

Compliance with Labour Legislation in Informal Labour Markets*

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Abstract

I study how strengthening labour law enforcement affects compliance with the legislation and the changes generated to the equilibrium in the labour market. Identification relies on discontinuities in the allocation of labour inspectors across Colombian departments. I find that stricter enforcement increases compliance with the legislation and the provision of mandated benefits, but this effect takes place only in the formal sector of the economy. There is also an increase in the probability of being formally employed. This partially comes from the formalization of workers who were already employed in the formal sector but were kept off-the-books by their employers (i.e. intensive margin), but it is largely explained by a shift in labour supply towards the formal sector of the economy to take advantage of the higher provision of mandated benefits (i.e. extensive margin). Despite the increase in labour costs associated with these changes, overall employment levels do not decrease and wages adjust in the formal sector only in labour markets characterised by low formality rates.

Keywords: Labour Legislation, Informal Economy, Enforcement, Compliance

JEL Codes: H10, J38, J58, K31

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1 Introduction

The majority of employment in low- and middle-income economies occurs in the informal sector, where workers are not covered by the labour law (e.g. minimum wages and working hours) and do not enjoy the provision of mandated benefits (e.g. social security and paid leave) (Bhorat et al., 2019; ILO, 2018). Magnac (1991) presents two interpretations of informality. According to the first view, the presence of a large informal sector is the result of a survival strategy adopted by unproductive enterprises that would not be able to operate if they had to face all the regulatory costs imposed by the legislation. Alternatively, self-selection into the informal sector reflects comparative advantages as in a Roy (1951) model and in equilibrium wages will reflect differences in amenities and workers will be indifferent between sectors. Despite presenting slightly different interpretations, both views imply that informality improves welfare by increasing the set of available jobs in the economy. In line with these models, some empirical studies have shown how reducing the size of the informal sector through stricter public enforcement can lead to a reduction in overall employment levels (Almeida and Carneiro, 2009, 2012).

These interpretations of informality are not able to fully explain some of the patterns observed in the data. Indeed, a number of 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 sector (either directly or via unemployment) as well as the presence of informal employment in both formal and informal enterprises (Bosch and Maloney, 2010; Botelho and Ponczek, 2011; Porta and Shleifer, 2008, 2014). Alternative models have therefore been proposed, which explain informality as the result of the presence of search frictions in the labour market (Meghir et al., 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 of the economy do so by choice rather than need. This generates a subsidy towards smaller and less productive enterprises that evade taxation. As a result, strengthening public enforcement can theoretically increase employment and productivity by reducing the size of the informal sector.

I provide empirical evidence supporting this second class of models by studying the effects of an increase in labour law enforcement 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 internationally (OECD, 2017).¹ In the labour 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). In an attempt to strengthen labour law compliance, the US Administration requested to the Colombian government to more than doubling the number of labour inspectors operating in the country within four years (i.e. between 2011 and 2014).² The request was advanced as a requirement for the approval by the US Congress of the “US-Colombia Trade Promotion Agreement”, which was being negotiated between the two countries at that time.

As a result of the policy change, the number of labour inspectors operating in Colombia increased from two to four per 100,000 individuals between 2010 and 2015. This increase contrasted with the lack of new hires in previous years and it brought Colombia in line with the levels of coverage of labour inspection registered in other countries in the region. The assignment rule used to distribute the new inspectors generated exogenous variations in the strength of law enforcement that I exploit for identification. In particular, the agreement between Colombia and the US established that the new inspectors should have been hired during four consecutive hiring rounds taking place between 2011 and 2014. The Colombian Ministry of Labour distributed the vacancies of inspectors across departments following an allocation rule that was elaborated at the central level, changed every year and was meant to capture the risk of non-compliance with the labour legislation (i.e. assigning more inspectors to high-risk areas). This rule presented discontinuities which generated variations in the number of inspectors allocated across otherwise comparable departments within each hiring round.

The data used in the analysis corresponds to repeated cross sections of the Colombian household survey matched with administrative data on labour inspection obtained from the Ministry of Labour. I find that an additional labour inspector in a given department raises workers’ probability to receive a number of job-related benefits (e.g. health and pension coverage, severance payments) and to have a written employment contract, while it reduces the probability of working an excessive number of hours (i.e. above 48 hours per week). Effects are similar for women and men as well as for individuals living in urban and rural areas. However, the increase in law compliance materialises only in the formal sector of the economy (i.e. registered enterprises). This asymmetric effect of labour inspection between the formal and the informal sector was generally assumed in previous empirical and theoretical studies, but never actually

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

²Labour inspectors are public officials in charge of monitoring compliance with the labour legislation and imposing sanctions to firms in case of violations. See Section 2.2 for details.

shown. It can be explained by the fact that inspectors only target registered enterprises that are easier to find (i.e. from administrative records) and from where complaints from workers are more likely to arrive (i.e. with less fear of being dismissed by the employer).

This asymmetric response to increased enforcement between the formal and the informal sector has important implications in terms of the labour market adjustment that occurs in the economy. In particular, I find an increase in the probability of being formally employed (i.e. registered worker). This partially results from a decrease in the probability of being employed off-the-books in the formal sector. I call this the “intensive” margin of formalization and interpret it as the direct effect of the increase in public enforcement, as firms in the formal sector are in practice the only ones subject to labour inspections. However, this phenomenon cannot explain the overall increase in formal employment that I observe in the data. Rather, a more important role is played by the “extensive” margin of formalization. This corresponds to individuals that move to the formal sector of the economy (from either informal employment or non-employment) and directly obtain a formal job. This represents an indirect effect of stricter enforcement and it can be explained by the fact that formal firms have become more attractive to workers, given the increased provision of mandated benefits.

The positive effect on formal employment has been already documented in some of the previous contributions ([Almeida and Carneiro, 2009, 2012](#)). However, compared to these studies I do not find any negative effect on overall employment levels. Rather, indicators for labour market status (i.e. employment, unemployment and inactivity) are not affected and – if anything – register a slight improvement following the increase in public enforcement. Even in the informal sector, employment levels do not decrease even though the share of informal employment in the overall economy decreases. I provide some suggestive evidence to interpret these results. First, I observe that average wages adjust in response to the increase in public enforcement. In particular, wages decrease in the formal sector to compensate for the higher provision of mandated benefits and they increase in the informal sector in a compensating differential fashion. Additionally, I find that wage decreases in the formal sector are stronger in labour markets characterized by initially lower levels of labour market formalization. I present a simple theoretical framework to rationalize these findings.

The paper contributes to a debate at the intersection between empirical studies on formalization policies and theoretical contributions on the nature of the informal sector. First, the micro-economic literature on formalization policies includes studies on the effects of tax reductions ([Fajnzylber et al., 2011](#); [Monteiro and Assuncao, 2012](#)), the simplification of bureaucratic procedures ([Bruhn, 2011](#); [de Mel et al., 2013](#); [Kaplan et al., 2011](#); [Rocha et al., 2018](#)) and

the increase in government auditing. This latter group of studies is the closest to the present contribution and Appendix A reviews them in details. However, previous analyses have looked separately at the effects of public enforcement on either the provision of mandated benefits (Bhorat et al., 2012; de Andrade et al., 2016; Gindling et al., 2015; Ronconi, 2010) or the distribution of employment and wages between the formal and the informal sector (Almeida and Carneiro, 2012). The present study is the first one that brings together these two dimensions. This is particularly important, given that I find an asymmetric effect on compliance between sectors of the economy as well as larger effects on formalization on the extensive than the intensive margin.

In this sense, the paper also relates to an applied theoretical literature that examines the determinants of workers' and firms' decision to operate in the formal or the informal sector. Meghir et al. (2015) develop a model in which search frictions allow formal and informal firms of equal productivity to coexist, since, in an economy populated uniquely by formal enterprises, a specific firm could deviate and move to the informal sector to avoid the cost of regulation without the need of fully compensating its workers for the loss in job-related benefits. The present study also adopts the framework of Ulyssea (2018), where informal employment can occur in either the formal or the informal sector (i.e. registered or unregistered enterprises) and heterogeneous enterprises self-select into different sectors based on the regulatory framework that they face. Both models are estimated using Brazilian data and find that reducing informality via stronger enforcement can increase output and productivity, without necessarily leading to an increase in unemployment. I see the present contribution as complementary to these papers, whose results rely on the structural estimation of the parameters of interest.

The paper thus aims to bridge the gap between these two branches of the literature by interpreting the results on the effects of public enforcement in light of the debate on the nature and composition of the informal sector. Using high-quality data on both the level of public enforcement and the risk of non-compliance with the legislation and adopting a novel identification strategy that relies on a natural experiment, I find that formal employment increases despite hiring formal workers has become more expensive and that overall employment levels do not decrease. In this way, the paper presents the first fully empirical contribution on the role of public enforcement which is supportive of the implications of recent theoretical studies on the nature of the informal sector. The rest of the paper is organised as follows: section 2 describes the policy change and the work conducted by labour inspectors; section 3 introduces the data sources, definitions and descriptive statistics; section 4 discusses the identification strategy and the specification used; section 5 presents the empirical findings and robustness tests; section 6 introduces a simple theoretical framework to interpret the results; section 7 concludes.

2 Policy and institutional setting

2.1 Action Plan

The paper exploits a reform of the system of labour inspection in Colombia, which generated a strong increase in the strength of labour law enforcement in all the country, with exogenous variations taking place across departments and years. This reform is particularly suitable for being used in empirical research because of its drastic scale and external origin. In particular, 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 strategic importance of the CTPA for Colombia can hardly be overstated: the US was the country’s main trade partners, accounting for 35 per cent of the country’s balance of trade in 2007. On the other hand, Colombia accounted for only around one per cent of US imports and exports and was its 26th trading partner worldwide ([OECD, 2019](#)). This generated an asymmetry in the importance of the agreement for the two countries, which, combined with pre-existing traditions of geopolitical influence, resulted in a substantial imbalance in bargaining power during the negotiations.

The ratification process required the approval of the trade agreement by the parliaments of both countries. Given the strategic importance of the agreement for Colombia, the country’s Congress approved the bill already in June 2007 with a large majority (85 “yes” and ten “no” in the House Floor and 55 “yes” and three “no” in the Senate Floor, during votes taking place a few days apart). In April 2007, the agreement was also sent for approval to the US Congress. However and despite the US Administration support for the CTPA, the debate never led to a vote amid concerns raised by the Congress over violations of labour rights in Colombia. There is evidence that partisan considerations played a role in shaping the debate over the CTPA. In particular, since January 2007 the US had a divided Government with a republican Administration and a democratic majority in the Congress. A legislative rule change to indefinitely delay action on the agreement was in the end approved by the Congress, after which the US Administration committed to include “worker protections in several pending trade accords” ([Weisman, 2007](#)).³ As a result, the CTPA was not passed by the end of the legislative session in December 2008 ([OAS, 2019](#)).

The newly elected US Administration immediately specified its opposition to the approval of the CTPA in its current form and requested additional guarantees with respect to

³This was meant to apply to trade agreements with Colombia, Panama, Peru and South Korea.

labour law compliance.⁴ This request can be connected to the increasing policy trend of including labour provisions in trade agreements ([ILO, 2016](#)) and it was specifically motivated by concerns over the low coverage of labour inspection 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 labour legislation in the country (the “Colombian Action Plan Related to Labour Rights”, or Action Plan) to be implemented by the Colombian government in collaboration with the ILO ([OAS, 2019](#)). After the Action Plan was agreed, the US Congress approved the CTPA in October 2011 and the trade agreement went into force in May 2012.

The Action Plan established that the Colombian Ministry of Labour 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).⁵ 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. A part from creating new vacancies, the government committed to fill the vacancies of inspectors 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 operating in Colombia increased from 353 in 2010 to 771 in 2015.⁶ This was well below the hiring target of 904 inspectors (see below for information on the policy implementation delays), but it nevertheless constituted a substantial improvement in the strength of labour law enforcement in the country. In particular, the Action Plan increased the coverage of labour inspection in Colombia from two to four inspectors per 100,000 individuals. This is in line with the levels registered in other countries in the region such as Argentina (four inspectors per 100,000 individuals) as well as Brazil, Ecuador and Peru (three inspectors per 100,000 individuals) and it significantly reduced the gap with many high-income economies (Figure 1).

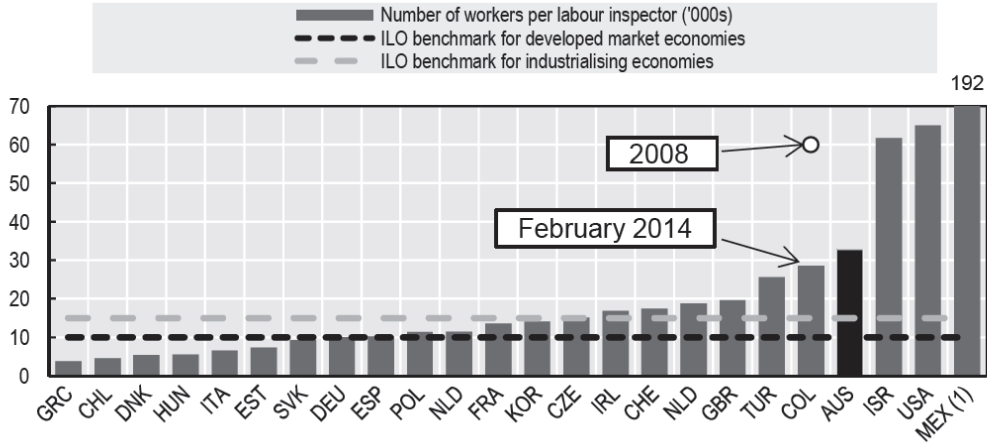
The legislative decrees implementing the Action Plan established that the newly hired inspectors should have been allocated across departments “taking into account the structure, plans, programmes and necessities of the Ministry” ([Government of Colombia, 2011, 2012, 2013](#)). In practice, vacancies 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

⁴In June 2009, the US President specified there was not a “strict timetable” to the agreement amid controversies for the violation of labour rights in Colombia. Additionally, the major US trade union federation was also opposed to the CTPA. This led to delays in the beginning of the new bargaining round.

