rule generated exogenous discontinuities in the number of inspectors working in departments characterized by an otherwise comparable risk of non-compliance with the labor legislation. In the main analysis, I use administrative data from the Ministry of Labor with information on the allocation rule and the number of inspectors across departments and years, matched with individual-level data from the household survey.

I find that an additional labor inspector in a given department raises workers’ probability to receive a number of job-related benefits (i.e., social-security coverage for pension) and to have a written employment contract, while it reduces the probability of working an excessive number of hours. Effects are similar in size across different indicators of law compliance and they correspond to an increase of around 2 percent. Point estimates are also similar in magnitude for women and men, high- and low-educated individuals, as well as for individuals in urban and rural areas. However, the increase in law compliance and benefit provision materializes only in the formal sector of the economy (i.e., among registered enterprises). This asymmetric effect between the formal and the informal sector can be explained by the fact that labor-law compliance is very low in the informal economy to begin with, but also with the fact that inspectors do not target informal firms (e.g., because they are more difficult to find).

¹ Only 4.1 percent of survey respondents in Colombia agree that “most people can be trusted,” compared to 7.1 percent in Brazil, 12.4 in Chile and Mexico, and 19.2 in Argentina (World Value Survey 2014).

² Labor inspectors are public officials in charge of monitoring compliance with the labor legislation and imposing sanctions to firms in case of violations. Inspectors can conduct preventive inspections based on available information (e.g., registry data on existing enterprises) or visit firms because of complaints raised by workers (ILO 2017b; OECD 2022). Inspectors are responsible for guaranteeing labor-law compliance in both formal and informal enterprises, and in Colombia they also have the task of promoting the formalization of informal firms.

All these changes can be costly for employers. However, I do not find any negative effect on overall employment levels. Nevertheless, the distribution of employment changes. In particular, informal employment in the formal economy decreases. I call this the “intensive” margin of formalization (Ulyssea 2018), and interpret it as the formalization effect that labor inspectors exercise when visiting formal firms (i.e., they force employers to register workers that were kept off the books). Instead, I do not find any effect on the probability of working informally in the informal sector (i.e., the so-called “extensive” margin of formalization). This suggests that inspectors operate by targeting firms in the formal sector, leaving the informal sector mostly unchecked. This result is also in line with the difference in results for the measures of labor-law compliance between the formal and the informal sectors, as discussed above.

Finally, I find that the number of hours worked decreases while wages slightly increase for those who remain in employment. These results can be interpreted as resulting from higher compliance with the labor legislation, as firms need to reduce the number of hours worked to avoid that workers are employed for more than the legally allowed number of hours and, at the same time, they need to increase monthly wages to respect the legislation over minimum wages. In order to understand how formal firms absorb these costs, I turn to a survey of formal enterprises in the manufacturing sector. To start with, I confirm that wages for permanent staff increase in formal firms. However, the evidence also shows that firms compensate around 40 percent of this increase via reduced payroll contributions. This suggests that formal firms try to absorb part of the increased costs via tax avoidance or outright evasion.

The paper contributes to a debate at the intersection between two strands of the literature: empirical studies on formalization policies and theoretical contributions on the nature of the formal and informal sectors. Starting with the empirical studies on formalization policies, previous contributions examined the effects of an increase in labor inspection on either indicators of labor-law compliance (Bhorat, Kanbur, and Mayet 2012; Gindling, Mossaad, and Trejos 2015; Ronconi 2010) or the equilibrium in the formal and informal labor markets (Almeida and Carneiro 2012; de Andrade, Bruhn, and McKenzie 2016).³ This paper brings together these two dimensions, which is important given that (a) changes in compliance with the labor legislation can affect employment levels, and (b) this effect can materialize differently in the formal and informal sectors. The only other contribution that looks jointly at measures of law compliance and the equilibrium in the labor market is Samaniego de la Parra and Fernández Bujanda (2024), which examines the effects of randomly allocating formal firms to inspections. However, they study the effects of increasing the risk of inspections for individual firms, while I exploit a nationwide policy change that allows me to look at the effects of increasing inspections for all firms. Additionally, they focus on the effects of increasing enforcement only on formal firms, while I can also investigate the effects for firms operating in the informal sector, which represent the majority of firms in these countries.

Turning to the debate on the nature of the formal and informal sectors, recent work has investigated whether formal firms enjoy monopsony power (Amodio, Medina, and Morlacco 2022). The presence of a large informal sector has instead been interpreted as the result of a survival strategy, or, alternatively, the deliberate decisions of enterprises (for a review, see La Porta and Shleifer (2014)). I contribute to this debate by providing novel findings from a natural experiment. My results suggest that different mechanisms might be at play. To start with, I find that overall employment does not decrease when labor-law enforcement increases. At the same time, I also find that formal employment increases, despite the fact that the cost of hiring a worker formally has increased due to higher compliance with mandated benefits. These findings are compatible with the fact that some formal firms enjoy monopsony power when they operate in largely informal labor markets and do not reduce employment despite an increase in labor costs.

³ The literature on the effects of labor inspection is part of a broader literature on the formalization effects of public policies, including tax reductions (Fajnzylber, Maloney, and Montes-Rojas 2011; Monteiro and Assuncao 2012) and the simplification of bureaucratic procedures (Bruhn 2011; de Mel, McKenzie, and Woodruff 2013; Kaplan, Piedra, and Seira 2011; Rocha, Ulyssea, and Rachter 2018).

2. Policy and institutional setting

The US and Colombia signed in November 2006 a comprehensive trade agreement aimed at reducing tariff and non-tariff barriers between the two countries (the “United States–Colombia Trade Promotion Agreement,” CTPA). The ratification process required the approval of the trade agreement by the parliaments of both countries. The Colombian Congress approved the bill in June 2007 with a large majority (85 yes and 10 no in the House Floor and 55 yes and 3 no in the Senate Floor).⁴ In April 2007, the agreement was also sent for approval to the US Congress. Despite the US administration support for the CTPA, the debate never led to a vote amid concerns raised by the Congress over violations of labor rights in Colombia. There is evidence that partisan considerations played a role in shaping the debate over the CTPA. In particular, from January 2007 the US had a divided government, with a Republican administration and a Democratic majority in the Congress. A rule change to delay action on the agreement was approved by the Congress, after which the US administration committed to include “worker protections in several pending trade accords” (Weisman 2007).⁵ As a result, the CTPA did not pass by the end of the legislative session in 2008 (OAS 2019).

The newly elected US Democratic administration immediately specified its opposition to the approval of the CTPA in its current form and requested additional guarantees with respect to labor-law compliance.⁶ This request can be connected to the increasing trend of including labor provisions in trade agreements (ILO 2016) and it was specifically motivated by concerns over the low coverage of the labor inspectorate in Colombia from an international perspective (OECD 2016). This led to the definition in April 2011 of a set of measures aimed at reinforcing compliance with the labor legislation in the country (the “Colombian Action Plan Related to Labor Rights,” or Action Plan), to be implemented by the Colombian government in collaboration with the International Labour Organization (ILO) (OAS 2019). After the Action Plan was agreed, the US Congress approved the CTPA in October 2011 and the trade agreement came into force in May 2012.

The Action Plan established that the Colombian Ministry of Labor would have hired 480 new inspectors within four years, in a move that would have more than doubled the number of inspectors operating in the country (i.e., from 424 at the end of 2010 to 904 in 2014).⁷ Vacancies were issued by ministerial decree over four hiring rounds (i.e., in each year between 2011 and 2014), with 100 vacancies posted in each of the first two hiring rounds and 140 in each of the last two rounds. The government also committed to fill the positions that were already open but still vacant (i.e., in 2010, only 353 out of 424 positions were actually filled). As a result of these changes, the number of inspectors increased from 353 in 2010 to 771 in 2015.⁸ This was well below the hiring target of 904 inspectors (see below on the implementation delays), but it nevertheless constituted an important improvement (OECD 2016).

The legislative decrees implementing the Action Plan established that the newly hired inspectors should have been allocated across departments “taking into account the structure, plans, programs and necessities of the Ministry” (Government of Colombia 2011, 2012, 2013). In practice, vacancies were distributed across departments according to an allocation rule that changed in each hiring round and was constructed as the weighted average of different indicators identified by the Ministry of Labor to capture the risk of non-compliance with the legislation.

I present here the details for the first hiring round in 2011, while information for the other years is available in supplementary online appendix S1. During the first hiring round, the allocation rule resulted from 18 different indicators belonging to 5 different thematic clusters (fig. 1). First, a set of indicators captured

⁴ The strategic importance of the CTPA for Colombia can hardly be overstated: the US was the country’s main trade partner, accounting for 35 percent of the country’s balance of trade in 2007 (OECD 2019).

⁵ This was meant to apply to trade agreements with Colombia, Panama, Peru, and South Korea.

⁶ In June 2009, the US president specified there was no “strict timetable” to the agreement amid controversies over the violation of labor rights in Colombia. Additionally, the major US trade union federation opposed the CTPA. This led to delays in the beginning of the new bargaining round.

⁷ The Action Plan also established the creation of the Ministry of Labor (previously a division of the Ministry of Social Security) and reinforced the legislation over anti-union behavior and the misuse of cooperatives. All these policy changes happened at the national level, with no variation across departments.

⁸ The increase was not constant over time, but it was slower in the years immediately after the approval of the Action Plan. In particular, the number of inspectors operating in the country increased from 353 in 2010 to 407 in 2011, 451 in 2012, 525 in 2013, 637 in 2014, and 771 in 2015.

Figure 1 Criteria for determining the allocation rule in the 2011 hiring round

DEFINICIÓN CRITERIOS CONSIDERADOS (Abril de 2011)		
CRITERIO	DETALLE	FUENTE
ACTUACIONES ADMINISTRATIVAS	Consultas	INFORMACIÓN ESTADÍSTICA DIRECCIONES TERRITORIALES 2002 - 2010
	Visitas de Inspección	
	Investigaciones Administrativas	
SECTORES ECONÓMICOS	Flores	ESTUDIO SECTOR FLORICULTOR DE COLOMBIA (1995 2005) - SUPERINTENDENCIA DE SOCIEDADES
	Caña (Azúcar, Panela, Miel)	MINISTERIO DE COMERCIO IND Y TURISMO PERFILES ECONÓMICOS POR DEPARTAMENTO -OFICINA DE ESTUDIOS ECONOMICOS -MINISTERIO DE AGRICULTURA Y DESARROLLO RURAL ANUARIO ESTADÍSTICO 2008
	Palma de Aceite	
	Puertos	
COOPERATIVAS Y PRECOOPERATIVAS	Autorizadas	INFORMACIÓN ESTADÍSTICA DIRECCIONES TERRITORIALES 2002 - 2010
	Visitadas	
	Investigadas	
EMPRESAS DE SERVICIOS TEMPORALES	Autorizadas	INFORMACIÓN ESTADÍSTICA DIRECCIONES TERRITORIALES 2002 - 2010
	Visitadas	
	Investigadas	
	Multadas	
	Suspendidas	
	Canceladas	
INSPECTORES ACTIVOS	Vigentes	TALENTO HUMANO MINISTERIO DE LA PROTECCIÓN SOCIAL
	Número de Inspectores de trabajo por Territorial	

Source: Government of Colombia (2011).

Note: The figure reports the indicators that entered the allocation rule in the first hiring round of the Action Plan. The first column reports the thematic cluster, the second one details the specific indicators, and the third one specifies the sources of the information used to compute the indicators for each department.

administrative actions (i.e., consultations, visits, and investigations) conducted by labor inspectors in a specific department over previous years (i.e., from 2002 to 2010). A second set of indicators reflected the importance of specific economic sectors (i.e., flowers, palm oil, and ports) that were defined as priority areas given the high risk of non-compliance. The third and fourth sets of indicators reflected the number of cooperatives and temporary agency firms in a given department between 2002 and 2010. Finally, vacancies were allocated according to the number of inspectors previously operating in a department.