⁵Other than the hiring of new inspectors, the Action Plan established the creation of the Ministry of Labour (previously a division of the Ministry of Social Security) and it reinforced the legislation over anti-union behaviour and the misuse of cooperatives.

⁶The increase was not constant over time, but it was rather 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: Number of workers per labour inspector, selected countries (per 100,000 workers)



Source: OECD (2016)

Note: The graph shows the number of 100,000 workers per labour inspector in selected OECD countries and Colombia. For Colombia, it also shows the evolution of the indicator between 2008 and 2014. The two horizontal lines represent the benchmarks set by the ILO in terms of coverage of labour inspectors for developed market and industrialising economies.

identified by the Ministry of Labour 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 all other years is available in Appendix B.⁷ During the first hiring round, the allocation rule resulted from 18 different indicators belonging to five different thematic clusters (Figure 2). First, a set of indicators captured administrative actions (i.e. consultations, visits and investigations) conducted by labour 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, ports, oil palm and ports) that were defined by the Action Plan as priority areas for labour inspection given the high perceived 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, given that there was evidence of the misuse of these types of enterprises in order to reduce labour costs. 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 ($Score_{s,t}$ in the equation below) was equal to the sum of the values obtained in each of the different indicators entering the allocation rule (from $Ind_{1,s,t}$ to $Ind_{n,s,t}$), weighted to reflect the relative importance of each indicator (with weights from $w_{1,t}$ to $w_{n,t}$, constant across departments but potentially changing over years) and divided by the scores obtained over these indicators by all departments in that same hiring round. The

⁷The same set of indicators was used for the 2013 and 2014 hiring rounds. However, the final score assigned for the different departments nevertheless differed even between these two hiring rounds since a different number of vacancies were kept at the central level and not distributed across departments. See Appendix B for details.

Figure 2: Criteria determining the final allocation rule for 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 ECONÓMICOS - 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: Colombian Ministry of Labour.

Note: The figure reports the indicators that entered the allocation rule in the first hiring round conducted as part 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 from where the information was collected.

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 normalised score and it can be decomposed into an integer part ($Integ_{s,t}$) and a decimal component ($Dec_{s,t}$).

$$Score_{st} = \left\{ \frac{w_{1t}Ind_{1s,t} + w_{2t}Ind_{2s,t} + \dots + w_{nt}Ind_{ns,t}}{\sum_{s=1}^S [w_{1t}Ind_{1s,t} + w_{2t}Ind_{2s,t} + \dots + w_{nt}Ind_{ns,t}]} \right\} * Tot_t = Integ_{s,t} + Dec_{s,t} \quad (1)$$

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 case the decimal part of the final score was greater than or equal to .5.

$$Target_{st} = \begin{cases} Integ_{s,t} & \text{if } Dec_{s,t} < 0.5 \\ Integ_{s,t} + 1 & \text{if } Dec_{s,t} \geq 0.5 \end{cases} \quad (2)$$

The rest of the hiring process then took place directly at the department level. This means that the Ministry of Labour was only responsible for setting the hiring target at the national level and distributing the vacancies (but not the inspectors) across departments following the allocation rule presented above. Labour offices in the different departments were then in charge of completing the hiring process (i.e. posting vacancies, selecting candidates) and training the newly hired inspectors before they could take office. The appointment of new

inspectors should have been made in the same year in which the hiring round took place. This decentralised implementation of the hiring procedure has important implications for the present analysis, since the hiring process was subject to large implementation delays. In particular, the Action Plan established a target of 904 inspectors for 2014 while only 637 inspectors were actually working in that year. Given the functioning of the policy as described above, these delays were not uniform over the country but rather varied across departments. In particular, hiring targets were met only in 21.74 per cent of the cases (i.e. counting each department and year as a separate observation) and only two departments out of 23 in the analysis had met their final hiring target as set by the Action Plan by the 2014 deadline. While it is difficult to trace the exact causes of these delays, they have been related to shortages of human resource capacities (which are particularly acute at the local level) as well as the limited attractiveness of the compensation package compared to the professional requirements for the position (see Section 2.2 for details) ([OECD, 2016](#)).

Table 1 presents data on labour 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. Additionally, the average of the decimal part of the normalised 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. This provides suggestive evidence for the fact that departments did not select themselves on either side of the cutoff (see Section 4.1 for additional details on this). As a result of differences in both hiring targets and implementation delays, there is a substantial difference across departments in the total increase in the number of inspectors between 2010 and 2015. While at the national level the number of inspectors per 100,000 individuals roughly doubled during this time (i.e. from two to four inspectors, as mentioned above), the rate of increase ranged between 40 and 240 per cent across Colombian departments (Figure 3).

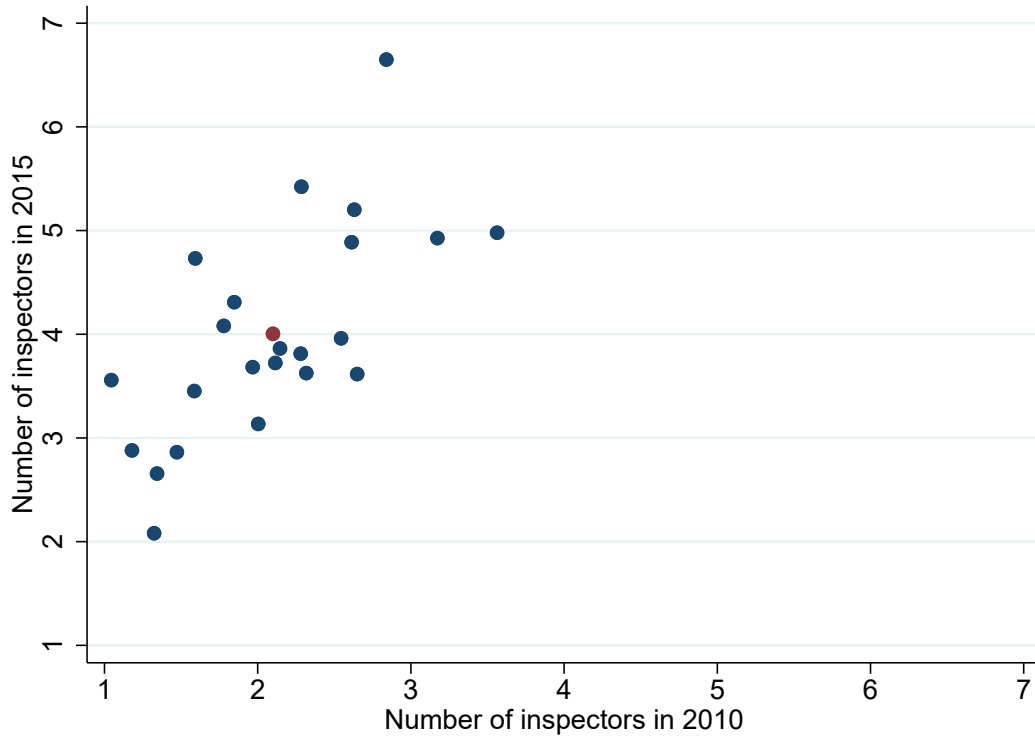
2.2 Labour inspectors

Labour inspectors are officials of the Ministry of Labour who are in charge of guaranteeing the respect of the labour legislation and collective bargaining rights. They are organized

Table 1: Descriptive statistics at the department level for policy variables

	mean	s.d.		mean	s.d.
Labour 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
N	92			92	

Note: The table reports means and standard deviations for selected policy variables computed at the department level between 2011 and 2014, unless otherwise specified. The difference between the allocation of inspectors and the actual change in the number of inspectors is related to the presence of policy implementation delays. See Section 2.1 for details.

Figure 3: Number of inspectors per 100,000 individuals in 2010 and 2015, national average and by department

Note: The graph shows the number of labour 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), while the red dot captures the unweighted national average.

around territorial directorates (corresponding to the Colombian departments), while there are 154 municipal labour offices around the country. Depending on the profile that is sought-after, candidates need to have an under-graduate degree in either law, medicine, engineering, business administration or economics and a post-graduate 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. Despite the relatively high professional requirements, working conditions are not necessarily at-

tractive. 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 extra compensation for inspectors operating in areas characterised by high violence rates. Acknowledging that low wages were preventing the hiring of highly qualified candidates, salaries for inspectors were raised in 2014 to reach between four to six times the minimum wage (OECD, 2016). Additionally, only a small share of inspectors are hired as career civil servants (15% of those operating in 2014). Recognizing that this might hinder their impartiality and independence, in 2008 the Constitutional Court ruled that inspectors can be fired only for disciplinary reasons (e.g. unsuitability, bad performance) or if they are replaced by a candidate that passed the career exam. Finally, limited public resources affect the way in which inspectors operate in Colombia. For instance, they 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, labour inspectors follow ILO guidelines as summarised in a manual of the Ministry of Labour (Government of Colombia, 2016). They have the authority to verify compliance with the labour code in general; including minimum wage, social security contributions, working hours, leave days, employment contract and collective bargaining rights. However, priority areas have been identified for labour formalization and labour intermediation. Similarly, specific sectors (or types of enterprises) can be prioritised. Inspectors can start an enquiry either independently or by responding to complaints by workers.⁸ Workers can file complaints (anonymously or otherwise) online, by telephone or directly at the labour office. Once inspectors decide to start an enquiry, they open a preliminary investigation to verify the presence of valid grounds. Based on this preliminary assessment, the case can either be archived or transformed into a formal investigation. The formal investigation terminates with an intervention which can take different forms: inspectors can either (i) conduct a preventive and informative function (e.g. inform the employer about a breach in the legislation), (ii) act as mediators between the employer and the worker (e.g. help defining an agreement in case of collective dismissals), (iii) implement initiatives to remove obstacles to the respect of the labour code (e.g. favour the development of a trade union), or (iv) impose fines and sanctions. Sanctions can be of financial nature (from one to 5,000 times the minimum wage) and/or involve the temporary closure of the enterprise (from three to ten days, up to 30 days in case of recidivism) or its immediate shutdown in case of health or security risks.⁹

⁸There is no available breakdown of the number of enquiries started autonomously or as a result of complaints.

⁹This wide set of interventions is in line with the new guidelines for labour inspection promoted by the ILO, which aim to move away from pure sanctioning strategies towards a strategic compliance model (ILO, 2017).

3 Data, definitions and descriptive statistics

3.1 Data sources

The present 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 it interviews every year 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 Labour.¹⁰ For each department and year between 2011 and 2014, this includes data on the allocation rule (both the overall final score and the different indicators that compose it) from which I obtain information on the number of inspectors that should have been hired in a given department and year following the Action Plan. 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), from which I compute the yearly actual change in the number of inspectors in a given department. In this way, I have access to detailed information over time on both the level of public enforcement in a given department (i.e. as proxied by the number of inspectors) and the perceived risk of non-compliance with the legislation (i.e. as measured by the final score).¹¹

3.2 Outcomes of interest

The aim of the analysis is to examine the effects of strengthening labour law enforcement on indicators of law compliance and to observe the resulting implications in terms of labour market adjustment between the formal and informal sectors. For this reason, the analysis focuses on two main clusters of outcomes of interest.

The first cluster includes measures of labour law compliance and the provision of mandated benefits, in order to examine whether labour inspection has any effect on its main stated

¹⁰Some remarks concern the matching process between the administrative and survey data. The Ministry of Labour 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. less than 5 per cent of the Colombian population)

¹¹See Appendix A for alternative approaches to measure public enforcement.

objective. Previous studies looked at either specific indicators of law compliance such as minimum wage legislation in [Bhorat et al. \(2012\)](#) or business registration in [de Andrade et al. \(2016\)](#), or rather examined a number of measures of law compliance at the same time ([Ronconi, 2010](#); [Gindling et al., 2015](#)). In the present study, I look at several measures based on regulations set in the Colombian legislation. In particular, outcomes of interest include indicators such as having a written employment contract, being entitled to a series of mandated benefits (e.g. severance payments, paid annual leave, maternity leave), working an excessive number of hours and being covered by social security for pension and health (i.e. with the employer contributing partially or fully to the funds).¹² This approach 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).¹³

The second cluster of outcome variables is meant to study changes to the equilibrium in the labour market arising from the increase in enforcement levels. Indeed, if stricter enforcement leads to a change in employment conditions, this might affect overall labour demand and supply as well as the distribution of employment between the formal and informal sectors ([Almeida and Carneiro, 2012](#)). For this reason, outcomes of interest in this cluster include dummy variables for labour market status (i.e. employment, unemployment and inactivity). Additionally and following ILO definitions, I look at whether the employment relationship is formal (i.e. formal employment, corresponding to a registered worker) and whether the individual works in a formal enterprise (i.e. formal sector or economy, corresponding to a registered enterprise).¹⁴ The intersection between these two variables gives rise to four possible employment statuses ([Ulyssea, 2018](#)): formal worker in formal enterprise, formal worker in informal enterprise, informal worker in formal enterprise and informal worker in informal enterprise.¹⁵ I will also look at the effects on the number of hours worked weekly and on monthly labour income, both overall and in the main job. A detailed description of the outcomes of interest is available in Table F1.

¹²Unfortunately, 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.

¹³These are all self-reported measures of labour law compliance and there is a risk of mis- or under-reporting leading to measurement error. Following [Ronconi \(2010\)](#), these indicators are better obtained from household surveys (compared to enterprise surveys, as employers might have a higher interest in under-reporting 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 labour legislation reported in the data, lead to some confidence in the quality of the information ([Ulyssea, 2018](#)).