A department received a final score as a function of these variables. More specifically, the score obtained by department s in year t (Score_{st} in the equation below) was equal to the sum of the values obtained in each of the different n indicators entering the allocation rule (from Ind_{1st} to Ind_{nst}), weighted to reflect the relative importance of each indicator (with weights from w_{1t} to w_{nt} , constant across departments but changing over years) and divided by the scores obtained over these indicators by all departments in that same hiring round. The resulting value represented a percentage, which was multiplied by the number of vacancies being issued at the national level in a given hiring round (Tot_t , equal to 100 or 140 depending on the hiring round) to determine the number of vacancies to be allocated to a given department in that specific hiring round. From now on, this will be referred to as the final or normalized score and it can be decomposed into an integer part (Integ_{st}) and a decimal component (Dec_{st}):

$$\text{Score}_{st} = \left\{ \frac{w_{1t} \text{Ind}_{1st} + w_{2t} \text{Ind}_{2st} + \dots + w_{nt} \text{Ind}_{nst}}{\sum_{s=1}^S [w_{1t} \text{Ind}_{1st} + w_{2t} \text{Ind}_{2st} + \dots + w_{nt} \text{Ind}_{nst}]} \right\} * \text{Tot}_t$$

$$= \text{Integ}_{st} + \text{Dec}_{st}.$$

A department was then assigned a target number of inspectors to be hired as part of each hiring round (Target_{st}) as a function of this final score, with a discontinuity within each integer. In particular, the allocation of new inspectors was equal to the integer part of the final score if the decimal component was strictly less than .5, while an additional inspector was assigned in the case that the decimal part of the final score was greater than or equal to .5:

$$\text{Target}_{st} = \begin{cases} \text{Integ}_{st} & \text{if } \text{Dec}_{st} < 0.5, \\ \text{Integ}_{st} + 1 & \text{if } \text{Dec}_{st} \geq 0.5. \end{cases}$$

The rest of the hiring process took place directly at the department level. This means that the Ministry of Labor was only responsible for setting the hiring target and distributing the vacancies across departments following the allocation rule. Labor offices in the different departments were then in charge of completing the hiring process and training the newly hired inspectors before they could take office. The

Table 1. Determinants of the probability of meeting the hiring target in a given hiring round

	(1)	(2)	(3)	(4)
Linear control for the integer	−0.0175*** (0.005)	–	−0.0177*** (0.00497)	–
Above the .5 cutoff	–	0.0775 (0.0863)	0.0815 (0.0853)	−0.00135 (0.121)
Integer dummies	No	No	No	Yes
N	92	92	92	92

Source: Author's elaboration based on administrative data.

Note: The table reports coefficient estimates and standard errors of different regressions run at the department and year level. The dependent variable is always the dummy for having met the hiring target in a given hiring round (i.e., equal to 1 if the target is met). Column (1) controls linearly for the integer part of the allocation rule, column (2) controls for the dummy being above the .5 cutoff within a given integer, column (3) includes both controls, column (4) controls for the dummy being above the cutoff and a set of dummies for the integer part of the final score (not reported in the table). * <10 percent, ** <5 percent, *** <1 percent.

appointment of new inspectors should have been made in the same year in which the hiring round took place. However, the hiring process was subject to sizeable delays. In particular, hiring targets were met in only 21.74 percent of the cases (i.e., counting each department and year as a separate observation) and only 2 departments out of 23 in the analysis had met their final hiring target as set by the Action Plan in 2014. While it is difficult to trace the exact causes of these delays, they have been related to shortages of human resource capacities at the local level, as well as the limited attractiveness of the compensation packages for the positions (OECD 2016). These problems pre-dated the approval of the Action Plan (i.e., 71 out of 424 vacancies were already unfilled in 2010) and were only accentuated by the introduction of new and highly demanding hiring targets.

In line with this interpretation, there is a positive correlation between hiring targets and implementation delays. In particular, a one unit increase in the final score reduces the probability of meeting the hiring target by almost 2 percentage points, from an average value of the dependent variable equal to 21.7 percent (column 1, table 1). At the same time, implementation delays are uncorrelated with the .5 cutoff within a given integer of the final score. The dummy for being above the .5 cutoff is uncorrelated with the probability of meeting the hiring target, both when included alone (column 2) or together with the linear control for the integer part of the final score (column 3). Point estimates become even smaller and close to zero once I control for the integer part of the final score with a set of dummies (column 4).

I can instead rule out alternative explanations behind the cross-department differences in the probability of meeting the hiring targets. In particular, these cannot be related to differences in the compensation packages or other working conditions across departments as these are uniformly determined by the central government (see below for details). Similarly, differences in the department's capacity of meeting the hiring target are not driven by differences in the profiles of labor inspectors that are sought by the different departments. Specifically, at the country level, the relative majority of vacancies were for labor inspectors with a legal background and this seems to apply rather uniformly across departments.⁹

Labor inspectors are officials of the Ministry of Labor in charge of guaranteeing the respect of the labor legislation and collective bargaining rights. They are organized around territorial directorates, corresponding to the Colombian departments. Labor inspectors need to have an undergraduate degree in either law, medicine, engineering, business administration, or economics and a postgraduate degree relevant to the specific area of work. In practice, around two-thirds of the positions are for inspectors with a legal background. Additionally, at least seven months of relevant work experience are required for entry-level positions. Working conditions are not very attractive. First, wages are low compared to those of other public officials in the country (e.g., around half of the salary of a judge) and there is no ex-

⁹ For instance, in the 2013–2014 hiring rounds, 45 percent of vacancies at the country level were for inspectors with a legal background. Among the eight departments that hired at least 10 inspectors over these two hiring rounds (i.e., for which we have enough variation in the profiles advertised), in seven cases the share of vacancies for positions with a legal background varied between 38 percent and 43 percent (while in one department this was equal to 27 percent).

tra compensation for inspectors operating in areas characterized by high violence rates.¹⁰ Additionally, only a small share of inspectors are hired as career civil servants (15 percent of those operating in 2014).¹¹ Finally, limited public resources affect the way in which inspectors operate in Colombia. For instance, inspectors do not have service vehicles but need to seek assistance by other government agencies or workers' organizations to visit enterprises (ILO 2014; OECD 2016).

In their operations, labor inspectors follow ILO guidelines as summarized in a manual of the Ministry of Labor (Government of Colombia 2016). They have the authority to verify compliance with the labor code in general. This includes areas such as minimum wage, social-security contributions, working hours, collective bargaining rights, and occupational safety and health. Labor inspectors are responsible for enforcing compliance with the labor legislation in both the formal and the informal sector of the economy. This is because all workers, irrespective of the nature of their employment contract, are entitled to the same rights and working conditions according to the legislation (Government of Colombia 2016; ILO 2020b). More specifically, labor formalization is one of the priority areas of the system of labor inspection in Colombia. In particular, labor inspectors have detailed guidelines on how to promote labor-law compliance in the informal sector of the economy, using both conciliatory and sanctionary methods. The actions undertaken to promote the formalization of informal enterprises follow a standard route, whereby inspectors (a) target informal enterprises based on a series of considerations (see below), (b) visit the establishments and encourage employers to register their business, and (c) follow up at regular intervals to monitor progress (ILO 2020a).

Inspectors can start an inquiry independently or by responding to complaints by workers (i.e., preventive or reactive inspections, respectively). Workers can file complaints (anonymously or otherwise) online, by telephone, or directly at the labor office. There is no available breakdown on the number of preventive and reactive inspections during the period of the analysis, as this type of information was originally not collected by the Ministry of Labor (OECD 2016). However, data for more recent years indicate that around 80 percent of inspections were of a preventive nature (OECD 2022). This is in line with the experience documented in most countries around the world, as well as with ILO recommendations on labor inspection (ILO 2017b, 2022), which were also adopted in Colombia with the signing of the Action Plan.

Preventive inspections represent the norm in particular in the informal sector of the economy, where workers are less likely to raise complaints autonomously, due to either lack of knowledge of the system of labor inspection or fear of being dismissed or punished by the employer following the intervention of the inspectors (ILO 2020a). To circumvent this problem, labor inspectors in Colombia have been provided with specific guidelines on how to target informal firms, drawing on collaboration with municipalities (e.g., to understand where informal firms are located), as well as based on the understanding of which economic sectors and types of economic units are associated with a higher risk of informality.

Once inspectors decide to start an inquiry, they open an investigation to verify the presence of valid grounds. Based on this preliminary assessment, the case can either be archived or a formal investigation will start. At the end of the investigation, inspectors can (a) conduct a preventive and informative function (e.g., inform the employer about a breach in the legislation), (b) act as mediators between the employer and the worker (e.g., help to define an agreement in the case of collective dismissals), (c) implement initiatives to remove obstacles to the respect of the labor code (e.g., favor the development of a trade union), or (d) impose fines and sanctions. Sanctions can be of a financial nature (from 1 to 5,000 times the minimum wage) and/or involve the temporary closure of the enterprise (from 3 to 30 days), or its immediate shutdown in the case of health or security risks. This wide set of interventions is in line with the guidelines for labor inspection promoted by the ILO, which aim to move away from pure sanctioning strategies towards a strategic compliance model (ILO 2017a).

¹⁰ Acknowledging that low wages were preventing the hiring of highly qualified candidates, salaries for inspectors were raised in 2014 to reach between four and six times the minimum wage (OECD 2016).

¹¹ Recognizing that this might hinder their impartiality and independence, in 2008 the Constitutional Court ruled that inspectors can be fired only for disciplinary reasons.

3. Data, definitions, and descriptive statistics

The main analysis draws from two different data sources. The population of interest comes from repeated cross-sections of the Colombian Integrated Household Survey (GEIH), conducted by the National Administrative Department of Statistics (DANE). The GEIH has a two-stage stratified sample and every year it interviews around 250,000 households. The sample used in this study is representative of the entire working-age population between 2011 and 2014, as this is the time period of theoretical implementation of the Action Plan (i.e., years for which hiring targets were set). Using information on the department of residence and the year of the interview, I match the GEIH with administrative data provided by the Colombian Ministry of Labor.¹² For each department and year between 2011 and 2014, this includes data on the allocation rule (both the final score and the different subindicators), from which I obtain information on the number of inspectors that should have been hired in a given department and year. I also have data on the number of inspectors that were actually working in each department and year between 2008 and 2015 (i.e., before and after the implementation of the Action Plan).

The aim of the analysis is to examine the effects of strengthening labor-law enforcement on indicators of law compliance and to observe the resulting implications in terms of labor-market adjustment in the formal and informal sectors. This is important given that changes in compliance with the labor legislation can affect employment levels, and this effect might materialize differently in the formal and informal sectors of the economy. Specifically, the analysis focuses on two main clusters of outcomes of interest.

The first cluster includes measures of labor-law compliance, in order to examine whether labor inspection has any effect on its main stated objective. Outcomes of interest include indicators for having a written employment contract, being entitled to a series of mandated benefits (e.g., severance payments, paid annual leave), working an excessive number of hours, and being covered by social security for pension and health.¹³ This allows examining compliance along the extensive margin (i.e., whether the individual has an employment contract), as well as the intensive margin (i.e., if mandated benefits are also provided).¹⁴

In Colombia, both written and oral contracts are allowed by the legislation. However, written contracts are the only ones that can be enforced, so labor inspectors will clearly promote their use. Individuals have the right to receive severance payments in case of dismissal without cause, at a rate of 30 days of salary for the first year of service and 20 days for each consecutive year. With respect to paid holidays, individuals have the right to 15 working days per year. Working an excessive number of hours is defined by the ILO as working for more than 48 hours per week. When defining social-security coverage for health and pension, I take into account whether employers contribute (partially or fully) to the social-security funds of their workers.¹⁵

The second cluster of outcome variables is meant to study changes to the equilibrium in the labor market arising from the increase in enforcement levels. If stricter enforcement changes working conditions, this might affect overall labor demand and supply as well as the distribution of employment between the formal and informal sectors (Almeida and Carneiro 2012). For this reason, I look at the effects of being in employment. Additionally and following ILO definitions, the employment relationship can be of formal or informal nature (i.e., depending on whether the worker is registered for social se-

¹² Some remarks concern the matching process between the administrative and survey data. The Ministry of Labor follows the traditional division of the country into 32 departments, while the GEIH covers only the 23 main departments in Colombia. Since it is impossible to connect observations in the GEIH to the 32 departments, I conduct the analysis for the 23 departments for which I have both survey and administrative data. In practice, the problem is of limited empirical importance given the small size of the population living in the excluded departments (i.e., jointly representing less than 5 percent of the Colombian population).

¹³ Unfortunately, I cannot directly measure effects on compliance with the minimum wage as I observe wages at the monthly level, but hours worked at the weekly level.