¹⁴While the definitions of formal employment and formal economy vary by employment status (see Table F1 for details), employees are considered in formal employment if the employer contributes at least partially to their social security coverage while they are considered working in the formal economy if the condition above is met or alternatively if they work in a fixed place that employs at least five individuals.

¹⁵In 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.

3.3 Descriptive statistics

In the present 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 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. The statistics show that only a minority of employed individuals works in the formal sector of the economy (i.e. corresponding to 23.3 per cent of the entire sample, or less than 40 per cent of the employed population) and is formally employed (i.e. only 20.9 per cent of the sample, or around one third of the employed population).¹⁶ 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 per cent of the overall sample, or around 10 per cent 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 per cent of informal employment takes place in the formal economy. This differs from the situation in Brazil ([Ulyssea, 2018](#)) and Mexico ([de la Parra, 2017](#)), where almost 50 per cent of informal workers are hired in formal enterprises.

Turning to the provision of mandated benefits and compliance with the legislation, less than 19 per cent 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 per cent of the entire sample (i.e. corresponding to less than 30 per cent of the employed population), while around 19 per cent of the sample works more than the maximum allowed number of hours (i.e. 48 per week) and less than 10 per cent 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 labour (in 2015 values) of 495,292.5 Colombian Pesos (corresponding to around 120 USD, not adjusted for PPP). Among 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).

¹⁶Here and in the rest of the analysis, variables related to employment characteristics (e.g. formal employment, job benefits) are set to zero rather than missing for individuals who are not in employment in order to avoid problems of sample selection in the absence of a plausible exclusion restriction.

Table 2: Descriptive statistics at the individual level for personal and labour 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, hours worked and wages		
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 labour 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	Parental leave	0.095	0.293
Status in employment			Severance pay	0.169	0.375
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	397029.700	900587.300
Formal employment in the informal economy	0.002	0.042	Overall labour income	495292.500	1064687.000
N	2,377,658			2,377,658	

Note: The table reports means and standard deviations for selected personal and labour 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.

4 Identification and estimation

4.1 Identification

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 in terms of the equilibrium in the labour 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 from the literature on the direction of the bias (Ronconi, 2010; Almeida and Carneiro, 2012). In particular, governments might want to concentrate enforcement efforts in areas characterised by a higher risk of non-compliance. This is explicitly the case for the Action Plan, as inspectors were distributed across departments according to the final score, which was meant to capture the risk of non-compliance with the legislation in a given department. At the same time, enforcement systems might be more effective in areas characterised by better socio-economic and institutional conditions that are themselves conducive to compliance. Depending on the relative importance of each of these dynamics, simple ordinary least squares (OLS) estimates would respectively be either downward or upward biased. Previous empirical studies on the topic have mostly adopted an instrumental variable (IV) approach to overcome this identification problem, with instruments ranging from election years to travel distance between the enterprise and the labour office (see Appendix A for details).

In this paper, I exploit discontinuities in the allocation rule used to distribute new vacancies of inspectors across departments as part of the Action Plan. In particular, a higher final score generally resulted into more inspectors being allocated to a department. However, the relationship between the normalised 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 (see Equation 2). Given that most departments presented relatively low values of the final score, this discontinuity generates variations in treatment intensity over time and space that are sizeable relative to the overall policy change. This corresponds to the standard setting of a regression discontinuity design (RDD), with the intuition being that departments on the two sides of the cutoff within the same integer of the normalised score are characterised by a similar risk of non-compliance with the legislation but different levels of law enforcement simply due to policy design. I will compare departments that had the same integer value of the normalised score and differed only with respect to its decimal part, determining their positioning around the .5 cutoff. The validity of this approach rests on the accuracy of the allocation rule in capturing the risk of non-compliance with the legislation as well as the exogeneity of the .5 cutoff with respect to that risk.

I slightly depart from this standard RDD setting by constructing both the treatment indicator (i.e. yearly change in the number of inspectors) and the dummy for being above the .5 cutoff cumulatively over time from 2011 to 2014. This means that for department s in year t , the treatment indicator measuring the cumulative change in the number of inspectors will take the following form:

$$\sum Insp_{s,t} = \Delta Insp_{s,t} + \Delta Insp_{s,t-1} + \dots + \Delta Insp_{s,2011} \quad (3)$$

Where $\Delta Insp_{s,t}$ is the yearly change in the number of inspectors taking place in a given department (i.e. compared to the previous year), while the cumulative version ($\sum Insp_{s,t}$) takes into account also the changes in the number of inspectors having taken place in the same department in all years from 2011 to t . In a similar fashion, the dummy for being above the .5 cutoff is constructed cumulatively over time as follows:

$$\sum Above_{s,t} = Above_{s,t} + Above_{s,t-1} + \dots + Above_{s,2011} \quad (4)$$

Where $Above_{s,t}$ is a dummy indicator taking the value of one 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 ($\sum Above_{s,t}$) takes into account the value of this dummy computed for the same department in all the previous years. By construction, the contemporaneous and cumulative versions of these variables will be equivalent in the first hiring round (i.e. in 2011).

These changes are introduced in order to obtain a first stage relationship which is

strong enough for identification. This is best explained in the context of a simple regression framework. In theory, being above the cutoff within the same integer of the final score should translate into an additional inspector being hired in that same year. Table 3 confirms how being above the cutoff is associated with a positive change in the number of inspectors, with a coefficient point estimate of 1.2 in the specification controlling for dummies for the integer part of the final score (column 2 of Table 3). At the same time, the table also shows how the relationship is not statistically significant at conventional levels (i.e. the coefficient in column 2 in Table 3 is significant at the 11 per cent). A similar picture emerges from Figure E1 in Appendix E, where it is possible to detect a jump of roughly one inspector around the cutoff, which is however only imprecisely estimated.

Failure to precisely identify a change in treatment variation around the cutoff can be connected to two different reasons, related to problems of sample size and policy implementation delays. First, treatment varies at the department and year level and this leads to the identification of only few units of treatment variation on the two sides of the cutoff (i.e. 92 observations over the entire bandwidth, corresponding to 23 departments over four hiring rounds). Secondly, the implementation of the policy was subject to large delays and this makes the relationship between the allocation of labour inspectors and their actual change in a given department and year particularly blurred (see Section 2.1 for details).¹⁷ In order to account for these issues, I transform both the treatment indicator (i.e. yearly change in the number of inspectors in a given department) and the dummy for being above the .5 cutoff cumulatively over time as detailed above. As a result of these transformations, the relationship between the variables of interest becomes statistically significant (column 4 of Table 3). However, its interpretation partially changes. In the preferred specification, having been above the .5 cutoff once more over subsequent hiring rounds leads to 1.4 inspectors more working in a given department and year.¹⁸

Constructing these variables cumulatively over time leaves the RDD intuition behind the estimation strategy unchanged, but it might impose additional assumptions concerning the reasons behind these policy implementation delays as well as their plausible exogeneity with respect to the outcomes of interest. Identification of a causal effect in the present context will then rely on a series of identification assumptions, some of which are common to the RDD literature (Imbens and Lemieux, 2008; Lee and Lemieux, 2010) while additional ones arise from the fact that I construct the main RDD variables cumulatively over time (i.e. rather than using

¹⁷These two problems also interact, as few observations are spread around multiple cutoffs (i.e. within different integers) with implementation delays varying proportionally with the size of the final score.

¹⁸This is slightly above what would be theoretically predicted (i.e. being above the cutoff once more should result into an additional inspector). This can be explained by differences in policy implementation delays, as departments above the cutoff have a slightly lower policy implementation delay.

Table 3: Relation between the change in the number of inspectors and the dummy for being above the .5 cutoff, using contemporaneous values or compounding over years

	Contemporaneous values		Compounding over years	
Above the .5 cutoff	1.487 (1.143)	1.193 (0.731)		
Above the .5 cutoff			1.592 (1.127)	1.404*** (0.433)
Integer dummies	No	Yes	No	Yes
N	92	92	92	92

Note: The table reports coefficient estimates and standard errors of different regressions run at the department and year level. The dependent variable is the change in the number of inspectors in a given department and year and the regressor of interest is a dummy for whether the department was above the .5 cutoff in the hiring round taking place in that year. Columns 1 and 2 present regressions using these variables with their contemporaneous values, while columns 3 and 4 construct them cumulatively over time as explained in the main text. Regressions also differ on whether dummies for the integer part of the final score are included. * $<10\%$, ** $<5\%$, *** $<1\%$.

their contemporaneous values). The continuation of this subsection discusses the plausibility of these assumptions, while the next subsection presents the empirical specification that I use in the analysis. In the robustness test subsection, I will also check how the results that I obtain using this modified identification differ from those that would result from a standard fuzzy RDD (which would however suffer from a weak instrument problem).

First, identification rests on the fact that departments could not manipulate their values of the normalised 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 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, in the present context there is both qualitative and descriptive evidence supporting the idea that manipulation of the allocation rule is unlikely to have happened.

In particular, the different indicators were announced by the central government through ministerial decree and there is no evidence that departments were involved by the Ministry of Labour in the definition of the composition 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 the definition of an international agreement. Additionally, it is also implausible that departments could manipulate the information that was used to compute the different indicators that entered in the allocation rule. Indeed, these indicators were based on official statistics collected at the national level (from DANE or the Ministry of Labour) and they referred to objective values of these variables in previous years. Finally, the allocation of inspectors for a given department and year resulted from the department relative ranking at the national level (i.e. the final score was normalised to sum to 100 across departments). Self-

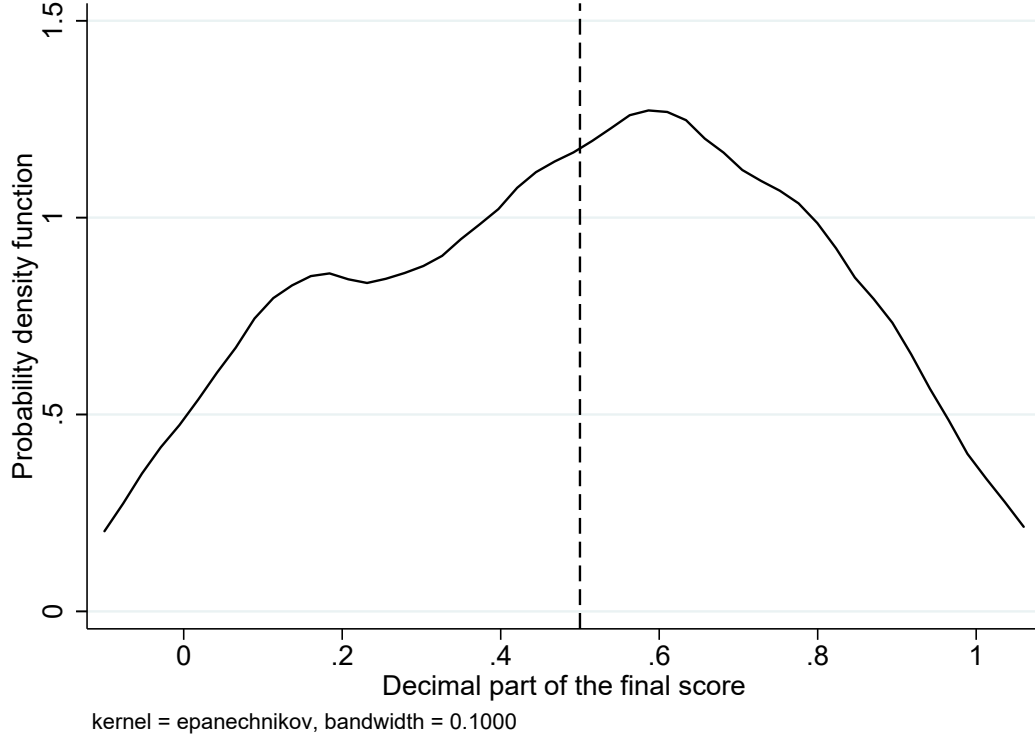
selection with respect to the cutoff would therefore require departments to be able to predict in each hiring round how the different indicators would influence their positioning within a given integer after being ranked against all other departments.

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 four hiring rounds) and focus on the decimal part of their normalised 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 plot the density of the decimal part of the normalised score and find that it is pretty smooth around the cutoff (Figure 4). Additionally and as mentioned in the descriptive statistics, the average of the decimal part of the final score is equal to .49 and the average department has been above the cutoff exactly twice over four hiring rounds (see Table 1 in Section 2.1 above). In order to further check this assumption, I construct bins of equal size over the support created by the decimal part of the normalised 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 ten different bins equally distributed around the cutoff) (Figure E2 in Appendix E). The graph shows that the number of observations in each bin changes somehow along the bandwidth, but these variations are not big in magnitude and do not happen systematically at the cutoff.

Secondly, identification in the RDD setting 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, which is however uncorrelated with the treatment itself. 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 some graphical analysis, I plot the average values for selected variables in each bin along the entire bandwidth (this time, for a much smaller bin size of 0.01) (Figure 5). Averages are computed in the year before the relevant normalised score was assigned (e.g. in 2010 for the 2011 hiring round), which can be thought as the baseline year before each hiring round. The figure plots residuals after controlling for the integer part of the normalised score, but results would be qualitatively similar when plotting the unconditional relation. The results for selected personal and labour market characteristics provide strong evidence in support of this identification assumption. Indeed, the average value of these variables is constant over the

Figure 4: Probability density function of the decimal part of the final score, over the four different hiring rounds



Note: The graph shows the probability density function of the decimal part of the final score for all the units of treatment variation (i.e. corresponding to a total of 23 departments over four different hiring rounds). Each observation enters the analysis with the decimal part of the final score, independently from its integer value.

entire bandwidth and there is no sign of a discontinuity at the cutoff.¹⁹ 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 demographic and labour market variables are well balanced between the two groups and none of the t-tests for equality of means reports a statistically significant difference.