¹⁴ These are all self-reported measures of labor-law compliance and there is a risk of misreporting or underreporting leading to measurement error. Following Ronconi (2010), these indicators are better obtained from household surveys (compared to enterprise surveys, as employers might have an interest in underreporting non-compliance). Additionally, DANE has strict confidentiality clauses and the information provided in the survey cannot be used for auditing purposes. These features, combined with the high rates of non-compliance with the labor legislation reported in the data, lead me to have some confidence in the quality of the information.

¹⁵ Employers should pay 12 percent of the worker's monthly salary in pension contributions and 8.5 percent in health contributions. Workers should contribute with 4 percent of their salary in each of the two accounts.

Table 2. Descriptive statistics at the department level for the policy variables

	Mean	S.D.		Mean	S.D.
Labor inspectors			Action Plan variables		
Number of inspectors (2011–2014)	20.630	16.166	Final score	3.899	4.874
Inspectors in 2010	14.783	11.852	Integer of the final score	3.402	4.883
Inspectors in 2011	16.739	14.012	Decimal of the final score	0.497	0.274
Inspectors in 2012	18.391	12.804	Allocation of inspectors	3.913	4.921
Inspectors in 2013	21.304	14.196	Actual change in inspectors	2.826	5.584
Inspectors in 2014	26.087	20.799	Above .5 cutoff	0.511	0.503
Inspectors in 2015	33.087	30.763	Number of times above cutoff	2.043	1.047
<i>N</i>	92			92	

Source: Author's elaboration based on administrative data.

Note: The table reports means and standard deviations for selected policy variables computed yearly at the department level between 2011 and 2014, unless otherwise specified.

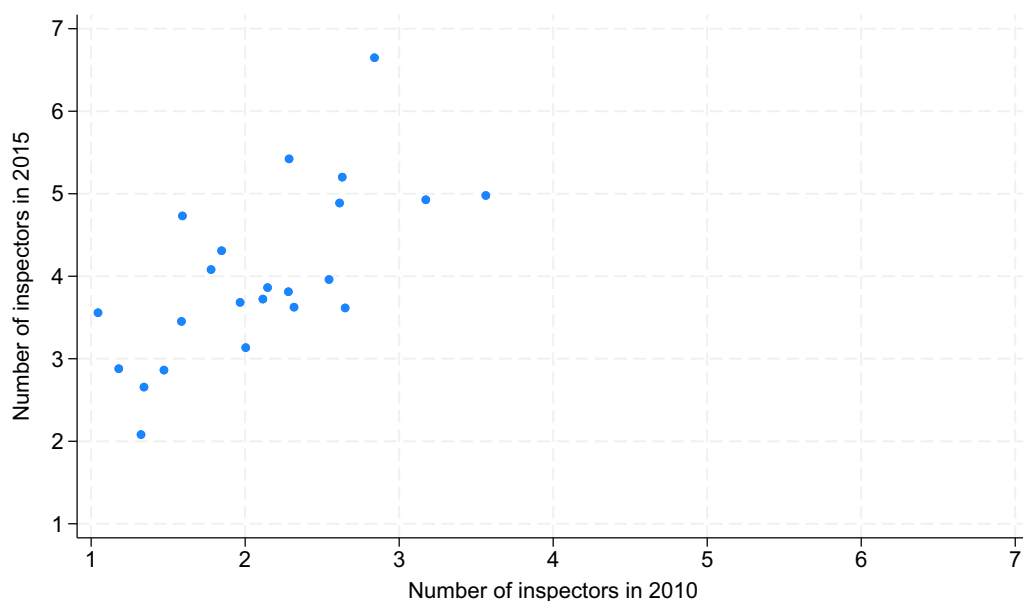
curity) and the enterprise can operate in the formal or informal sector (i.e., depending on whether the firm is registered with the relevant authorities). The intersection between these two variables gives rise to four possible employment statuses that I examine (Ulyssea 2018): formal worker in formal enterprise, formal worker in informal enterprise, informal worker in formal enterprise, and informal worker in informal enterprise.¹⁶ I will also look at the effects on the number of hours worked and on labor income. The definition of outcomes of interest is available in supplementary online appendix table S2.1.

In the main analysis, treatment varies at the department and year level (leading to the definition of 92 units of treatment variation, corresponding to 23 departments over four years), while the unit of observation corresponds to the individual living in a given department and year.

Table 2 presents data on labor inspectors by department before and after the policy change. Between 2011 and 2014, the 23 departments with available information had on average 20.6 inspectors (median of 15.5). This increased substantially over time as a result of the Action Plan (i.e., from 14.8 inspectors in 2010 to 33.1 inspectors in 2015). The final score was on average equal to 3.9 across the four different hiring rounds (median value of 2.1), leading to the assignment of 3.9 inspectors per year in a given department. In practice, the yearly actual change in the number of inspectors was equal to 2.8 on average due to policy implementation delays. As a result of differences in both hiring targets and implementation delays, the overall increase in the number of inspectors between 2010 and 2015 varied greatly across departments. While at the national level the number of inspectors per 100,000 individuals roughly doubled during this time, the increase ranged from 40 to 240 percent across departments (fig. 2).

Table 3 presents individual-level descriptive statistics for the main sample, corresponding to the working-age population interviewed in the GEIH between 2011 and 2014 in the 23 departments included in the analysis. Only a minority of employed individuals works in the formal sector of the economy (i.e., corresponding to 23.3 percent of the entire sample, or less than 40 percent of the employed population) and is formally employed (i.e., only 20.9 percent of the sample, or around one-third of the employed population). The vast majority of employment in the formal economy is constituted by formal employment (i.e., registered workers in registered enterprises), while only a small share of workers are employed informally by registered enterprises (i.e., 2.5 percent of the overall sample, or around 10 percent of employment in the formal sector). At the same time, in the informal economy virtually all employment is informal. As a result, only around 10 percent of informal employment takes place in the formal economy. This differs from the situations in Brazil and Mexico, where almost 50 percent of informal workers are hired in formal enterprises (de la Parra 2017; Ulyssea 2018).

¹⁶ In practice, the condition of formal worker in the informal economy is difficult to imagine as it would require unregistered firms to register (some of) their workers (e.g., to the social-security system). Accordingly, the descriptive statistics will show how this category is in practice non-existent in the data and the main empirical analysis will not consider it among the outcomes of interest.

Figure 2 Number of inspectors per 100,000 individuals in 2010 and 2015, by department

Source: Author's elaboration based on administrative data.

Note: The graph shows the number of labor inspectors per 100,000 individuals in 2010 and 2015. Each dot corresponds to a different department that is included in the analysis (i.e., 23 departments).

Turning to the provision of mandated benefits and compliance with the legislation, less than 19 percent of the sample has a written employment contract (corresponding to around one-third of the employed population). Compliance with a number of mandated benefits (e.g., social security for health and pension, right to paid holidays, and severance payments) similarly ranges between 15 and 17 percent of the entire sample (i.e., corresponding to less than 30 percent of the employed population), while around 19 percent of the sample works more than the maximum allowed number of hours (i.e., 48 per week), and less than 10 percent has access to maternity or paternity leave. The average individual in the sample works 26.6 hours per week and has a monthly income from labor (in 2015 values) of 495,292.5 Colombian pesos (corresponding to around 120 USD, not adjusted for PPP).¹⁷

4. Identification and estimation

The purpose of the paper is to identify and estimate the effects of an increase in the number of inspectors on compliance with the legislation and to examine the resulting implications for the equilibrium in the labor market. Identification of a causal effect might be difficult in the presence of a simultaneous relationship between the treatment indicator and the outcome of interest, with no clear predictions on the direction of the bias. Governments might want to concentrate enforcement efforts in areas characterized by a higher risk of non-compliance. At the same time, enforcement systems might be more effective in areas characterized by better socioeconomic and institutional conditions. This means that simple ordinary least squares (OLS) estimates could be either downward or upward biased.

To circumvent this problem, I exploit discontinuities in the allocation rule used to distribute new vacancies of inspectors across departments and years as part of the Action Plan. These discontinuities generated differences in the number of inspectors working in departments that were otherwise characterized by a similar risk of non-compliance with the legislation. Specifically, a higher final score generally resulted in more inspectors being allocated to, and eventually working in, a department. However, the

¹⁷ Restricting the attention to the employed population, these values would correspond to 44.7 hours worked per week and a monthly income of 868,949.5 pesos (210 USD).

Table 3. Descriptive statistics at the individual level for personal and labor-market variables

	Mean	S.D.		Mean	S.D.
Individual characteristics			Informal employment in the formal economy	0.025	0.157
Male	0.456	0.498	Informal employment in the informal economy	0.361	0.480
Age	39.645	17.802	Law compliance		
Intermediate level of education	0.185	0.389	Written contract	0.187	0.390
Rural	0.095	0.294	Excessive working hours	0.189	0.392
Status in the labor market			Social-security pension	0.166	0.372
Employed	0.596	0.491	Social-security health	0.177	0.382
Unemployed	0.073	0.260	Paid holidays	0.157	0.363
Inactive	0.331	0.471	Severance pay	0.169	0.375
Status in employment			Hours worked and labor income		
Formal economy	0.233	0.423	Number of hours worked in main job	26.104	26.399
Formal employment	0.209	0.407	Number of hours worked overall	26.648	26.864
Formal employment in formal economy	0.208	0.406	Income from main job	397,029.7	900,587.3
Formal employment in the informal economy	0.002	0.042	Overall labor income	495,292.5	1,064,687.0
N	2,377,658			2,377,658	

Source: Author's elaboration based on the Gran encuesta integrada de hogares (GEIH).

Note: The table reports means and standard deviations for selected personal and labor-market variables computed over the main population of interest, corresponding to the working-age population interviewed in the GEIH between 2011 and 2014 in the 23 departments included in the analysis. All indicators related to status in employment and characteristics of the employment relation (e.g., compliance with the legislation, hours worked, and wages) are set to zero for individuals not in employment.

relationship between the normalized score and the distribution of inspectors presented a discontinuity within each integer of the score: being (below) above the .5 cutoff resulted in an additional inspector (not) being allocated.

This process took place in every hiring round between 2011 and 2014. To reflect this, the treatment indicator for department s in year t measures the cumulative change in the actual number of inspectors working in that department and year, and takes the following form:

$$\begin{aligned}
 & \sum_{t=2011}^{t=2014} \Delta \text{Insp}_{st} \\
 &= \begin{cases} \Delta \text{Insp}_{s2011} & \text{if } t=2011, \\ \Delta \text{Insp}_{s2011} + \Delta \text{Insp}_{s2012} & \text{if } t=2012, \\ \Delta \text{Insp}_{s2011} + \Delta \text{Insp}_{s2012} + \Delta \text{Insp}_{s2013} & \text{if } t=2013, \\ \Delta \text{Insp}_{s2011} + \Delta \text{Insp}_{s2012} + \Delta \text{Insp}_{s2013} + \Delta \text{Insp}_{s2014} & \text{if } t=2014, \end{cases} \quad (1)
 \end{aligned}$$

where ΔInsp_{st} is the yearly change in the actual number of inspectors taking place in a given department (i.e., compared to the previous year), while the cumulative version ($\sum_{t=2011}^{t=2014} \Delta \text{Insp}_{st}$) also takes into account the changes in the number of inspectors having taken place in the same department in all years from 2011 to year t .

Note that in the baseline results I construct this variable based on the change in the number of inspectors that were actually working in a given department and year, rather than the change in the number of inspectors that should have been working in a department and year based on the allocation rule set in the Action Plan. This is not a trivial difference, given that the capacity of meeting the hiring targets differed across departments, as discussed above. The decision to construct the treatment variable based on the actual change in the number of inspectors is motivated by the fact that this captures the true change in labor-law enforcement that was generated by the Action Plan. However, and also to shed light on the interpretation of my findings in terms of local average treatment effects (LATE), I will present additional results where the treatment variable is constructed based on the theoretical change. This is important given that the hiring of inspectors was subject to sizable delays, as discussed above, and these delays might be correlated with the outcomes of interest.

In a similar fashion, I construct the instrumental variable by counting the number of times a department s has been above the .5 cutoff between the beginning of the hiring process in 2011 and year t . Specifically, the instrumental variable is constructed as follows:

$$\sum_{t=2011}^{t=2014} \text{Above}_{s,t} = \begin{cases} \text{Above}_{s2011} & \text{if } t=2011, \\ \text{Above}_{s2011} + \text{Above}_{s2012} & \text{if } t=2012, \\ \text{Above}_{s2011} + \text{Above}_{s2012} + \text{Above}_{s2013} & \text{if } t=2013, \\ \text{Above}_{s2011} + \text{Above}_{s2012} + \text{Above}_{s2013} + \text{Above}_{s2014} & \text{if } t=2014, \end{cases} \quad (2)$$

where Above_{st} is a dummy indicator taking the value of 1 if department s has a final score whose decimal part is above the .5 cutoff within a given integer in year t , while the cumulative version ($\sum_{t=2011}^{t=2014} \text{Above}_{st}$) takes into account the value of this dummy computed for the same department in all previous years from 2011 onwards.

The intuition behind the identification strategy is to compare departments that had the same integer value of the normalized score in a given hiring round and differed only with respect to its decimal part, determining their positioning around the .5 cutoff. The idea is that departments characterized by largely different final scores are characterized by a very different risk of non-compliance with the labor legislation, and might differ on a number of observable and unobservable dimensions. Restricting attention to departments within the same integer value of the final score instead allows me to obtain a more meaningful comparison and to exploit a variation in treatment that is more likely to be exogenous. Identification thus resembles a regression discontinuity design (RDD), with the only difference being that the treatment indicator is constructed cumulatively over time.¹⁸ In the rest of this section, I discuss the main identification assumptions behind this empirical approach.

A first necessary step is to verify the presence of a first-stage relationship. Here, the aim is to verify whether being above the .5 cutoff within a given integer across different hiring rounds generates a strong enough variation in the number of inspectors that were working in a given department. In this respect, the main concern is represented by the fact that simply relying on the variation in the number of inspectors generated by the .5 cutoff might not provide enough variation in treatment intensity. Recall that the allocation of inspectors was first based on the integer value of the final score, and the department's positioning around the .5 cutoff determined only whether an additional inspector would have been allocated to the department.

I directly test for this hypothesis by presenting the first-stage relationship, thereby regressing $\sum_{t=2011}^{t=2014} \Delta \text{Insp}_{st}$ against $\sum_{t=2011}^{t=2014} \text{Above}_{st}$. I also vary the specification in terms of (a) how I control for the integer value of the final score (i.e., not controlling for it, including it as a linear control, and including dummies for the different integers), (b) whether I add additional individual-level controls (i.e., for sex, age, urban area of residence, and educational attainments), and (c) whether I also include a first-order polynomial of the decimal part of the final score.

¹⁸ In this sense, the identification strategy also resembles the Maimonides' rule as in Angrist and Lavy (1999).

The results are presented in supplementary online appendix [table S2.2](#). They show that the first-stage relationship is strong enough to be used for identification. Point estimates are very similar when including or excluding individual-level controls. Similarly, adding the first-order polynomial of the decimal part of the final score does not substantially change the results. However, the first-stage relation is strong enough for identification only when I include the integer part of the final score through dummy variables (i.e., rather than not controlling for it, or controlling for it linearly). This makes sense, as the .5 cutoff captures only part of the variation in the change in the number of inspectors (i.e., the part that is determined by the decimal value of the final score, as explained above). This variation is precisely estimated only when comparing departments that lie on the same integer value of the final score. Overall, the first-stage results show that, after controlling for the integer part of the final score with dummies, I obtain a first-stage relationship which is both strong enough and robust to changes in the specification.

While the presence of the first-stage relationship can be directly tested, for the other identification assumptions only suggestive evidence can be provided.

First, identification rests on the assumption that departments could not manipulate their values of the normalized score in order to lie on either side of the cutoff. While it is unclear in which directions this dynamic would play, it could be assumed that departments with a high (low) commitment to labor-law enforcement would self-select above (below) the cutoff. This would likely result into an upward bias of treatment effects if these same characteristics also have an independent effect on the outcomes of interest.

However, manipulation of the allocation rule is unlikely to have happened. To start with, this is because the different indicators were announced by the central government through ministerial decrees and there is no evidence that departments were involved in the definition of the allocation rule. Some of these indicators (e.g., focus on specific economic sectors or types of enterprises) were directly mentioned in the Action Plan agreed between Colombia and the US, meaning that manipulation would require departments being able to influence an international agreement. Additionally, it is implausible that departments could manipulate the information that was used to compute the indicators that entered in the allocation rule. These indicators were measured using official statistics by DANE or the Ministry of Labor and they referred to values of these variables in previous years. Finally, the allocation of inspectors resulted from the department's relative ranking at the national level (i.e., the final score was normalized to sum to 100 across departments). Self-selection with respect to the cutoff would require departments to be able to predict how the different indicators would influence their positioning within a given integer, after being ranked against all other departments.

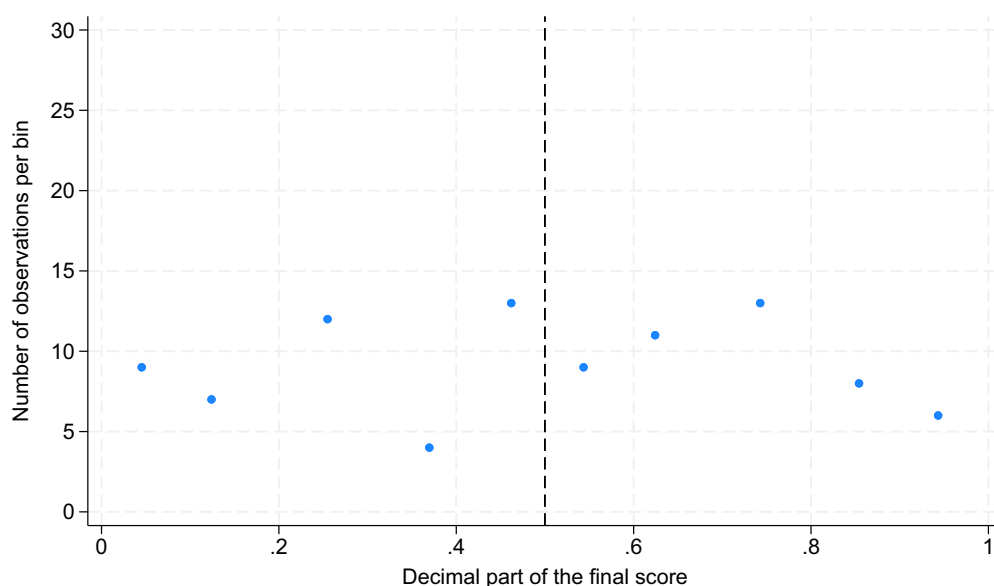
In line with this understanding of the policy, there is descriptive evidence in support of the fact that departments were unlikely to manipulate the final score. In this part of the analysis, I look at the 92 units of treatment variation (i.e., combination of 23 departments over 4 hiring rounds) and focus on the decimal part of their normalized score irrespective of the integer (e.g., a department with a final score of 2.25 in a given hiring round enters the analysis with .25). Using this categorization, I construct bins of equal size over the support created by the decimal part of the normalized score (i.e., from .0 to .99) and plot the number of observations in each bin (for an arbitrary bin size of 0.1, leading to 10 different bins) (fig. 3). The graph shows that the number of observations in each bin changes along the bandwidth, but these variations are not big and do not happen systematically at the cutoff.¹⁹

Secondly, identification also requires that the density of unobserved heterogeneity and the conditional expectation of potential outcomes evolve smoothly around the cutoff. If this is not the case, estimates of treatment effects would be biased by the presence of heterogeneity also jumping around the cutoff. In this case, there are no clear predictions of the direction of the bias. In the absence of a direct test of the identification assumption, the most sensible approach is to provide supportive evidence by studying the evolution of observable characteristics around the cutoff. Overall, these tests point to the absence of any discontinuity.

Starting with the graphical analysis, I plot the average values for selected characteristics in each bin along the entire bandwidth (this time, for a much smaller bin size of 0.01) (fig. 4, plotting residuals after

¹⁹ In line with this evidence, [table 2](#) shows that the average of the decimal part of the normalized score was equal to 0.49, the dummy for being above the .5 cutoff within each integer was equal to 0.51, and the number of times a department has been above the cutoff over the four hiring rounds was equal to 2.

Figure 3 Number of departments whose decimal part of the final score falls within a given decimal, over the four hiring rounds



Source: Author's elaboration based on administrative data.

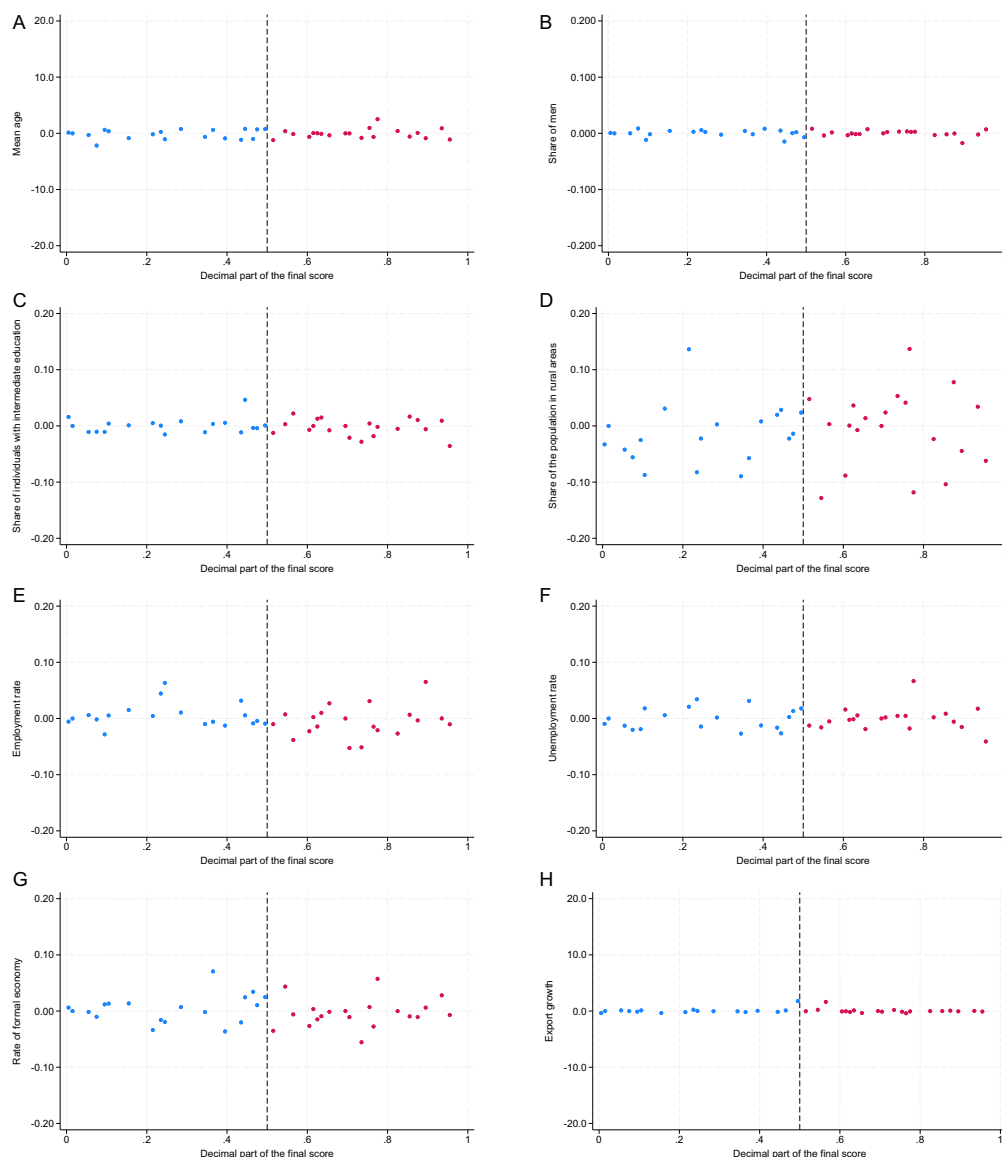
Note: The graph shows the number of units of treatment variation (i.e., corresponding to 23 departments over 4 different hiring rounds for a total of 92 observations) whose decimal part of the final score falls within a given bin for an arbitrary bin size of 0.1. Each observation enters the analysis with the decimal part of the final score, independently from its integer value.

controlling for the integer part of the final score). Averages are computed in the year before the relevant normalized score was assigned (e.g., in 2010 for the 2011 hiring round), which can be thought of as the baseline year before each hiring round. The results report no sign of discontinuity at the cutoff for individual-level characteristics (panels A to D), labor-market variables (panels E to G), and the evolution of exports (panel H).²⁰ A similar picture emerges when looking at a broader range of characteristics (always in the year before the relevant hiring round), but splitting observations simply based on whether they are above or below the cutoff (table 4). All the selected characteristics are balanced between the two groups and none of the *t*-tests for equality of means reports a statistically significant difference. Since the dummy for being above the cutoff will be constructed cumulatively over time, I want to compare departments that over the years have been above the cutoff a given number of times (i.e., from zero to four). These descriptive statistics are measured at policy baseline (i.e., in 2010). Even in this case, all characteristics are balanced across groups (see supplementary online appendix table S2.3). The table also presents, for each variable, the correlation with the instrument used in the main analysis, showing that this is low in all cases.