In the same spirit, I can test the extent to which the normalised score and the .5 cutoff correlate with some observable characteristics measured in the year before the relevant hiring round. In particular, the final score is likely to be correlated with a number of demographic and labour market variables as the allocation rule was designed specifically to capture the risk of non-compliance with the legislation in a given department and year. However, after controlling for the integer part of the final score, it should be the case that this correlation becomes substantially weaker and eventually disappears. If this does not happen, it would suggest that the remaining component of the final score (i.e. its decimal part) still plays an important role in explaining non-compliance. Under this scenario, relying on the within integer distribution of observations

¹⁹I also conduct a placebo test by running the baseline specification that will be introduced in Section 4.2, but using as outcome variables the covariates that will be used in the analysis (see Appendix C for details).

Table 4: Descriptive statistics for units of treatment variation below and above the cutoff, year before the relevant hiring round

	Below the cutoff		Above the cutoff		T-test
	mean	s.d.	mean	s.d.	
Individual characteristics					
Male	0.488	0.009	0.488	0.009	-0.149
Age	39.017	1.173	38.907	1.333	0.418
Intermediate level of education	0.184	0.018	0.182	0.023	0.444
Rural	0.293	0.088	0.307	0.137	-0.587
Status in the labour 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, hours worked and wages					
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
Parental leave	0.124	0.043	0.122	0.059	0.198
Severance pay	0.080	0.038	0.075	0.039	0.668
Number of hours worked in main job	43.433	2.872	43.903	3.012	-0.766
Number of hours worked overall	44.528	2.588	44.951	2.553	-0.788
Income from main job	887013.400	96571.950	916941.400	138970.200	-1.195
Overall labour income	721077.900	118901.500	726714.800	175232.200	-0.180
N	45		47		

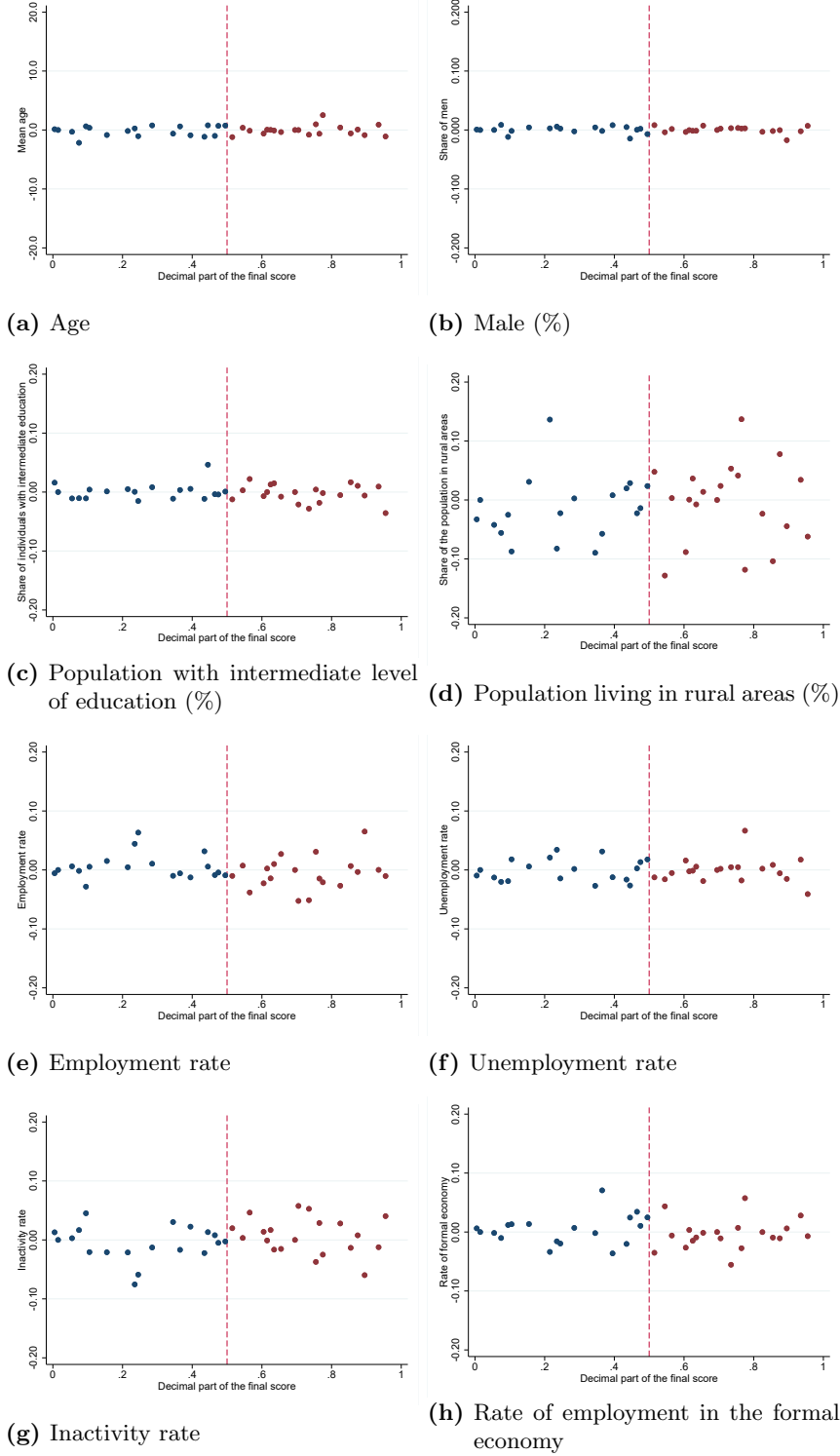
Note: The table reports means and standard deviations for selected variables computed at the department level between 2010 and 2013 as well as the results of t-tests for equality of means. Departments are divided according to whether they are above or below the .5 cutoff in a given hiring round. Descriptive statistics refer to the year before the relevant hiring round. This allows to examine if departments on two different sides of the cutoff differed along observable characteristics at baseline. Given the functioning of the policy as described in Section 2.1, the same department can be above or below the cutoff over different hiring rounds. The baseline year differs for each hiring round.

Table 5: Relation between the final score and observable characteristics in the year before the relevant hiring round, with and without integer dummies

	Share of men		Average age		Intermediate level of education		Share of rural	
Final score	-2.376*** (0.601)	-0.0436 (0.0687)	0.00806*** (0.00235)	0.000171 (0.000466)	0.111 (0.193)	0.00321 (0.0378)	-0.277*** (0.0536)	-0.00780 (0.00784)
	Employment rate		Unemployment rate		Inactivity rate		Rate of formal economy	
Final score	0.495*** (0.143)	-0.0193 (0.0184)	-0.0838 (0.126)	0.0200 (0.0222)	-0.417*** (0.124)	0.0114 (0.0143)	0.622*** (0.0719)	0.0229 (0.0194)
Integer dummies	No	Yes	No	Yes	No	Yes	No	Yes
N	92	92	92	92	92	92	92	92

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 final score assigned to a given department and year as part of the Action Plan. The reported coefficient are those of selected demographic and labour market characteristics measured in the year before the relevant hiring round. Regressions differ on whether dummies for the integer part of the final score are included. This is meant to capture how much of the observable heterogeneity is accounted for after focusing on departments within the same integer of the final score. *<10%, **<5%, ***<1%.

Figure 5: Evolution of personal and labour market characteristics around the cutoff (plotting residuals)



Notes: The graph plots residuals of the relationship between the decimal part of the final score and selected personal and labour market characteristics measured in the 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 four 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). This means that the same department can belong to a different bin over years, according to the final score assigned in that hiring round.

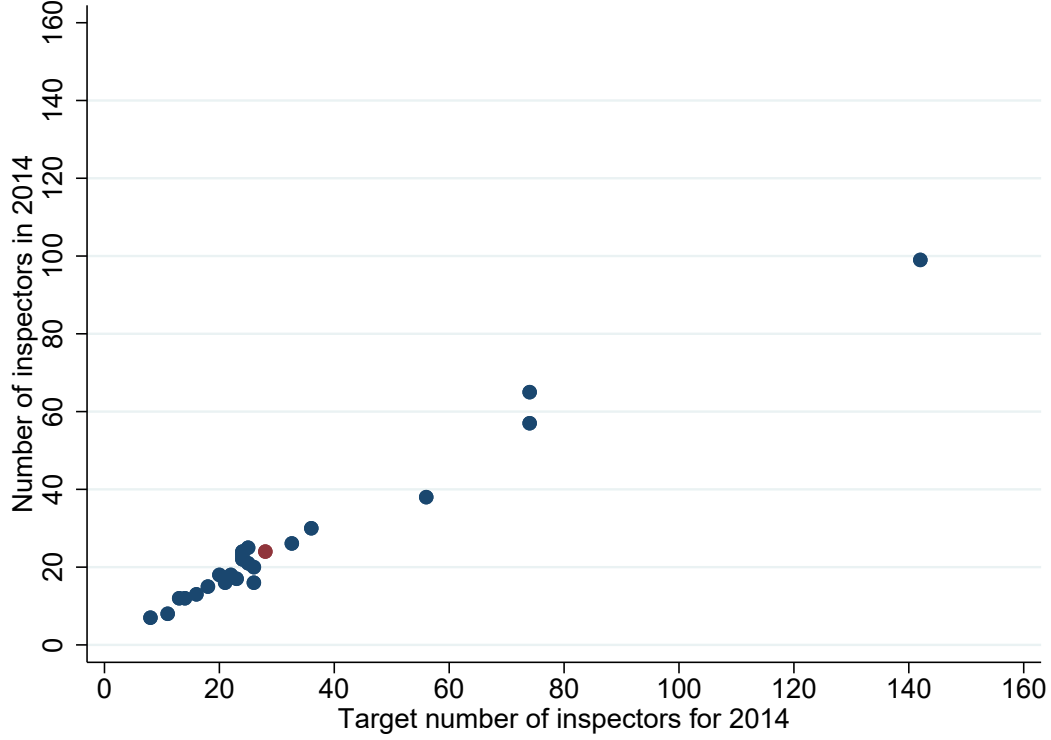
would not provide for a valid estimation strategy as departments below and above the cutoff would not necessarily be comparable. In Table 5, I regress the final score against selected

demographic and labour market characteristics measured in the year before the relevant hiring round. Results show how there is a strong correlation between these variables and the allocation rule when integer dummies are excluded (six out of eight variables are statistically significant at the 1 per cent level), but point estimates become close to zero and statistically non-significant after having included the integer dummies.

The discussion presented so far has shown how the standard RDD identifying assumptions are likely to be met in the present context. The following step is to understand which additional assumptions are imposed by the fact that I construct the main RDD variables cumulatively over time and to test whether these assumptions are also satisfied. Indeed, identification now exploits not only the discontinuity at .5 within each integer of the final score, but it also relies on the presence of policy implementation delays that vary across departments and years. If these delays were not random and also correlated with the .5 cutoff, this would invalidate the exogeneity of the treatment variation that I exploit for identification. Fortunately, this does not seem to be the case in the present context.

As mentioned in Section 2.1, previous reviews of the Action Plan have linked implementation delays to the constrained human resources capacities (especially at the local level) as well as the limited attractiveness of the job offers compared to the professional requirements for the positions (OECD, 2016). These problems predated the approval of the Action Plan (i.e. 71 out of 424 vacancies were already unfilled already in 2010) and were only accentuated by the introduction of quite demanding hiring targets, which for most labour offices required more than doubling the existing staff within a few years. In line with this interpretation, there is descriptive evidence supportive of the fact that a higher final score is associated with a larger implementation gap and a lower probability of meeting the hiring target. Dividing units of treatment variation in two groups by the median of their final score, I find that the share of departments meeting the hiring target is equal to 35 per cent below the median final score (i.e. with a final score less than two in a given hiring round) and only 11.5 per cent above the median (i.e. with a final score above or equal to 2). I also plot the target number of inspectors in 2014 according to the Action Plan against the actual number of inspectors working in a given department in that same year (Figure 6). The figure confirms that policy implementation gaps were larger in departments that were assigned a higher hiring target. This can be seen also within a simple regression framework (Table 6). In particular, a higher final score reduces the probability of meeting the hiring target (i.e. a one unit increase in the integer of 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 per cent, see Column 1) .

Figure 6: Target number of inspectors for 2014 and actual number of inspectors operating in the same year, national average and by department



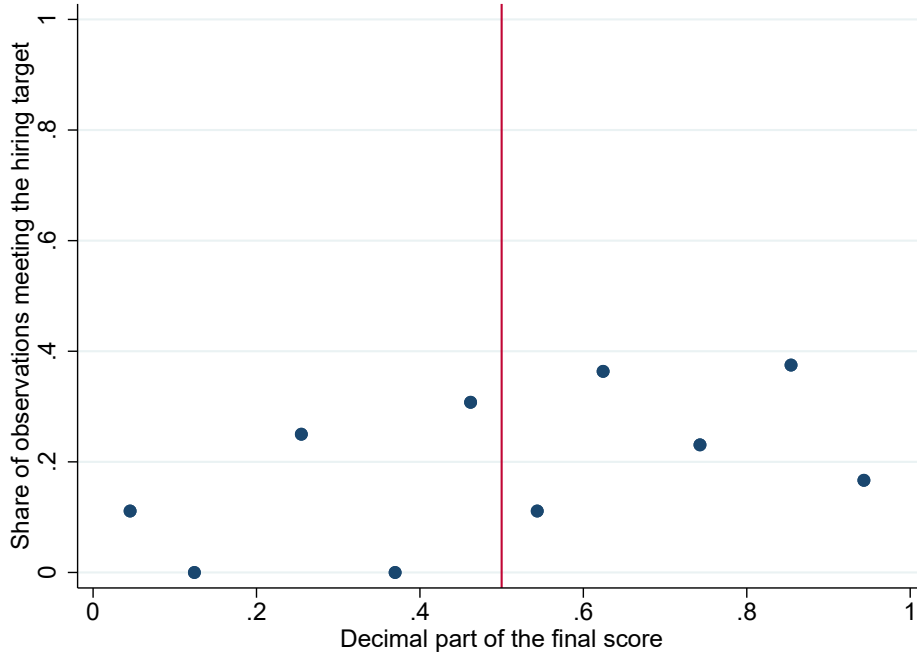
Note: The graph shows the target and actual number of inspectors operating in 2014. Each dot corresponds to a different department that is included in the analysis (i.e. 23 departments), while the red dot captures the unweighted national average.

At the same time, there is evidence supporting the idea that implementation delays are uncorrelated with the .5 cutoff within a given integer of the final score. This emerges from Figure 7, where I use the same partitioning introduced above (i.e. sorting units of treatment variation along the decimal part of the final score and dividing observations into ten different bins of equal size) and exploit it to plot the share of observations meeting the hiring target within each bin. The figure shows that this share is slightly lower for observations below the .5 cutoff compared to those above, but differences are not substantial in magnitude and they do not appear to be related to the presence of the cutoff. This appears even more neatly in the context of the simple regression framework introduced above (Table 6). The dummy for being above the .5 cutoff is uncorrelated with the probability of meeting the hiring target, both when included alone (Column 3) or together with the linear control for the integer part of the final score (Column 4). Point estimates become even smaller in magnitude and close to zero once I control for the integer part of the final score with a set of dummies, as it will be done in the baseline specification (Column 4). Overall, this suggests that policy implementation delays were strongly associated with the integer part of the final score but can be largely taken as uncorrelated with the .5 cutoff within a given integer.

Table 6: Determinants of the probability of meeting the hiring target in a given hiring round

	(1)	(2)	(3)	(4)
Integer of the final score	-0.0175*** (0.00500)		-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

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 one if the target is met). Column (1) controls linearly for the integer part of the allocation rule, Column (2) for the dummy for being above the .5 cutoff within a given integer, Column (3) includes both covariates, Column (4) controls for the dummy for being above the cutoff and includes a set of dummies for the integer part of the final score (not reported). The results show that the probability of meeting the hiring target is inversely correlated with the integer part of the final score, but it is uncorrelated with the .5 cutoff after restricting the observation to departments within the same integer. * <10%, ** <5%, *** <1%.

Figure 7: Share of observations having met the hiring target in a given year, over the four different hiring rounds

Note: The graph shows the share of units of treatment variation (i.e. corresponding to 23 departments over four different hiring rounds for a total of 92 observations) that have met the hiring target set by the Action Plan in a given department and year. Each observation enters the analysis with the decimal part of the final score, independently from its integer value.

4.2 Empirical specification

I run the following regression:

$$Y_{i,s,t} = \beta_0 + \beta_1 \sum Insp_{s,t} + \beta_2 Integ_{s,t} + \beta_3 Dec_{s,t} + \beta_4 Dec_{s,t} * Above_{s,t} + \beta_5 X_{i,s,t} + \epsilon_{i,s,t} \quad (5)$$

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

$$\sum Insp_{s,t} = \gamma_0 + \gamma_1 \sum Above_{s,t} + \gamma_2 Integ_{s,t} + \gamma_3 Dec_{s,t} + \gamma_4 Dec_{s,t} * Above_{s,t} + \gamma_5 X_{i,s,t} + v_{i,s,t} \quad (6)$$

$Y_{i,s,t}$ represents the outcome of interest for individual i , living in department s , in year t (i.e. between 2011 and 2014). $\sum Above_{s,t}$ 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 4 in Section 4.1). This variable is used as an instrument for $\sum Insp_{s,t}$, which is the main regressor of interest and it represents the cumulative yearly change between 2011 and year t in the number of inspectors working in department s (i.e. corresponding to the formula presented in Equation 3).

The main control variable is $Integ_{s,t}$, which is a series of dummies corresponding to the integer part of the final score assigned to department s in year t . This allows comparing departments with similar risks of law non-compliance as captured by the allocation rule in that given year. $Dec_{s,t}$ is a polynomial of order three in the decimal part of the final score, which is allowed to vary depending on whether the department is above or below the cutoff in a given hiring round (i.e. corresponding to the dummy $Above_{s,t}$). This last variable differs from $\sum Above_{s,t}$, as it is not constructed cumulatively over time. I also include a vector of individual level characteristics $X_{i,s,t}$. This includes dummies for sex, location of residence (i.e. urban or rural, as classified in the GEIH), highest level of educational attainment (equal to one if the individual has reached upper secondary education or the first stage of tertiary education) and a linear control for age. Finally, all the specifications control for department dummies as well as a linear time trend for each year in which a different allocation rule was set (i.e. taking a different value in each year but 2013 and 2014, see Appendix B for details). These last two variables are also interacted in order to allow for time trends to differ across departments.

While the spirit of the estimation is the one of a fuzzy RDD, constructing the treatment indicator and the instrumental variable cumulatively over time partially changes 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 is no longer univocal within a given year. In a standard fuzzy RDD, all departments (below) above the cutoff within a given year would take the value of (zero) one in the instrumental variable and this relationship would renew itself every year. Instead, in the present context departments characterised 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 in the present context. Nevertheless, I will replicate their spirit and also compare my baseline regression with the

results obtained from a normal fuzzy RDD which would however suffer from a weak first stage.

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 four different hiring rounds), where variations in treatment intensity take place. The results are robust to clustering standard errors at the department level (but this would allow to identify only few clusters) and I also conduct the analysis at the department and year level to confirm the main findings (see Section 5.3). All outcome variables (including those related to measures of compliance with the labour law) are set to zero for individuals not in employment, in order to avoid issues of sample selection in the absence of a valid exclusion restriction. This might lead to an attenuation bias of point estimates, which needs to be kept in mind when discussing the results. Triangle kernel weights are used in all specifications.

5 Empirical findings

5.1 Labour law compliance

Table 7 presents the main results of the analysis with respect to a number of indicators of labour law compliance, as introduced in Section 3.2. For each outcome of interest, I report two estimates that differ simply with respect to whether I control for the decimal part of the final score with a third order polynomial which is allowed to vary on the two sides of the cutoff. The coefficient presented every time corresponds to β_1 in Equation 5 and it captures the effect of an additional inspector in a given department and year. Overall, the results show that the hiring of an additional inspector has a positive and significant effect on different measures of law compliance. This finding is consistent across the board and point estimates are similar in size and of reasonable magnitude. In the most complete specification, an additional inspector in a given department increases the probability of having a number of job-related benefits by between 0.2 and 0.3 percentage points (from a mean in the outcomes of interest below the cutoff generally between 15 and 20 per cent, depending on the variable analysed). This corresponds to an increase of around 2 per cent in the probability of complying with the legislation and receiving mandated benefits in a given department and year.

The positive effect of the increase in enforcement levels is reported on a number of different indicators of law compliance. These outcomes include the probability of having a written employment contract. In Colombia, both written and oral contracts are allowed by the legislation and oral contracts are quite popular especially in the informal sector. However, written contracts are the only ones that can be enforced so labour inspectors clearly encourage

their adoption. Similarly, there is a positive treatment effect on the probability of being entitled to severance payments upon layoff (which need to be paid 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) and a negative effect on the probability of working an excessive number of hours per week (i.e. the legislation allows for a maximum of 48 hours). At the same time, the effect on the probability of receiving paid holidays (individuals have right to 15 working days per year) is positive but becomes non-significant in the most complete specification. There is no effect on the probability of receiving maternity or paternity leave (i.e. the legislation provides for 18 weeks for mothers and 8 working days for fathers), which also corresponds to the benefit with the lowest initial coverage in the sample (around 10 per cent).

Table 7: Baseline results: measures of labour law compliance

	(1)	(2)	Mean below cutoff
Written contract	0.00236*** (0.000505)	0.00256** (0.000996)	0.185
Excessive working time	-0.00302* (0.00169)	-0.00648* (0.00365)	0.191
Severance payments	0.00245*** (0.000531)	0.00289*** (0.00104)	0.169
Paid holidays	0.00248*** (0.000837)	0.00144 (0.00129)	0.157
Parental leave	0.00108 (0.000758)	-0.000562 (0.00146)	0.099
Health social security	0.00193*** (0.000502)	0.00200** (0.000872)	0.177
Pension social security	0.00198*** (0.000453)	0.00213** (0.000857)	0.166
N	2,377,658	2,377,658	
F test	34.2776	13.4172	
Main controls			
Integer of the final score FE	Yes	Yes	
Polynomial of order 3 for decimal of the final score	No	Yes	
Polynomial of order 3 for decimal of the final score*Above	No	Yes	
Baseline controls			
Department fixed effects	Yes	Yes	
Time trends	Yes	Yes	
Department fixed effects*Time trends	Yes	Yes	
Individual level characteristics	Yes	Yes	

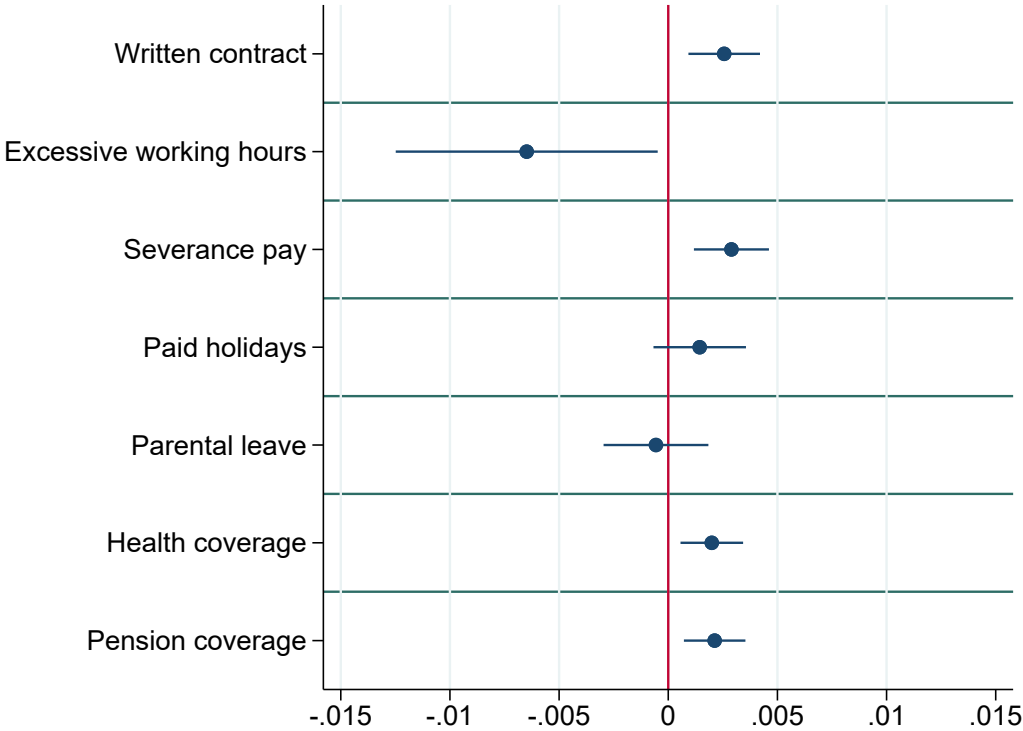
Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on different measures of labour law compliance. Reported coefficients correspond to the parameter β_1 in equation 5 in the main text. Standard errors are clustered at the department and year level, leading to the identification of 92 clusters. The definition of the outcomes of interest is available in Table F1 in Appendix F. The two columns differ for whether the specification includes a control for the third order polynomial of the decimal part of the final score, which is allowed to vary on the two sides of the cutoff. Coefficients in bold remain significant after applying the correction proposed by [Benjamini and Hochberg \(1995\)](#), with a false discovery rate equal to 5 per cent. * <10%, ** <5%, *** <1%.

I also find that the strengthening public enforcement increases the probability of being covered by social security for both health and pension. These variables take into account whether employers contribute (partially or fully) to the social security funds of their workers, which is required by the legislation in Colombia.²⁰ However, positive treatment effects would appear

²⁰In particular, employers should pay 12% of the worker's monthly salary in pension contributions and 8.5%

also if considering as outcomes of interest social security coverage independently from how contributions are made. These results show that the increase in labour law enforcement might generate long-term effects which potentially extend beyond the realm of the labour market, as individuals gain access to public services and are included within formal social safety nets. These beneficial effects are likely to also have an impact on health and poverty outcomes both in the short- and long-term as well as to potentially generate spillover effects on other members of the household. Figure 8 provides for a graphical presentation of the results for all measures of labour law compliance discussed above, plotting point estimates and confidence intervals corresponding to the most complete specification for the different outcomes.

Figure 8: Baseline results: measures of labour law compliance



Note: The figure plots point estimates and 90 per cent confidence intervals for the effect of an additional inspector on different measures of labour law compliance, following the most complete specification presented in Table 7.

Returning to Table 7, adding the third order polynomial for the integer part of the final score (i.e. moving from column 1 to column 2) does not generally change the results. This provides additional evidence that unobserved heterogeneity is adequately accounted for when looking at observations within the same integer of the final score (in line with results presented in Table 5 in Section 4.1). The F test confirms that the exogenous variation generated by the presence of the cutoff creates changes in treatment intensity that are strong enough for identification, after having constructed the two variables cumulatively over time as described above.

in health contributions. Workers should contribute with 4% of their salary in each of the two accounts.