Thirdly, I test whether the normalized score and the .5 cutoff correlate with some observable characteristics. The final score is likely correlated with a number of demographic and labor-market variables, as the allocation rule was designed to capture the risk of non-compliance with the legislation. However, after controlling for the integer part of the final score, this correlation should become substantially weaker and eventually disappear. If this does not happen, it would suggest that departments below and above the cutoff within the same integer are not yet comparable, thus invalidating the main intuition behind my identification strategy. In table 5, I regress the final score against selected demographic and labor-market characteristics measured in the year before the relevant hiring round. Results show that there is a strong correlation between these variables and the allocation rule when integer dummies are excluded (five out of eight variables are statistically significant at the 1 percent level). However, point estimates become close to zero and statistically non-significant after including the integer dummies.

²⁰ Unfortunately, the publicly available department-level information on exports does not allow the country of destination to be identified. However, the US represents the main market for Colombian exports.

Figure 4 Evolution of selected characteristics around the cutoff, plotting residuals. (A) Age, (B) male (%), (C) population with intermediate level of education (%), (D) population living in rural areas (%), (E) employment rate, (F) unemployment rate, (G) rate of employment in the formal economy, and (H) exports growth.



Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The graph plots residuals of the relationship between the decimal part of the final score and selected personal and labor-market characteristics measured in the year before the relevant hiring round, after controlling for the integer part of the final score. The analysis is conducted for the different units of treatment variation (i.e., corresponding to 23 departments over 4 different hiring rounds for a total of 92 observations) that have been sorted according to the decimal part of the allocation rule (e.g., a department with a final score of 2.25 in a given hiring round enters the analysis with .25) and for a bin size of 0.01. This means that the same department can belong to a different bin over different years.

Finally, I return to the presence of policy implementation delays. The presence of these delays, which differed across departments, could represent a concern for identification if these delays are related to the policy variation that I use for identification (i.e., whether or not the department is above the .5 cutoff). Descriptive evidence ruling out this risk has already been provided in table 1, showing that the

Table 4. Descriptive statistics for units of treatment variation below and above the cutoff, year before the relevant hiring round, and *t*-tests for equality of means

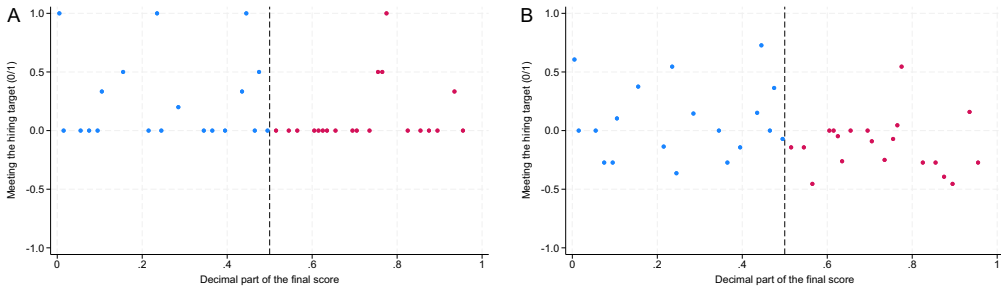
	Below the cutoff		Above the cutoff		t-test
	Mean	S.D.	Mean	S.D.	
Individual characteristics					
Male	0.49	0.01	0.49	0.01	−0.149
Age	39.02	1.17	38.91	1.33	0.418
Intermediate level of education	0.18	0.02	0.18	0.02	0.444
Rural	0.29	0.09	0.31	0.14	−0.587
Status in the labor market					
Employed	0.599	0.040	0.593	0.049	0.669
Unemployed	0.103	0.027	0.099	0.024	0.830
Inactive	0.331	0.045	0.342	0.054	−0.992
Status in employment					
Formal economy	0.175	0.052	0.175	0.073	0.053
Formal employment	0.156	0.049	0.155	0.069	0.077
Formal employment in formal economy	0.155	0.048	0.153	0.067	0.099
Informal employment in formal economy	0.021	0.006	0.021	0.007	−0.374
Informal employment in informal economy	0.423	0.061	0.417	0.063	0.448
Law compliance					
Written contract	0.136	0.043	0.137	0.060	−0.119
Excessive working hours	0.195	0.040	0.194	0.040	0.106
Social-security pension	0.120	0.046	0.119	0.060	0.056
Social-security health	0.130	0.049	0.129	0.063	0.102
Paid holidays	0.114	0.042	0.111	0.057	0.228
Severance pay	0.124	0.043	0.122	0.059	0.198
Hours worked and wages					
Number of hours worked in main job	43.43	2.87	43.90	3.01	−0.766
Number of hours worked overall	44.53	2.59	44.95	2.55	−0.788
Income from main job	887,013.40	96,571.95	916,941.40	138,970.20	−1.195
Overall labor income	721,077.90	118,901.50	726,714.80	175,232.20	−0.180
Exports					
Exports growth	0.00	0.41	−0.01	0.33	0.180
N	45		47		

Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The table reports means and standard deviations for selected variables computed at the department level between 2010 and 2013, as well as the results of *t*-tests for equality of means. Departments are divided according to whether they are above or below the .5 cutoff in a given hiring round. Descriptive statistics refer to the year before the relevant hiring round. Given the functioning of the policy, the same department can be above or below the cutoff over different hiring rounds.

probability of meeting the hiring target, while being correlated with the integer part of the final score, is uncorrelated with the .5 cutoff, especially after controlling for the integer part of the final score through dummy variables (see column 4 of table 1). I now provide additional evidence, replicating the analysis presented in fig. 4, but now using as variable of interest the dummy indicator equal to 1 if the department has met the hiring target in a particular hiring round (fig. 5). The results are presented using the raw association between these two variables (panel A), as well as plotting residuals after controlling for the integer part of the final score (panel B). In both cases, they confirm that the probability of meeting the hiring target does not present any discontinuity around the cutoff.

Figure 5 Evolution of the probability of meeting the hiring target around the cutoff. (A) Raw, and (B) plotting residuals.



Source: Author's elaboration based on administrative data.

Note: Panel A plots the relation between the probability of a department meeting the hiring target in a particular year and the decimal part of the final score. Panel B plots the same relation, but after controlling for the integer part of the final score. The analysis is conducted for the different units of treatment variation (i.e., corresponding to 23 departments over 4 different hiring rounds for a total of 92 observations) that have been sorted according to the decimal part of the allocation rule (e.g., a department with a final score of 2.25 in a given hiring round enters the analysis with .25) and for a bin size of 0.01. This means that the same department can belong to a different bin over different years.

I run the following regression:

$$Y_i = \beta_0 + \beta_1 \sum_{t=2011}^{t=2014} \Delta \text{Insp}_{st} + \beta_2 \text{Integ}_{st} + \beta_3 \text{Dec}_{st} + \beta_4 X_i + \epsilon_i, \quad (3)$$

where the endogenous treatment indicator is instrumented by the first stage:

$$\sum_{t=2011}^{t=2014} \Delta \text{Insp}_{st} = \gamma_0 + \gamma_1 \sum_{t=2011}^{t=2014} \text{Above}_{st} + \gamma_2 \text{Integ}_{st} + \gamma_3 \text{Dec}_{st} + \gamma_4 X_i + v_i.$$

Here, Y_i is the outcome of interest for individual i , and $\sum_{t=2011}^{t=2014} \text{Above}_{st}$ is the number of times the department s has been above the .5 cutoff over the different hiring rounds that have taken place up until year t included (i.e., constructed cumulatively over time, as presented in equation (2)). This variable is used as an instrument for $\sum_{t=2011}^{t=2014} \Delta \text{Insp}_{st}$, which is the main regressor of interest and represents the cumulative actual change in the number of inspectors actually working in department s between 2011 and year t (i.e., see equation (1)).

Without the summations, these two equations would correspond to a standard RDD setting. The cumulative construction of these variables over time accounts for the presence of several hiring rounds and policy implementation delays. This leaves the intuition behind the identification strategy mostly unchanged, while partially modifying the mechanics of the regression. In particular, the relationship between the running variable (i.e., the decimal part of the final score) and the instrumental variable (i.e., the dummy for being above the cutoff) is no longer univocal within a given year. In a standard RDD, all departments (below) above the cutoff within a given year would take the value of (0) 1 in the instrumental variable. Instead, here departments characterized by a final score whose decimal part lies on different sides of the cutoff in a given hiring round might nevertheless present the same value of the instrumental variable in that year. This is because the construction of the instrumental variable reflects the department's positioning around the cutoff not only in that year, but also in previous hiring rounds (i.e., since 2011). For this reason, not all standard tests that are common in the RDD literature are directly applicable here.

The main control variable is Integ_{st} , which is a series of dummies corresponding to the integer part of the final score assigned to department s in year t . This allows me to compare departments with similar risks of law non-compliance, as captured by the allocation rule in that given year (e.g., to avoid comparing departments with a score of 1 and a score of 4). The discussion presented above has also shown that the inclusion of these dummy variables (a) is necessary to obtain a strong enough first-stage relation (see again supplementary online appendix table S2.2), and (b) it allows me to compare departments that are

much more similar in terms of observable characteristics (see [table 5](#)). Nevertheless, it is important to note that, even within the same integer, the variation in the final score generated by its decimal part might nevertheless be non-random. To partially account for this, I also include Dec_{st} , which is a polynomial of order 1 in the decimal part of the final score. This controls for the smooth relationship between the outcome and the running variable, allowing me to isolate the discontinuous jump at the cutoff. If the identifying assumptions introduced above are satisfied, potential outcomes vary smoothly in the running variable, so controlling linearly for the decimal part of the final score should not affect the results. I also include a vector of individual-level characteristics X_i . This includes dummies for sex, location of residence (i.e., urban or rural), highest level of educational attainment (equal to 1 if the individual has reached upper secondary education or the first stage of tertiary education), and a linear control for age.

Standard errors are clustered at the department and year level (i.e., leading to the identification of 92 different clusters corresponding to 23 departments over 4 different hiring rounds). Unless otherwise specified, all outcome variables, including those related to measures of compliance with the labor law, are set to zero for individuals not in employment. This is to avoid issues of sample selection related to the fact that the employed population, for which these indicators can be observed, is not necessarily representative of the overall population. Triangle kernel weights are used in all specifications. All regressions are estimated using two-stage least squares and the tables that follow will report coefficient estimates.

5. Main results

This section presents the results of the paper on indicators of labor-law compliance and the equilibrium in the labor market. Jointly analyzing these aspects is of key importance for interpretation, as the results on labor-law compliance will help understand the changes observed in terms of employment levels in the formal and informal labor sectors of the economy.

5.1. Labor-law compliance

[Table 6](#) presents the main results of the analysis with respect to a number of indicators of labor-law compliance. For each outcome of interest, I report four estimates. In column (1), I present results for regressions that control only for the integer part of the final score with dummy variables. In column (2), I add the individual-level controls mentioned above. Columns (3) and (4) follow the same logic, but here I also include a linear control for the decimal part of the final score. In panel A, I report results where the treatment variable is constructed based on the actual change in the number of inspectors working in a department (corresponding to $\sum_{t=2011}^{t=2014} \Delta \text{Insp}_{s,t}$ in equation (1)). In panel B, the treatment variable is instead constructed using information on the theoretical allocation of inspectors in a department. In both cases, the treatment variable is instrumented with the variable counting the number of times a department has been above the .5 cutoff across different hiring rounds (corresponding to $\sum_{t=2011}^{t=2014} \text{Above}_{s,t}$ in equation (2)). The reported coefficient corresponds to β_1 in equation (3) and it captures the effect of an additional inspector.