While a detailed set of robustness tests will be presented later in this section, I already want to check the extent to which the results are sensitive to an adjustment for multiple hypothesis testing. In particular, it is important to account for the possible presence of false discoveries given the number of outcomes of interest analysed (Benjamini and Hochberg, 1995). Two main approaches are followed in the literature to account for this issue: to aggregate separate outcomes in summary indicators or to control for false discovery rates within families of outcomes. The first method will be used in Section 5.2, while here the second approach is more appropriate given the interest in specific measures of law compliance (de Andrade et al., 2016). Adjusting for false discovery rates does not substantially change the results. In the most complete specification, only the coefficient for excessive working time (which individually was significant at the 10 per cent) loses statistical significance after accounting for multiple hypothesis testing.

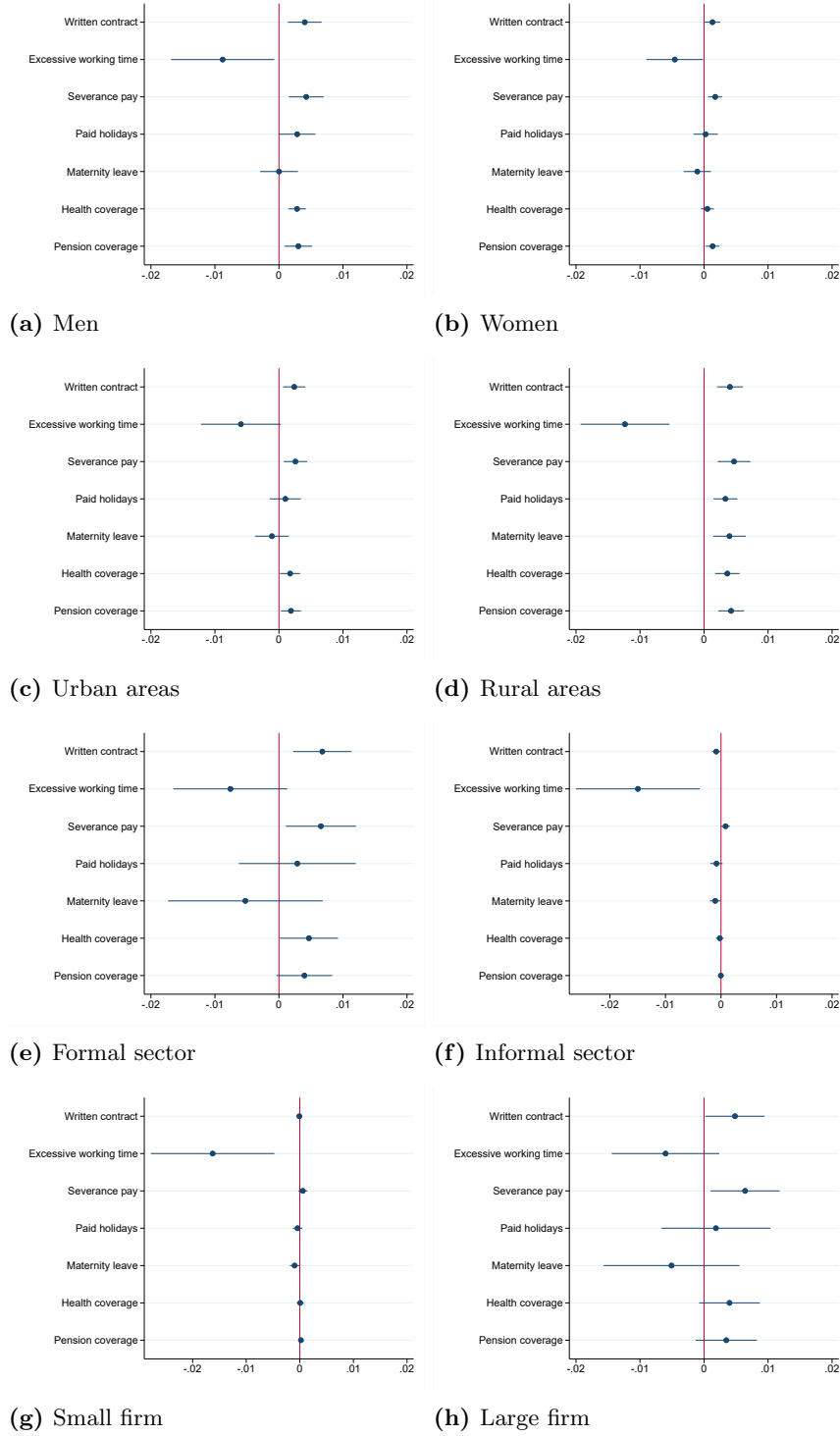
I also conduct some heterogeneous analysis and summarise the main findings in Figure 9, which plots for each separate group in the population the point estimate and confidence intervals of treatment effects on the different measures of labour law compliance analysed in Table 7. I find that the positive effects of labour inspection on compliance with the legislation are very similar by sex and location of residence (i.e. urban or rural). This happens despite initial differences in the levels of law compliance across these groups, as women and individuals living in rural areas have on average a lower probability of having a written employment contract and are also less likely to receive job-related mandated benefits. At the same time, the positive treatments effects on measures of law compliance 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 of interest.²¹ A similar pattern emerges when analysing 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 are largely driven by the prevalence of small firms in the informal sector of the economy and they would almost entirely disappear when comparing small and large enterprises within the same sector.²²

The presence of asymmetric effects of public enforcement between the formal and the informal economy was assumed in previous empirical and theoretical contributions, despite never being directly shown before. The phenomenon can be explained by how the system of labour inspection operates. Despite being formally in charge of verifying law compliance in all types of economic units, labour inspectors are in practice more likely to target registered firms that are easier to find (from administrative registries) and visit. This is especially the case in Colombia,

²¹The only exception is represented by the effect on the probability of working an excessive number of hours, which is negative and significant also in the informal sector of the economy.

²²Results for small and large firms by sector of economic activity are available from the author upon request.

Figure 9: Heterogeneous treatment effects: measures of labour law compliance



Notes: The figure plots point estimates and 90 per cent confidence intervals for the effect of an additional inspector on different measures of labour law compliance separately for different samples, following the most complete specification presented in Table 7. 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.

where inspectors do not have service vehicles and need to seek assistance from workers, employers or trade unions to organise inspections (OECD, 2016). Despite the possibility of raising complaints anonymously, it is also possible that fewer inspections in the informal sector result from informal workers' lower likelihood of reporting violations of the labour legislation for fear

of being punished or dismissed by the employer in case non-compliance is confirmed. In this setting, the absence of treatment effects on measures of labour law compliance in the informal economy can also be seen as a placebo test as it shows how the positive treatment effects discussed above are not driven by differential time trends across departments in the coverage of job-related mandated benefits.

5.2 Status in the labour market, hours worked and wages

I now look at whether the positive effects on law compliance lead to any adjustment with respect to overall employment levels and the distribution of employment between the formal and the informal sector. Even if labour inspection has a direct impact only in the formal economy, it is likely to affect also the informal sector due to the strong linkages between labour markets (Almeida and Carneiro, 2012). However, the type of adjustment that will take place depends on the nature and composition of the informal sector. According to traditional models of informality, firms that operate informally would not be able to face the costs of formalization (Magnac, 1991). In line with this interpretation, previous empirical contributions have found that strengthening public enforcement increases formal employment but reduces overall employment (Almeida and Carneiro, 2009, 2012). However, traditional models are not able to explain some of the key patterns observed in the data. These include the frequent transition of workers between the formal and the informal sector (Bosch and Maloney, 2010), the substantial overlap in productivity distributions between formal and informal firms (Porta and Shleifer, 2008) and the presence of informal workers in both registered and unregistered enterprises (de la Parra, 2017). Alternative views of informality have therefore been proposed, whose final implications is that most informal firms deliberately decide to operate informally in order to reduce labour costs (Meghir et al., 2015; Ulyssea, 2018). The estimation of these models shows that higher public enforcement can increase output and productivity by decreasing the size of the informal sector, without necessarily leading to an increase in unemployment.²³

Table 8 presents the results of the present analysis with respect to the status in the labour market. The main finding is that the increase in public enforcement leads to a rise in formal employment. This is partially due to the formalization of workers that were already employed in the formal sector, but were kept off-the-books by their enterprise (i.e. I observe a small decrease in the probability of being informally employed in the formal economy). Following

²³In the counterfactual exercise, Ulyssea (2018) finds that strengthening enforcement raises output and productivity but reduces welfare. The result on welfare differs from the positive one obtained by Meghir et al. (2015), but this difference is entirely accounted for by the focus of Ulyssea (2018) on firms (rather than workers as in Meghir et al. (2015)) and the presence of fixed costs to register the enterprise in the formal sector that enter negatively into the welfare function.

Ulyssea (2018), I call this the intensive margin of formalization and interpret it as the direct effect of the increase in public enforcement given that the heterogeneous analysis discussed above has shown that inspectors target uniquely formal firms. However, this channel alone cannot explain the total increase in formal employment that I observe in the data. This means that the majority of the increase in formal employment comes as a result of an expansion of the formal sector of the economy along the extensive margin (i.e. there is an increase in the probability of being formally employed in the formal economy). Although I cannot directly study transitions in the labour market in the absence of longitudinal data, this can result from workers in the informal sector and/or individuals in non-employment that move to the formal sector and directly obtain a formal job. This phenomenon can be explained by the fact that workers value mandated benefits and move to the formal sector in response to their increased provision (Almeida and Carneiro, 2012).

At the same time and differently from previous empirical studies, I do not find any negative effects on indicators of labour market participation (i.e. employment, unemployment and inactivity). Rather, point estimates are positive for employment and negative for unemployment and inactivity. However, estimates partially lose significance in the most complete specification which controls flexibly for the third order polynomial of the decimal part of the final score. This means that I cannot reject the null hypothesis of zero effects of the increase in public enforcement on overall employment, unemployment and inactivity at conventional levels, but – if anything – evidence would be in favour of a positive effect on these indicators. As a result, informal employment in the informal economy does not decrease even if the share of employment in the formal sector decreases. I also do not find any effect on hours worked (overall and in the main job) and labour income (overall and in the main job). Estimates for these coefficients are both small in magnitude and statistically non-significant. However, lack of adjustment along the intensive margin at the level of the entire economy might hide differential trends between the formal and informal sectors.

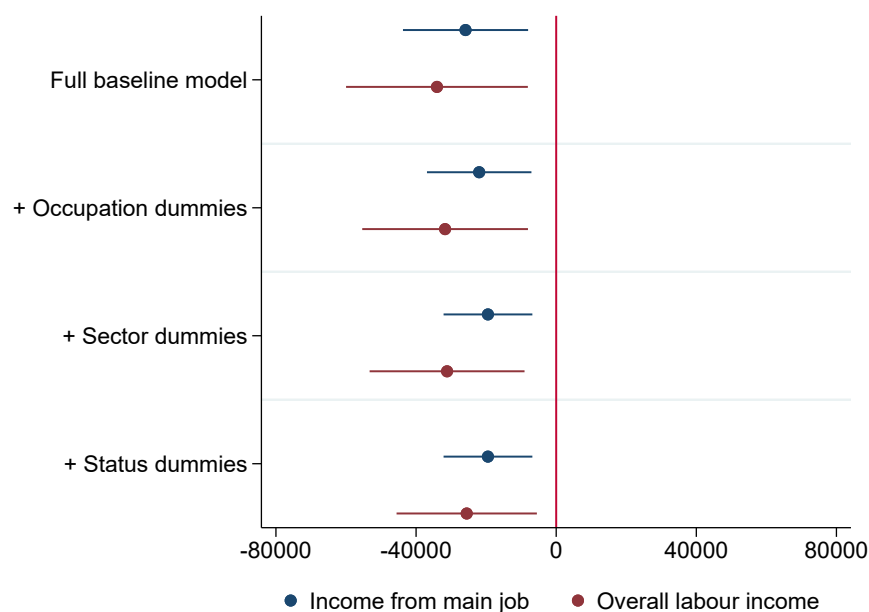
In order to explore this hypothesis, I restrict the analysis to employed individuals and examine treatment effects on hours worked and wages separately in the formal and the informal sector. I find that wages decrease in the formal sector and increase in the informal sector as a result of the increase in labour inspectors (Table F2 in Appendix F). While the absolute magnitude of the coefficients is different, the relative change is roughly equal to 2 per cent in both sectors after accounting for initial differences in wages. I interpret the decrease in wages in the formal sector as a response to the increase in labour costs due to the higher provision of mandated benefits. At the same time, the wage increase in the informal sector can

Table 8: Baseline results: status in the labour market, hours worked and wages

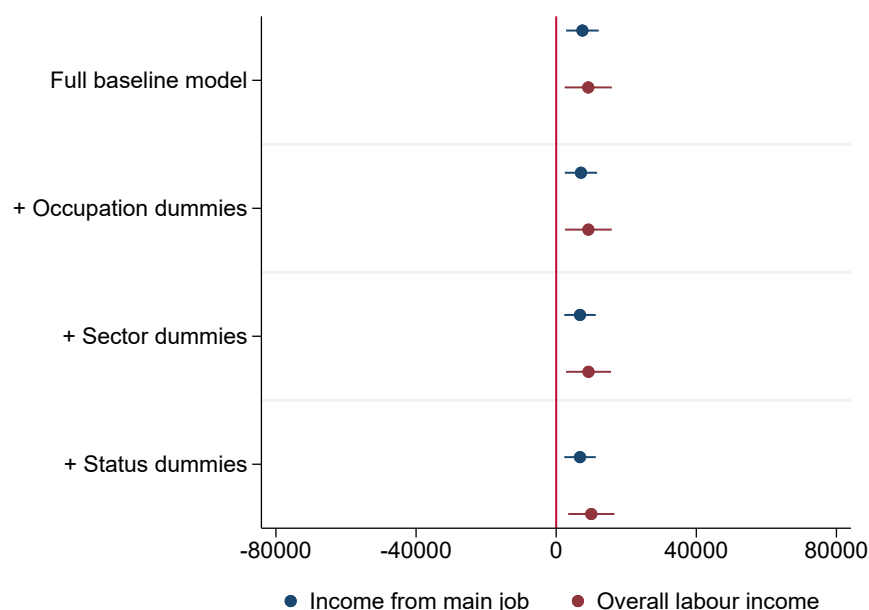
	(1)	(2)	Mean below cutoff
Employed	0.00206** (0.000948)	0.00257* (0.00150)	0.597
Unemployed	-0.000990** (0.000458)	-0.000512 (0.000868)	0.073
Inactive	-0.00107 (0.00110)	-0.00206 (0.00194)	0.331
Formal employment	0.00244*** (0.000519)	0.00260** (0.00103)	0.208
Formal economy	0.00224*** (0.000561)	0.00202** (0.000938)	0.232
Formal employment in formal economy	0.00241*** (0.000518)	0.00257** (0.00103)	0.206
Informal employment in formal economy	-0.000179 (0.000169)	-0.000551* (0.000309)	0.025
Informal employment in informal economy	-0.000196 (0.000785)	0.000528 (0.00148)	0.363
Number of hours worked in main job	-0.0304 (0.0509)	-0.110 (0.120)	26.128
Number of hours worked overall	-0.0379 (0.0552)	-0.117 (0.131)	26.662
Income from main job	-2,547 (2,643)	-9,478 (6,527)	392152.3
Overall labour income	-2,663 (2,916)	-9,749 (7,147)	491710.6
N	2,377,658	2,377,658	
F test	34.2776	13.4172	
Main controls			
Integer of the final score FE	Yes	Yes	
Polynomial of order 3 for decimal of the final score	No	Yes	
Polynomial of order 3 for decimal of the final score*Above	No	Yes	
Baseline controls			
Department fixed effects	Yes	Yes	
Time trends	Yes	Yes	
Department fixed effects*Time trends	Yes	Yes	
Individual level characteristics	Yes	Yes	

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on indicators of status in the labour market, hours worked and wages. Reported coefficients correspond to the parameter β_1 in equation 5 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 Table F1 in Appendix F. The two columns differ for whether the specification includes a control for the third order polynomial of the decimal part of the final score, which is allowed to vary on the two sides of the cutoff. *<10%, **<5%, ***<1%.

Figure 10: Treatment effects on wages in the formal and informal sectors, different specifications



(a) Wages in the formal sector



(b) Wages in the informal sector

Notes: The figure plots point estimates and 90 per cent confidence intervals for the effect of an additional inspector on labour income (from the main job and overall) in the formal and informal sectors (panels A and B, respectively). The figure reports point estimates of different specifications, which add different sets of controls to the baseline specification (i.e. corresponding to the most complete specification presented in the main text). In particular, the specifications add complete sets of dummies for (i) occupation, (ii) sector of economic activity (at the two digit level), and (iii) status in the labour market. This is meant to provide suggestive evidence to test how much results are driven by compositional changes rather than pure treatment effects. See Section 5.2 for details.

be seen within the context of a compensating differential framework to avoid further shift in labour supply.²⁴ A possible concern with these interpretations is that results might be driven

²⁴Adequately interpreting these adjustments would ideally require having access to matched employer-employee data of longitudinal nature for both formal and informal enterprises. While the former type of information can be obtained in many countries through social security records, matching workers and firms in the informal sector is

by compositional changes given the shifts in employment discussed above. In order to check for this risk, I augment the most complete specification by cumulatively adding dummies for occupation, sector of activity (at the 2 digit level) and status in the labour market. If results were simply driven by changes in the composition of the formal and informal labour markets, treatment effects would decrease as I add a large set of additional controls which are strongly associated with wages. The results provide instead evidence in favour of a causal interpretation of the findings, as point estimates remain very stable across specifications. This is shown for both hours worked and labour income in Table F2 in Appendix F, while the results for income in both the formal and the informal sector are also plotted in Figure 10 here.

5.3 Robustness tests

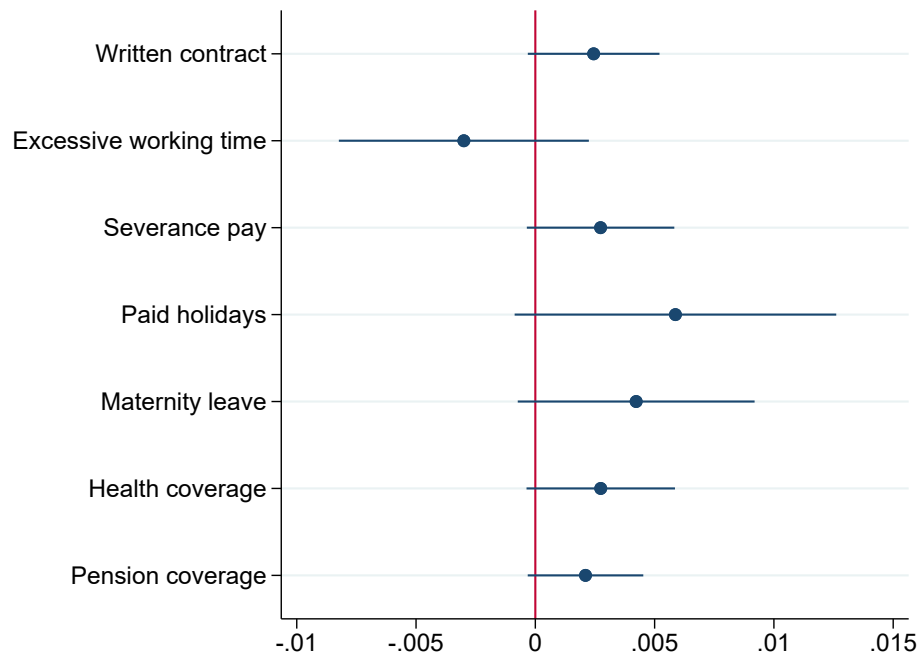
In order to confirm the main findings of the analysis, I conduct different robustness tests. In this section I briefly summarise them, while I leave a more thoughtful discussion to Appendix C. First, I want to check how much the results discussed above would differ from a standard fuzzy RDD (which would however suffer from a weak first stage). The results of this exercise are very encouraging, as all coefficients estimates are of the same sign and very similar in magnitude to those presented above (see Figure 11, panel A for measures of labour law compliance and panel B for indicators of status in the labour market). They are also close to statistical significance at conventional levels despite the F test is well below the benchmark accepted in the literature (i.e. being equal to 2.869). Additionally, I test the sensitivity of the results to the presence of few clusters by replicating the main results at the department and year level (i.e. the unit of observation becoming the department rather than the individual) and changing the clustering procedure for standard errors (i.e. clustering at the department level to account for possible serial correlation over years). Results are similar in magnitude to those discussed above and they largely remain statistically significant. Additionally, I exploit the presence of multiple cutoffs (i.e. within each integer) and the fact that this generates variations in the relative intensity of the exogenous change in public enforcement.²⁵ The results confirm the prediction that treatment effects are stronger when the relative variation in treatment intensity is larger. Finally, I conduct a set of miscellaneous tests by (i) not controlling for individual level covariates, (ii) excluding departments that do not have a valid counterpart on the other side of the same integer, (iii) excluding departments that have been always above or below the cutoff in all different hiring

virtually impossible (Meghir et al., 2015; Ulyssea, 2018). In the absence of this type of data, these interpretations are only of suggestive nature.

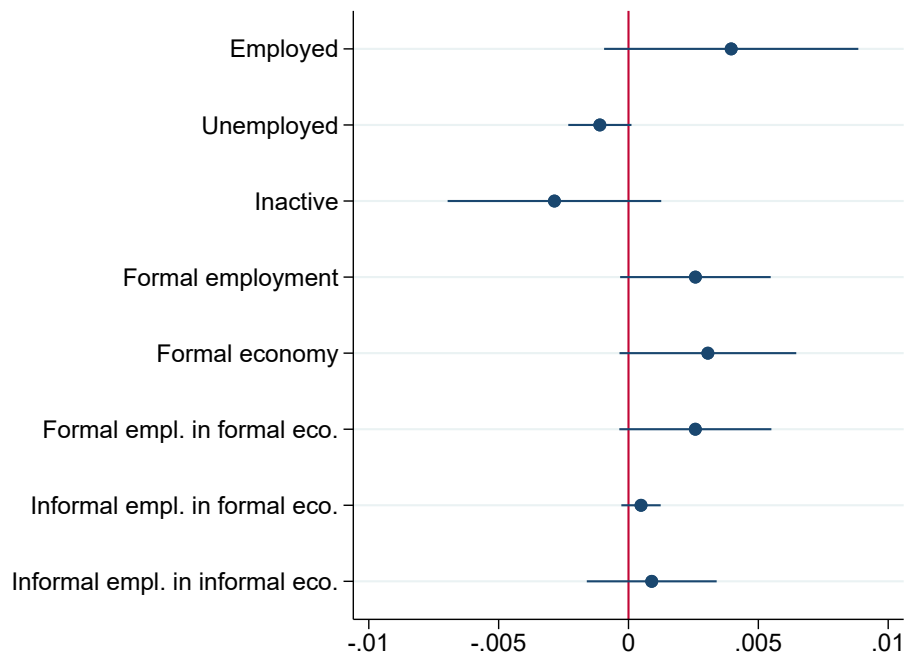
²⁵For instance, an additional inspector resulting from being above the .5 cutoff represents a 100 per cent change in treatment intensity for a department whose integer part of the final score was equal to one and a 50 per cent increase for a department whose integer of the final score was equal to two.

rounds, and (iv) running the baseline regressions on covariates. None of these tests challenges the results presented above.

Figure 11: Treatment effects using RDD



(a) Indicators of labour law compliance



(b) Indicators of status in the labour market

Notes: The figure plots point estimates and 90 per cent confidence intervals for the effect of an additional inspector on different measures of labour law compliance (Panel A) and indicators of status in the labour market (Panel B). In Panel B, I do not plot results for hours worked and wages for ease of exposition. Compared to the baseline results, point estimates plotted in this figure are obtained from a fuzzy RDD specification (i.e. the instrumental variable and the treatment indicator are not constructed cumulatively over time). The regressions do not flexibly control for the third order polynomial in the decimal part of the final score. This is because the F test for these regressions is already low (i.e. being equal to 2.869) and it would be even lower in the most complete specification.

6 A simple theoretical framework

As a final step, I interpret the results discussed above with a simple theoretical framework. There is a large literature that has modeled firms' decisions to operate in the formal or informal sector of the economy, with some contributions specifically examining the role of public enforcement in reducing informality ([Albrecht et al., 2009](#); [Almeida and Carneiro, 2012](#); [Meghir et al., 2015](#); [Ulyssea, 2018](#)). I build on this literature to explain key patterns observed in the empirical part of the paper. In particular, I find that an increase in public enforcement raises compliance with the legislation and the provision of mandated benefits in the formal sector of the economy. This generates an increase in formal employment, which largely takes place along the extensive margin. Despite these adjustments, indicators of labour market status (i.e. employment, unemployment and inactivity) for the entire economy are not affected and – if anything – experience a slight improvement. As a result, employment in the informal sector does not decrease even if the share of informal employment in the overall economy decreases. Wages adjust in the two sectors in order to compensate for these adjustments. This section summarizes the main insights from the model, which is fully presented in Appendix D.

A natural point of departure is to consider a competitive labour market. In the absence of any frictions, the rise in labour costs generated by the increased provision of mandated benefits leads to a fall in wages in the formal sector of the economy. The extent of this adjustment depends on how much workers value benefits over wages and it determines the changes to the labour market equilibrium in both the formal and informal sectors. A competitive labour market would be consistent with the empirical findings presented above only after assuming that workers value benefits more than their cost to employers. In that case, wages decrease in the formal sector to more than compensate the increase in labour costs (i.e. rate of pass through from benefits to wages greater than one). In this scenario, increasing public enforcement facilitates the emergence of an equilibrium characterised by higher benefits and lower wages which would be otherwise difficult to achieve in the absence of perfect commitment by the firm to provide these benefits. It is theoretically possible to imagine a rate of pass through above one, given that in most countries benefits are untaxed (unlike profits) and workers might therefore value one unit of benefit more than its cost for employers ([Almeida and Carneiro, 2012](#)). However, in practice there is ample empirical evidence from both high- and low- and middle-income economies, including Colombia, showing that such a high rate of pass through is unrealistic ([Arulampalam et al., 2012](#); [Cruces et al., 2010](#); [Fuest et al., 2018](#); [Gruber, 1994, 1997](#); [Kugler, 2005](#); [Kugler and Kugler, 2009](#); [Serrato and Zidar, 2016](#)).

For this reason, I need to depart from the simple model of a competitive labour market. [Almeida and Carneiro \(2012\)](#) do so by including downward wage rigidities due to the presence of the minimum wage in the formal sector, which prevent employment to fully adjust. However, their implications are not consistent with the increase in formal employment and the lack of reduction of overall employment levels that I observe in the data. For these reasons, I augment the model by including the possibility that formal labour demand does not adjust one-to-one following an increase in the level of taxation paid on formal employment. This introduction is consistent with (but not limited to) the presence of search frictions as in [Meghir et al. \(2015\)](#). This modified version of the model generates predictions which are consistent with the empirical findings of the paper under less binding conditions on the rate of pass through. In particular, this model can replicate the empirical findings presented above provided that the value placed by workers on these benefits is larger than the reduction in formal labour demand which results from a one unit increase in taxes. In the extreme case in which labour demand cannot adjust, any non-negative valuation of the benefits by the workers would be consistent with my results. This modified version of the model has some testable implications. In particular, the reduction in wages observed in the formal sector should be larger in those labour markets where workers have fewer outside options. In order to suggestively test for this hypothesis, I augment the baseline specification by including an interaction term between the treatment indicator and the share of formal sector employment in a given two digit industry and department. This is computed in 2010 in order to avoid any confounding effect of the Action Plan. The results confirm the main intuition of the model, showing that the decrease in formal sector wages discussed above takes place only in labour markets characterised by an initially high rate of informal sector employment (Figure D1).