The results show that the hiring of an additional inspector has a small positive effect on some measures of law compliance. Specifically, I find that increasing labor-law enforcement raises the likelihood of having a written employment contract, reduces the probability of working an excessive number of hours, and increases the chances of being covered by social security for pensions. I also find positive effects on the probability of being entitled to severance payments, having the right to paid holidays and being covered by social security for health. However, coefficient estimates for these latter outcome variables are smaller in magnitude and fall short of statistical significance. In all cases, the estimated coefficients are very similar when I include or exclude the individual-level controls, as well as with and without the first-order polynomial of the decimal part of the final score.

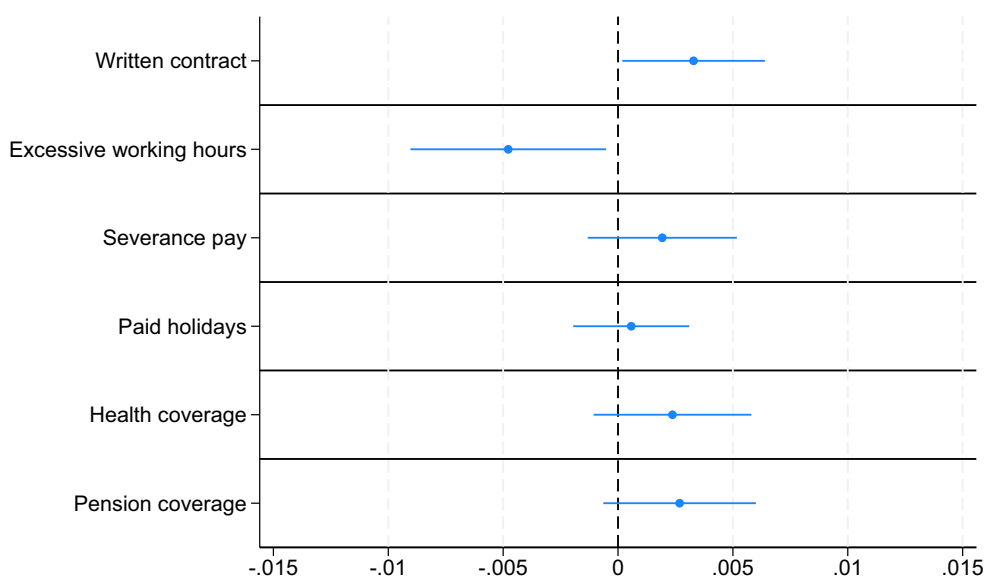
Even more importantly, results are also similar when constructing the treatment variable based on the actual change or, alternatively, the theoretical change in the number of inspectors. This suggests that treatment effects are not driven by the subgroup of departments that were more successful in meeting their hiring targets, thereby shedding light on the LATE interpretation of the IV results. This also

Table 6. Baseline results: Measures of labor-law compliance

	Panel A: Using actual change in inspectors				Panel B: Using theoretical change in inspectors				Mean below cutoff	Target non-compliance
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)		
Written contract	0.005** (0.003)	0.005** (0.003)	0.004* (0.002)	0.004* (0.003)	0.004** (0.002)	0.004** (0.002)	0.003* (0.002)	0.003* (0.002)	0.185	0.247
Excessive working time	-0.008* (0.004)	-0.007* (0.004)	-0.006* (0.004)	-0.006* (0.004)	-0.005** (0.002)	-0.005** (0.002)	-0.005* (0.003)	-0.005* (0.003)	0.191	0.691
Severance payments	0.003 (0.003)	0.003 (0.003)	0.002 (0.003)	0.002 (0.003)	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)	0.169	0.316
Paid holidays	0.001 (0.002)	0.002 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.001)	0.001 (0.001)	0.001 (0.002)	0.001 (0.002)	0.157	0.367
Health social security	0.004 (0.003)	0.004 (0.003)	0.003 (0.003)	0.003 (0.003)	0.003 (0.002)	0.003 (0.002)	0.002 (0.002)	0.002 (0.002)	0.177	0.29
Pension social security	0.004* (0.003)	0.005* (0.003)	0.003 (0.003)	0.003 (0.003)	0.003* (0.002)	0.003* (0.002)	0.003 (0.002)	0.003 (0.002)	0.166	0.301
Above (cumulative over years)	1.490*** (0.405)	1.490*** (0.405)	1.610*** (0.437)	1.609*** (0.437)	2.094*** (0.299)	2.094*** (0.299)	2.056*** (0.320)	2.056*** (0.320)	-	-
F-test	13.5352	13.5456	13.5445	13.5484	49.1006	49.1183	41.1904	41.2051	-	-
N	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	-	-
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	-	-
Individual level controls	No	Yes	No	Yes	No	Yes	No	Yes	-	-
Polynomial of order 1 for decimal of	No	No	Yes	Yes	No	No	Yes	Yes	-	-
final score									-	-

Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on different measures of labor-law compliance. Reported coefficients correspond to the parameter β_1 in equation (3) in the main text. Standard errors are clustered at the department and year level, leading to the identification of 92 clusters. The definition of the outcomes of interest is available in supplementary online appendix table S2.1. Panel A presents results obtained by using the actual change in the number of inspectors to construct the endogenous variable (constructed cumulatively over years, as described in the main text), while panel B presents results obtained by using the theoretical change in the number of inspectors to construct the endogenous variable (always constructed cumulatively over years). In both cases, the instrumental variable corresponds to the number of times a given department has been above the .5 cutoff (see the main text for details). The table also reports (a) the mean in the outcome of interest below the cutoff, and (b) the share of non-compliance for a given variable in the formal economy. * <10 percent, ** <5 percent, *** <1 percent.

Figure 6 Treatment effects on measures of labor-law compliance

Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The figure plots point estimates and 90 percent confidence intervals for the effect of an additional inspector on different measures of labor-law compliance, corresponding to the estimates presented in column (4) of panel A in [table 6](#).

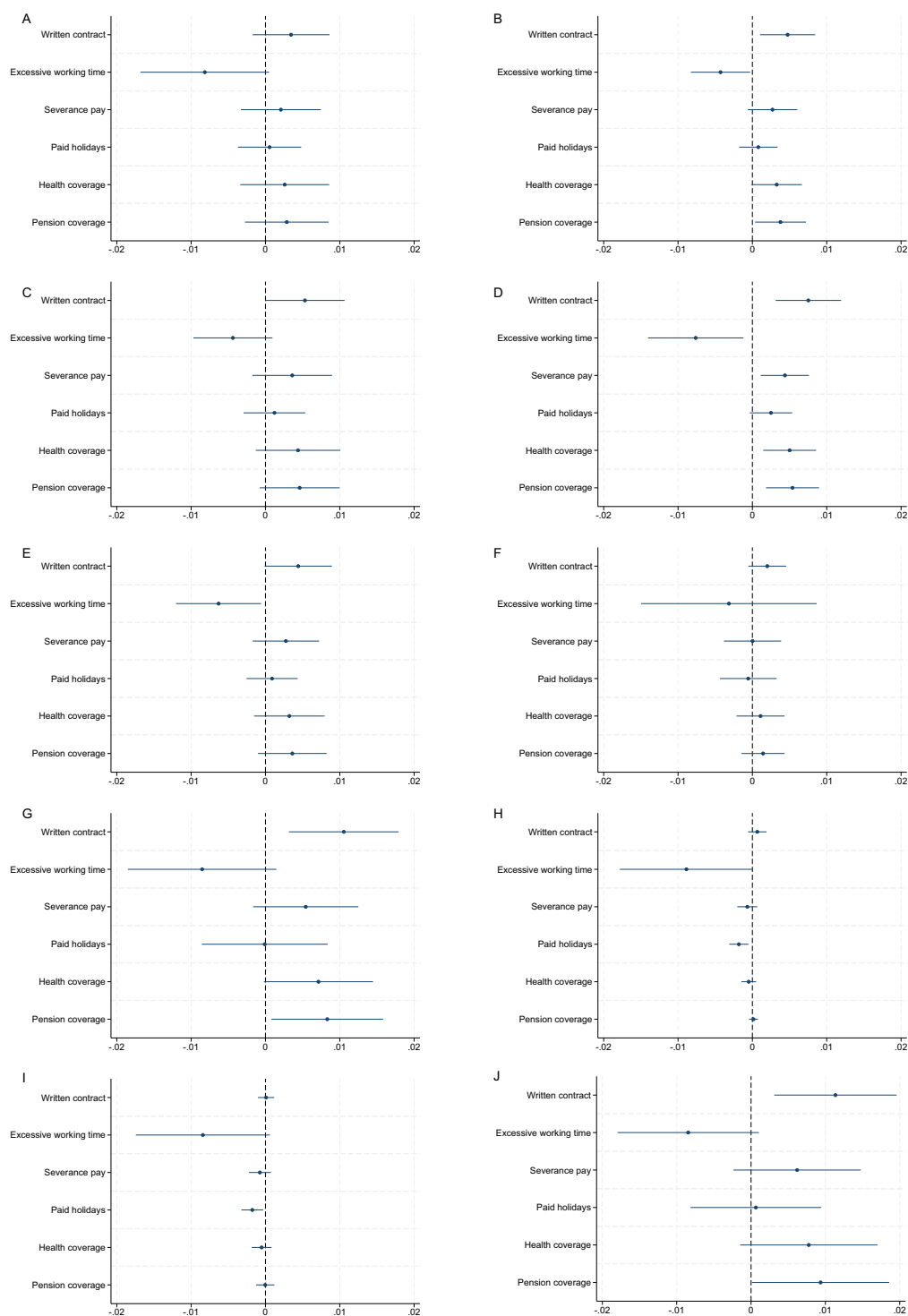
provides additional suggestive evidence of the fact that differences in departments' abilities to meet the hiring targets were probably not driven by unobservable factors: if this was the case, I would expect that treatment effects would also differ between departments that complied with the hiring targets and departments that did not. This means that the variation in treatment intensity across departments generated by differences in policy implementation delays does not play any meaningful role in identifying treatment effects within the current identification approach. I also note that the F -test is higher when constructing the treatment variable using the target number of inspectors, since in this case it is not influenced by hiring delays. However, the F -test is sufficiently high in all specifications.

[Figure 6](#) presents a graphical visualization of the results for all measures of labor-law compliance discussed above, plotting point estimates and confidence intervals.

Looking at the magnitude of the effects, I note that an additional inspector increases the probability of having a number of job-related benefits by between 0.3 and 0.5 percentage points (from a mean in the outcomes of interest below the cutoff between 15 and 20 percent, depending on the variable analyzed). This corresponds to an increase of around 2 percent, or a 14 percent increase when expressing this in terms of the increase in the number of inspectors that corresponds to moving from the department at the 25th percentile to the department at the 75th percentile in terms of its number of labor inspectors. I note that this is not the only approach to rescaling. In particular, the analysis below will clarify that labor inspectors improve labor-law compliance primarily in formal-sector enterprises. I can then rescale my estimates by the level of non-compliance with the legislation in the formal sector (i.e., what I call "target non-compliance" in [table 6](#)). Using this higher benchmark, I find that an additional inspector increases labor-law compliance by around 1 percent. This corresponds to a 7 percent increase when expressing this in terms of the increase in the number of inspectors that corresponds to moving from the department at the 25th percentile to the department at the 75th percentile.

I then conduct some heterogeneous analysis and summarize the findings in [fig. 7](#), which plots, for each group, point estimates and confidence intervals for the different measures of labor-law compliance analyzed above. I find that the positive effects on compliance with the legislation are very similar between men and women. Effects are also very similar among individuals with low and high educational attainments, despite previous studies having found heterogeneous effects of labor enforcement between

Figure 7 Heterogeneous effects on measures of labor-law compliance . (A) Men, (B) Women, (C) low education, (D) high education, (E) urban areas, (F) rural areas, (G) formal sector, (H) informal sector, (I) small firm, and (J) large firm.



Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The figure plots point estimates and 90 percent confidence intervals for the effect of an additional inspector on different measures of labor-law compliance separately for different samples, following the most complete specification presented in table 6. The distinction between small and large firms is based on whether the enterprise employs at least four people. All other definitions are directly taken from the GEIH or follow the terminology already introduced in the paper.

skilled and unskilled workers (Dix-Carneiro and Kovak 2017; Ponczek and Ulyssea 2022).²¹ Treatment effects are larger in urban than rural areas, in line with the evidence showing that labor inspectors are more likely to target enterprises close to their labor offices, which tend to be located in cities (Almeida and Carneiro 2012). At the same time, the positive treatment effects appear only in the formal sector of the economy (i.e., registered enterprises) while point estimates for the informal economy are precisely estimated around zero for most outcomes.²² A similar pattern emerges when analyzing differences in treatment effects by firms' size, as these are close to zero for small firms while being positive and significant for large firms. However, these differences might be driven by the prevalence of small firms in the informal sector of the economy.

Tables 7 and 8 replicate the results presented in table 6, but separately for individuals in the formal and in the informal sector, respectively. They confirm the presence of asymmetric effects of public enforcement between the formal and the informal economy. This was assumed in previous empirical and theoretical contributions, despite never being shown before. The phenomenon can be explained by two separate, but related, factors. To start with, compliance with the labor legislation is extremely low in the informal sector at baseline. As an example, the probability of having a written employment contract is equal to around 75 percent in the formal sector but only 3 percent in the informal sector. This means that promoting compliance with the labor legislation is very difficult, also because it would often imply that the firm registers.²³ Relatedly, it is possible that the lack of an effect in the informal sector of the economy is connected to the fact that inspectors operate by mostly visiting formal firms (e.g., because these are the only firms that appear in official registries), while leaving the informal sector unchecked.

5.2. Status in the labor market, hours worked, and wages

Table 9 presents the results on overall employment, as well as the distribution of employment between the formal and the informal sector. Differently from previous studies, I find that overall employment does not change. Nevertheless, I document a change in the distribution of employment. Specifically, I find a decrease in informal employment in the formal economy. These are workers that were already employed in the formal sector, but were kept off the books by their enterprise. Following Ulyssea (2018), I call this the intensive margin of formalization and interpret it as the formalization effect exercised by labor inspectors when visiting formal firms (i.e., they do not allow formal employers to hire informally). This effect is rather large, with an additional inspector decreasing the probability of working informally in the formal sector by 4 percent. Moving from a department at the 25th percentile to a department at the 75th percentile would decrease the probability of working informally in the formal sector by 28 percent. Instead, I do not find any effect on the probability of working informally in the informal economy, known as the extensive margin of formalization. This is in line with the fact that labor inspectors, while in theory should also target informal firms, mostly operate by targeting firms in the formal economy, while leaving informal firms mostly unchecked. This is also in line with the heterogeneous results on measures of labor-law compliance, which showed lack of effects on the provision of mandated benefits in the informal sector of the economy.

And what are the effects on hours worked and labor income? Table 10 presents the results for these two outcome variables. I find a decrease in the number of hours worked. This is true when considering only employed individuals, or when also including non-employed individuals in the analysis (i.e., assigning them a value of 0 for hours worked). Next, I do not find any effect on labor income for the overall sample (i.e., when labor income is set to 0 for individuals not in employment). However, results change when restricting attention to employed individuals. For this sample, I find that an increase in labor inspection leads to an increase in labor income (between 1 and 2 percent depending on the chosen

²¹ In my analysis, individuals with low educational attainments are those with (a) less than basic or (b) basic education (i.e., International Standard Classification of Education (ISCED) groups 0–2), while individuals with high educational attainments are those with more than basic education (i.e., ISCED 3 and above).

²² The only exception is represented by the effect on the probability of working an excessive number of hours, which is negative and significant in the informal sector of the economy also. This might suggest a spillover from the formal to the informal sector, also because the number of hours worked is the only indicator, among those considered in the paper, whose variation does not necessarily require the existence of an employment contract.

²³ For instance, in order to provide social security to its workers (for either pension or health), the firm needs to register with the social-security institute. This would imply that, by definition, the firm is no longer informal.

Table 7. Formal sector: Measures of labor-law compliance

	Panel A: Using actual change in inspectors				Panel B: Using theoretical change in inspectors			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Written contract	0.011** (0.005)	0.012** (0.005)	0.010*** (0.004)	0.010*** (0.004)	0.008*** (0.003)	0.008*** (0.003)	0.007*** (0.003)	0.008*** (0.003)
Excessive working time	-0.009 (0.006)	-0.009 (0.006)	-0.009 (0.006)	-0.009 (0.006)	-0.006 (0.004)	-0.006 (0.004)	-0.007 (0.004)	-0.006 (0.004)
Severance payments	0.006 (0.004)	0.007 (0.004)	0.005 (0.004)	0.005 (0.004)	0.004 (0.003)	0.004 (0.003)	0.004 (0.003)	0.004 (0.003)
Paid holidays	0.001 (0.005)	0.001 (0.005)	-0.000 (0.005)	-0.000 (0.005)	0.000 (0.004)	0.001 (0.004)	-0.000 (0.004)	-0.000 (0.004)
Health social security	0.008* (0.005)	0.009* (0.004)	0.006 (0.005)	0.007 (0.004)	0.005* (0.003)	0.006** (0.003)	0.005 (0.003)	0.005* (0.003)
Pension social security	0.009** (0.005)	0.010** (0.005)	0.008* (0.005)	0.008* (0.005)	0.006** (0.003)	0.007*** (0.003)	0.005* (0.003)	0.006* (0.003)
N	554,096	554,096	554,096	554,096	554,096	554,096	554,096	554,096
F-test	11.5654	11.5797	17.7551	10.9645	44.4238	44.4527	38.2374	38.2658
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual level controls	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial of order 1 for decimal of final score	No	No	Yes	Yes	No	No	Yes	Yes

Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on different measures of labor-law compliance. The analysis is conducted only for individuals working in the formal sector. Reported coefficients correspond to the parameter β_1 in equation (3) in the main text. Standard errors are clustered at the department and year level, leading to the identification of 92 clusters. The definition of the outcomes of interest is available in supplementary online appendix table S2.1. Panel A presents results obtained by using the actual change in the number of inspectors to construct the endogenous variable (constructed cumulatively over years, as described in the main text), while panel B presents results obtained by using the theoretical change in the number of inspectors to construct the endogenous variable (always constructed cumulatively over years). In both cases, the instrumental variable corresponds to the number of times a given department has been above the .5 cutoff (see the main text for details). * <10 percent, ** <5 percent, *** <1 percent.

Table 8. Informal sector: Measures of labor-law compliance

	Panel A: Using actual change in inspectors				Panel B: Using theoretical change in inspectors			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Written contract	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Excessive working time	-0.011* (0.006)	-0.011* (0.006)	-0.009 (0.006)	-0.009 (0.005)	-0.008** (0.004)	-0.008** (0.004)	-0.008* (0.004)	-0.007* (0.004)
Severance payments	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Paid holidays	-0.002** (0.001)	-0.002** (0.001)	-0.002** (0.001)	-0.002** (0.001)	-0.001** (0.001)	-0.001** (0.001)	-0.001** (0.001)	-0.002** (0.001)
Health social security	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Pension social security	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
N	862,167	862,167	862,167	862,167	862,167	862,167	862,167	862,167
F-test	15.5149	15.5473	22.5657	15.6363	50.488	50.5462	41.5582	41.6076
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual level controls	No	Yes	No	Yes	No	Yes	No	Yes
Polynomial of order 1 for decimal of final score	No	No	Yes	Yes	No	No	Yes	Yes

Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on different measures of labor-law compliance. The analysis is conducted only for individuals working in the informal sector. Reported coefficients correspond to the parameter β_1 in equation (3) in the main text. Standard errors are clustered at the department and year level, leading to the identification of 92 clusters. The definition of the outcomes of interest is available in supplementary online appendix table S2.1. Panel A presents results obtained by using the actual change in the number of inspectors to construct the endogenous variable (constructed cumulatively over years, as described in the main text), while panel B presents results obtained by using the theoretical change in the number of inspectors to construct the endogenous variable (always constructed cumulatively over years). In both cases, the instrumental variable corresponds to the number of times a given department has been above the .5 cutoff (see the main text for details). * <10 percent, ** <5 percent, *** <1 percent.

Table 9. Baseline results: Overall, formal and informal employment

	Panel A: Using actual change in inspectors				Panel B: Using theoretical change in inspectors				Mean below cutoff
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	
Employed	-0.004 (0.003)	-0.003 (0.003)	-0.002 (0.003)	-0.002 (0.003)	-0.003 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.002)	0.597
Formal employment in formal economy	0.005	0.005	0.003	0.004	0.003*	0.003*	0.003	0.003	0.206
Informal employment in formal economy	(0.003) -0.001**	(0.003) -0.001**	(0.003) -0.001*	(0.003) -0.001*	(0.002) -0.001**	(0.002) -0.001**	(0.002) -0.001*	(0.002) -0.001*	0.025
Informal employment in informal economy	(0.000) -0.008	(0.000) -0.007	(0.000) -0.005	(0.000) -0.004	(0.000) -0.005*	(0.000) -0.005	(0.000) -0.004	(0.000) -0.003	0.363
<i>N</i>	(0.005) 2,377,658	(0.005) 2,377,658	(0.004) 2,377,658	(0.004) 2,377,658	(0.003) 2,377,658	(0.003) 2,377,658	(0.003) 2,377,658	(0.003) 2,377,658	-
<i>F</i> -test	13.5352	13.5456	13.5445	13.5484	49.1006	49.1183	41.1904	41.2051	-
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	-
Individual level controls	No	Yes	No	Yes	No	Yes	No	Yes	-
Polynomial of order 1 for decimal of final score	No	No	Yes	Yes	No	No	Yes	Yes	-

Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

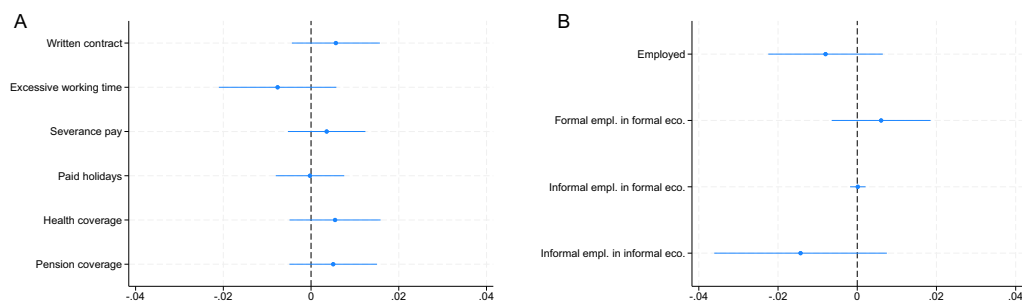
Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on indicators of status in the labor market. Reported coefficients correspond to the parameter β_1 in equation (3) in the main text. Standard errors are clustered at the department and year level, leading to the identification of 92 clusters. The definition of the outcomes of interest is available in supplementary online appendix table S2.1. Panel A presents results obtained by using the actual change in the number of inspectors to construct the endogenous variable (constructed cumulatively over years, as described in the main text), while panel B presents results obtained by using the theoretical change in the number of inspectors to construct the endogenous variable (always constructed cumulatively over years). In both cases, the instrumental variable corresponds to the number of times a given department has been above the .5 cutoff (see the main text for details). The table also reports the mean in the outcome of interest below the cutoff. * <10 percent, ** <5 percent, *** <1 percent.

Table 10. Baseline results: Hours worked and labor income

	Panel A: Using actual change in inspectors				Panel B: Using theoretical change in inspectors				Mean below cutoff
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	
Number of hours worked	-0.380* (0.209)	-0.346* (0.196)	-0.287 (0.189)	-0.258 (0.178)	-0.270** (0.122)	-0.246** (0.116)	-0.224* (0.131)	-0.202 (0.124)	26.662
Number of hours worked (employed only)	-0.348*	-0.323*	-0.319*	-0.297*	-0.250**	-0.232**	-0.251*	-0.235*	44.685
Labor income	(0.193)	(0.183)	(0.182)	(0.174)	(0.123)	(0.117)	(0.130)	(0.125)	
	-1,159,005	-518,940	-737,013	-92,438	-810,573	-362,982	-564,095	-70,753	392,152.3
	(5,372.591)	(4,854.827)	(5,233.027)	(4,729.539)	(3,702.130)	(3,371.198)	(3,969.870)	(3,615.531)	
Labor income (employed only)	22,099.375***	18,517.801**	21,320.719***	18,014.961**	15,278.938***	12,806.706**	15,922.137***	13,457.467**	1,002,191
	(7,901.163)	(7,310.931)	(7,895.760)	(7,337.148)	(5,146.135)	(5,018.309)	(5,823.355)	(5,680.249)	
N	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	2,377,658	-
F-test	13.5352	13.5456	13.5445	13.5484	49.1006	49.1183	41.1904	41.2051	-
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	-
Individual level controls	No	Yes	No	Yes	No	Yes	No	Yes	-
Polynomial of order 1 for decimal of final score	No	No	Yes	Yes	No	No	Yes	Yes	-

Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).
Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on indicators of hours worked and labor income. Reported coefficients correspond to the parameter β_1 in equation (3) in the main text. Standard errors are clustered at the department and year level, leading to the identification of 92 clusters. The definition of the outcomes of interest is available in supplementary online appendix [table S2.1](#). Panel A presents results obtained by using the actual change in the number of inspectors to construct the endogenous variable (constructed cumulatively over years, as described in the main text), while panel B presents results obtained by using the theoretical change in the number of inspectors to construct the endogenous variable (always constructed cumulatively over years). In both cases, the instrumental variable corresponds to the number of times a given department has been above the .5 cutoff (see the main text for details). The table also reports the mean in the outcome of interest below the cutoff: * <10 percent, ** <5 percent, *** <1 percent.

Figure 8 Treatment effects using regression discontinuity design (RDD). (A) indicators of labor law compliance, and (B) indicators of status in the labor market.



Source: Author's elaboration based on administrative data and the Gran encuesta integrada de hogares (GEIH).

Note: The figure plots point estimates and 90 percent confidence intervals for the effect of an additional inspector on different measures of labor-law compliance (panel A) and indicators of status in the labor market (panel B). Compared to the baseline results, point estimates plotted in this figure are obtained from an RDD specification (i.e., the instrumental variable and the treatment indicator are not constructed cumulatively over time). The regressions do not include the decimal part of the final score or any individual-level controls.

specification). These results on hours worked and labor income are in line with the effects of increased compliance with the labor legislation, as firms might be more likely to comply with the legislation over the maximum number of hours as well as the minimum wage legislation.

5.3. Robustness tests

In this section, I summarize the robustness tests. First, I want to check how much the results discussed above would differ from a standard fuzzy RDD (i.e., not constructing the treatment indicator and the instrument cumulatively over time). The results show that all coefficient estimates are of the same sign and very similar in magnitude to those presented in the baseline specification (see fig. 8, panel A for measures of labor-law compliance and panel B for indicators of status in the labor market).²⁴ They are also close to statistical significance despite the F -test being around 3, for the reasons discussed above in the paper. Similarly, I replicate my results but now restricting the bandwidth used. Indeed, even within the same integer value of the final score, departments with different levels of the decimal part of the score might still be different from each other, given that the remaining variation in final score might still be endogenous. However, this concern should decrease the closer I get to the .5 cutoff. I therefore restrict the analysis to departments that lie just on the $+/- 0.1$ bandwidth around the cutoff.²⁵ While the number of observations decreases substantially, the main results of the analysis are confirmed, especially in terms of the indicators of labor-law compliance (supplementary online appendix [table S2.4](#)).

Returning to my baseline specification, I note that the institutional framework that generates the exogenous variation in treatment intensity that I use for identification gives rise to some testable implications. Specifically, the paper exploits discontinuities in the allocation of inspectors occurring at .5 within each integer of the final score. This means that the relative strength of the exogenous change in treatment variation is higher for departments positioned at lower integer values of the final score. In order to exploit this variation, I modify the baseline specification by adding interaction terms between the treatment indicator and dummies for the integer part of the final score.²⁶ Supplementary online appendix fig. [S3.1](#) presents the results of this exercise for the measures of labor-law compliance. The coefficients plotted are those of the interaction terms, with the omitted category corresponding to departments with an integer value of 0 (i.e., those with the strongest increase in public enforcement in relative terms). As expected, I find smaller effects among departments with a smaller relative change in enforcement.

²⁴ While in theory I could present these results using the standard RDD graphical representation, I plot the simple point estimate to allow their comparability with the baseline findings presented above (see fig. 6).

²⁵ Note that the baseline analysis exploits the entire integer, so the bandwidth is of size equal to $+/- 0.5$.

²⁶ For ease of exposition and in order to have a more balanced number of observations across integers, I group together all departments whose integer of the final score is equal to or above 4.

A final set of robustness tests aims to check some miscellaneous aspects. First, I exclude all departments that, in a given hiring round, do not have another department that lies on the other side of the .5 cutoff within the same integer.²⁷ The number of observations decreases and the F -test is just above 10, leading to some estimates being less precisely estimated. However, point estimates are of a very similar magnitude and the decrease in informal employment in the formal economy remains statistically significant (supplementary online appendix [table S2.5](#)). Additionally, I cluster standard errors at the department level to account for the possible presence of serial correlation within departments over years. Point estimates lose precision given that the first stage becomes weak ([table S2.6](#)). I then replicate the results, but now using a probit instrumental variable model for the regressions where the outcome of interest is a binary indicator. Supplementary online appendix [table S2.7](#) reports coefficient estimates from these regressions, which confirm all the results discussed above.

6. Complementary evidence from enterprise survey data

The results presented above have shown that the increase in the number of labor inspectors has important effects on the functioning of formal firms. When visiting formal firms, labor inspectors aim to improve compliance with the labor code. This results in an increase in the provision of mandated benefits ([table 7](#)), as well as a decrease in the number of informally employed individuals ([table 9](#)). At the same time, the number of hours worked decreases while labor income increases ([table 10](#)). These changes are likely to be costly for formal firms. So a natural question is how do formal firms react?

To study this question, I exploit longitudinal data on formal enterprises in the manufacturing sector from the Annual Manufacturing Survey (EAM) by DANE. The survey contains detailed information on enterprises' workforce (e.g., number of individuals employed as well as contract types), as well as data on firms' labor costs. The EAM samples around 9,000 enterprises every year and follows them at the establishment level. The survey targets large formal firms in the manufacturing sector. These firms are not representative of the universe of enterprises in Colombia. For this reason, the purpose of this exercise is to provide complementary evidence on how *certain* enterprises adjust to the increase in labor-law compliance. Enterprises surveyed in the EAM are nevertheless particularly interesting to study in this context, as the results above have shown that large establishments in the formal sector are those most likely to be targeted by labor inspectors. The analysis adopts the same estimation strategy introduced in the previous section. Descriptive statistics for this sample are in supplementary online appendix [table S2.8](#).

[Table 11](#) presents the main results on enterprise-level costs. I find a small increase in the per capita cost of the salary of permanent staff. Even though I cannot measure individuals' monthly wages, this is consistent with the positive effect on labor income documented above. Other labor costs for permanent staff, as well as salary costs for temporary employees and social-security contributions, do not change. Instead, I find a decrease in the per capita labor costs associated with payroll contributions.²⁸ This is despite the fact that salaries for permanent staff have increased, which should lead to an increase in payroll contributions. Instead, the reduction in payroll contributions compensates for around 40 percent of the increase in costs for salaries. Although I am not able to explicitly examine what lies behind this result, it might represent an attempt by firms to compensate for the increase in costs generated by higher labor-law compliance (i.e., higher provision of mandated benefits and higher salaries) through tax avoidance or outright tax evasion. Indeed, there is evidence that these types of contributions are often evaded in the country ([Alm and Castano 2002](#); [Concha Llorente, Ramírez Jaramillo, and Acosta 2017](#)).²⁹ However, this adjustment is only partial and it is notable that formal employment does

²⁷ Note that the majority of departments had a valid counterpart on the other side of the cutoff. Specifically, this is true for 60 out of the 92 possible department*year combinations.

²⁸ In the EAM, this category includes contributions to the Colombian public training institution (the Servicio Nacional de Aprendizaje, SENA), the Colombian Institute of Family Welfare (Instituto Colombiano de Bienestar Familiar), as well as the Family Welfare Fund (Caja de Compensación Familiar).

²⁹ Note that it is possible that firms decide to engage more in tax avoidance, despite being subject to a higher level of labor-law enforcement. This is because labor inspectors are not responsible for verifying tax compliance, so that firms are not subject to higher enforcement along this dimension.

Table 11. Results at the enterprise level

	(1)	(2)
Salary for permanent staff	108.9** (48.74)	119.8** (53.78)
Other labor costs for permanent staff	18.33 (18.80)	19.37 (21.18)
Salary and other costs for temporary staff	−60.46 (306.3)	−43.61 (384.1)
Social-security contributions	−77.42 (65.53)	−89.68 (78.50)
Payroll contributions	−44.66** (19.29)	−48.23** (23.07)
Other voluntary contributions	1.788** (0.885)	1.737* (1.012)
<i>N</i>	37,911	37,911
Integer of the final score FE	Yes	Yes
Polynomial of order 1 for decimal of final score	No	Yes

Source: Author's elaboration based on administrative data and the Encuesta anual manufacturera (EAM).

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on firm-level outcomes using data from the EAM. Reported coefficients correspond to the parameter β_1 in equation (3) in the main text. Standard errors are clustered at the enterprise level and all specifications also include fixed effects at the enterprise level. The two columns differ for whether the specification includes a control for the decimal part of the final score. All the regressions are conducted using the identification strategy presented above. The endogenous variable is constructed using the actual change in the number of inspectors (constructed cumulatively over years). With respect to the reported variables, (a) salary for permanent staff includes all the monetary and non-monetary components of the remuneration, except for any transport subsidy, (b) other labor costs for permanent staff includes costs for leave days, firing costs, and other social benefits, (c) salary and other costs for temporary staff includes cost categories in (a) and (b) above, but for temporary employees, (d) social-security contributions include mandatory contributions for health and pension, (e) payroll contributions include other types of mandatory contributions (e.g., employment services, family compensation funds; see the main text for details), (f) other voluntary contributions include non-mandatory contributions of a different nature. * <10 percent, ** <5 percent, *** <1 percent.

not decrease (see results from the main analysis) despite labor costs having increased. I interpret this as evidence of the fact that formal firms in a largely informal labor market enjoy monopsony power, in line with the predictions of some of the models on informality (Meghir, Narita, and Robin 2015; Ulyssea 2018).

7. Conclusions

The paper analyzes the effects of a reform of the system of labor inspection in Colombia, which was requested by the US as part of the negotiations for a bilateral trade agreement. I find that an additional labor inspector increases worker's probability of having an employment contract and receiving mandated benefits, including social-security coverage for pension. Effects are similar for men and women, low- and high-educated individuals, as well as for individuals living in urban and rural areas. However, they materialize only in the formal sector of the economy. I also document that informal employment in the formal sector decreases, as inspectors force firms to register workers that were previously kept off the books. These changes are not associated with a decline in overall employment, while hours worked decrease and labor income increases. Complementary evidence from an enterprise survey shows that formal sector firms are able to partially compensate the increase in labor costs by reducing their payroll contributions.

The paper contributes to a debate on the reasons that drive firms to operate in the formal or informal sectors of the economy. Traditional models of informality imply that the informal sector expands the set of available opportunities by creating jobs that would not exist otherwise. Accordingly, some

of the previous empirical studies on labor inspection found that stricter public increases reduce overall employment (Almeida and Carneiro 2009, 2012). Alternative models have found that a large share of informal firms deliberately decide to operate in the informal sector in order to reduce labor costs and increase profits. In these contexts, increasing the strength of labor inspection does not necessarily generate an increase in unemployment. My findings provide evidence in support of this second class of models. Specifically, I do not observe a decline in employment following the increase in labor costs. This is also in line with evidence showing that formal firms have monopsony power when they operate in largely informal labor markets.

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Data availability statement

The administrative data used in the paper has been provided by the Colombian Ministry of Labor and cannot be shared with third parties for confidentiality reasons. The code required to replicate the results of the paper is available upon request.

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