7 Conclusions

This paper analyses the effects of increasing public enforcement on compliance with the legislation and the implications in terms of labour market adjustments. This research question is examined in a particularly interesting setting, as Colombia 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. This includes measures to reinforce law compliance in the labour market, where the vast majority of the population works in the informal sector and does not enjoy the provision of mandated benefits nor has access to basic rights at work. There have also been systematic reports of episodes of violence at the workplace, including the kidnapping and murder of trade union activists. In particular, between 2000 and 2010 Colombia accounted for 63.12

per cent of trade union murders registered worldwide ([ITUC, 2010](#)). The genesis of the reform at the centre of the analysis is also interesting, as an increasing number of trade agreements includes labour provisions ([ILO, 2016](#)).

The results of the analysis shed light on the possible reasons that drive firms' decision to operate in the formal or informal sector of the economy as well as the potential role of public enforcement within largely informal labour markets. In particular, traditional models of informality imply that the informal sector expands the set of available opportunities by creating jobs that would not exist otherwise. Accordingly, previous empirical studies have found that stricter public enforcement increases the share of formal employment at the expense of employment creation ([Almeida and Carneiro, 2009, 2012](#)). More recently, alternative models have found that a large share of informal firms operate in the informal sector by choice rather than need. This means that they could operate formally, but prefer not to do so in order to reduce labour costs and obtain higher profits. In this context, stronger public enforcement can increase output and productivity ([Ulyssea, 2018](#)) and this might also lead to a reduction in unemployment due to better job matches ([Meghir et al., 2015](#)).

This paper presents empirical support for this second class of interpretations of informality, without the need of structurally estimating the parameters of interest. Adopting a robust estimation strategy which relies on the presence of a natural experiment, I show that overall employment levels do not decrease when compliance with mandated benefits increases. There is a shift in employment from the informal to the formal sector of the economy, partially due to the registration of workers that were kept off-the-books within a formal enterprise. However, most of the increase in formal employment results from workers that move to the formal sector of the economy in order to take advantage of the higher provision of mandated benefits. Wages decrease in the formal sector to compensate for the increase in labour costs, but only in labour markets characterised by low formality rates. At the same time, wages increase in the informal sector of the economy in a compensating differential fashion. Similar results were obtained by ([Botelho and Ponczek, 2014](#)), although in the context of a policy that extended social protection coverage.

The results of the paper contribute to a debate at the intersection between empirical studies on formalization policies and theoretical contributions on the informal sector and they provide the first fully empirical support for recent models of informality and the role of public enforcement ([Meghir et al., 2015](#); [Ulyssea, 2018](#)). However, some caveats need to be kept in mind. One limitation of the present analysis relates to the absence of longitudinal data to track individuals and enterprises over time. While in many countries this type of information

is available for the formal sector (i.e. through social security records), matching workers and enterprises in the informal sector is virtually impossible. The findings presented in the paper are also specific to an economy in which large formal and informal sectors coexist. While this is the case in many middle-income economies, the same findings would not necessarily apply to low-income countries where virtually all employment is informal and most informal firms are of subsistence nature.

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Appendices

A Appendix: Literature review

As stated in the introduction, the paper contributes to a debate at the intersection between empirical studies on formalization policies and theoretical contributions on the nature and composition of the informal sector of the economy. This Appendix reviews some of the empirical studies related to the paper, with a focus on contributions that examine the impact of labour law enforcement on compliance with the legislation and employment outcomes. This represents a relatively small literature, as most empirical studies focusing on issues related to labour legislation in low- and middle-income economies have instead been traditionally investigating the effects of the strictness of legal provisions (e.g. length of notice period or firing costs) on labour market outcomes (e.g. unemployment rate) (Botero et al., 2004). However, this approach fails to take into account whether these provisions are actually implemented, which is often not the case in low- and middle-income economies (Bhorat et al., 2019; Chatterjee and Kanbur, 2018; ILO, 2018). The absence of an adequate body of knowledge on issues of labour law enforcement can be connected to two problems, notably of identification and measurement error.

Identification of a causal link in this field of research is challenged by the non-random relation between enforcement and compliance (Almeida and Carneiro, 2012; Ronconi, 2010). On the one hand, governments might be willing to strengthen enforcement in areas characterised by a higher risk of non-compliance. On the other hand, enforcement capacities might be higher in regions characterised by better institutional conditions which are themselves conducive to compliance (Chatterjee and Kanbur, 2018). The relative importance of these forces will determine whether simple OLS estimates are downward or upward biased. Ideally, one would solve the problem by randomly selecting enterprises to be inspected. However, this is difficult to happen due to both ethical and implementation concerns. Accordingly, only one paper in the literature takes an experimental approach and it faces problems related to the fact that most randomly allocated inspections fail to take place because many of the selected enterprises are closed at the time of the visit or it is impossible to find them (de Andrade et al., 2016). The authors then revert to estimating a local average treatment effect, using the assignment to treatment as an instrument for the inspection actually taking place. With this adjustment, the results reveal that inspections have a strong positive impact on the probability of firm's formalization.

The rest of the literature on labour law enforcement has pretty consistently adopted an IV approach.²⁶ Almeida and Carneiro (2009, 2012) exploit the fact that inspectors in Brazil are

²⁶One of the few exceptions is represented by Gindling et al. (2015), who compare workers earning just above or below the minimum wage before and after the introduction of a campaign to increase minimum wage compliance

assigned to labour centres and need to travel by car to visit firms. As a result, distance to the labour centre will decrease the probability of a firm being inspected and this will especially be the case in areas where labour centres have few inspectors. Using this indicator of enforcement, the authors find that labour inspection increases formal employment and reduces wage dispersion in the formal sector. However, stronger enforcement also increases unemployment and reduces firms' growth. Similarly, [Viollaz \(2018a,b\)](#) uses the arrival cost of labour inspectors (measured as the logarithm of per capita crossing vehicles per kilometer) as an instrument for enforcement in Peru. The results reveal limited effects of labour inspection on law compliance. [Bhorat et al. \(2012\)](#) exploit the institutional organization of labour centres in South Africa, where inspectors and non-inspectors are based in the same centres while having different tasks. In this context, they use the number of non-inspectors in a labour centre as an instrument for the number of inspectors in the same centre. Their results do not reveal any significant effect of enforcement on non-compliance with the minimum wage. [Ronconi \(2010\)](#) exploits the fact that in the proximity of an election, governments might be more willing to strengthen labour law enforcement in order to gain political support. Using election years as an instrument for enforcement, he finds that an additional inspector per 100,000 people increases compliance with mandated benefits.

Turning to challenges related to measurement error, these might affect both the treatment indicator and the outcome of interest. The former type of measurement error is potentially more dangerous, as it would automatically generate an attenuation bias of coefficient estimates. Starting with the problem on how to measure enforcement, firms respond both to the probability of being sanctioned as well as the amount of the sanction to be expected in case of non-compliance ([Ronconi, 2010](#)). Ideally, a measure of labour law enforcement should therefore include both the threat of the sanction (e.g. as captured by the number of inspectors and/or the number of inspections) as well as the size of the sanction (e.g. as captured by the amount of the fine). In practice, data on labour inspection is often very scant and only one of the two dimensions of enforcement is used. Most papers use the number of inspectors ([Almeida and Carneiro, 2012](#); [Bhorat et al., 2012](#); [Ronconi, 2010](#)) or the number of inspections ([Almeida and Poole, 2017](#)) to construct the treatment indicator. This is generally normalised by the population levels or the number of firms in the area of interest (i.e. city, region) in order to take into account of size effects. [Bhorat et al. \(2012\)](#) complement data on the number of inspectors with information on the allocated budget by the Ministry of Labour and the number of labour centres in the province.

in Costa Rica. They find that the campaign had a positive effect on the wages of those earning below the minimum wage and possible spillover effects on the provision of other mandated benefits.

Discussing the measurement of the outcomes of interest, most studies use information on compliance reported by workers in household surveys ([Almeida and Carneiro, 2012](#); [Bhorat et al., 2012](#); [Ronconi, 2010](#)). Indeed, firms are likely to under-report non-compliance with the legislation and the measurement error is also expected to be correlated with the level of enforcement ([Ronconi, 2010](#)).²⁷ Even when using information from household surveys, measures of law compliance are generally self-reported and this might lead to measurement error. For this reason, particular attention needs to be paid to the indicators of labour law compliance which are taken into consideration. Labour legislation is generally very complex and several rules regulate working conditions for different groups of individuals. Additionally, labour inspectors can be assigned to priority areas. Accordingly, papers focus on either few selected indicators or rather analyse the effects on different measures of labour law compliance based on the content of the labour code in the country and the institutional characteristics of the system of labour inspection. These measures have included non-compliance with the minimum wage ([Bhorat et al., 2012](#)), formal employment ([Almeida and Carneiro, 2009](#)), the presence of an employment contract and pension contributions ([Viollaz, 2018a,b](#)), the right to paid holidays and compliance with the legislation over working hours ([Ronconi, 2010](#)).

²⁷Additionally, surveys of enterprises generally only cover registered firms.

B Appendix: Allocation rule

This Appendix provides details of the allocation rule that was used to distribute vacancies of inspectors across Colombian departments over the four hiring rounds. The Ministry of Labour made available to the author the normalized score obtained by each department in each hiring round between 2011 and 2014 (from which I derive the target number of inspectors that should have been hired in a given department and year) as well as the number of inspectors actually working in each department and year between 2008 and 2015 (from which I compute the yearly change in the number of inspectors in a department, which might differ from the target number due to implementation delays as discussed in the paper). While the indicators entering the allocation rule were set through Ministerial decrees and are therefore public, the actual score obtained by each department on these indicators have been made available confidentially. The composition of the allocation rule for 2011 is discussed in the main text (see Figure 2 in Section 2.1), while here I present the indicators entering the allocation rule in the other hiring rounds.

For the 2012 hiring round, the allocation rule resulted from 15 different indicators (Figure B1). Some of them were the same as in the previous hiring round (e.g. number of cooperatives and temporary agency firms, previous coverage of the system of labour inspection), but the majority of indicators changed. These were not formally grouped into clusters as in 2011, but they can nevertheless be re-conducted into broad family groups. In particular, four indicators meant to capture reported cases of violations of the legislation (i.e. child labour, workplace harassment, occupational hazards and other sanctions). Additionally, the list of priority sectors was expanded to include also transportation, construction, mining and trade. Differently from the 2011 hiring round, the indicators entering the allocation rule also had different weights. In particular, the highest weights were assigned to indicators on the previous coverage of the system of labour inspection as well as the relative importance of the priority sectors (which both accounted for 20 per cent of the final score) while the lowest weights concerned the indicators capturing the number of temporary agency firms and cases of workplace harassment (each accounting for 5 per cent of the final score).

Finally, vacancies in 2013 and 2014 were distributed following the same allocation rule. This does not mean that departments had the same final score, as the total number of inspectors to be allocated nationwide differed (i.e. a larger number of inspectors was kept for the central level in 2013). Nevertheless, there is perfect correlation between the scores assigned in the two years. The allocation rule used in these two hiring rounds was based on nine different indicators, generally corresponding to a mix of the indicators already used in the two previous hiring rounds.

Figure B1: Criteria determining the final allocation rule for the 2012 hiring round

DEFINICIÓN CRITERIOS DE ESTIMACIÓN		
CRITERIO	VARIABLES	FUENTE
COOPERATIVAS Y PRECOOPERATIVAS	AUTORIZADAS, INVESTIGADAS, VISITADAS, SANCIONADAS	INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
EMPRESAS DE SERVICIOS TEMPORALES	AUTORIZADAS, INVESTIGADAS, VISITADAS, SANCIONADAS, SUSPENDIDAS, CANCELADAS	INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
TRABAJO INFANTIL	SOLICITUDES, AUTORIZADAS, NEGADAS, REVOCADAS, VISITAS, SANCIONES	INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
ACOSO LABORAL	INVESTIGACIONES	INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
OTRAS ACTUACIONES ADMINISTRATIVAS	CONSULTAS, CONCILIACIONES, VISITAS, INVESTIGACIONES, SANCIONES, CESES	INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
RIESGOS PROFESIONALES	INVESTIGACIONES, VISITAS, SANCIONES, VALOR MULTAS, ARP INVESTIGADAS, ARP VISITADAS, REGISTRO COMITÉS INTERDISCIPLINARIOS	INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
TRANSPORTE	SOCIEDADES COMERCIALES CONSTITUIDAS, VISITAS DE INSPECCIÓN	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE, INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
MINERÍA	SOCIEDADES COMERCIALES CONSTITUIDAS, VISITAS DE INSPECCIÓN	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE, INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
COMERCIO	SOCIEDADES COMERCIALES CONSTITUIDAS, VISITAS DE INSPECCIÓN	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE, INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
CONSTRUCCIÓN	SOCIEDADES COMERCIALES CONSTITUIDAS, VISITAS DE INSPECCIÓN	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE, INFORME ESTADÍSTICO DIRECCIONES TERRITORIALES
FLORES	NÚMERO DE HECTARIAS CULTIVADAS	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE
CAÑA DE AZÚCAR	PRODUCCIÓN NACIONAL DE TONELADAS DE CAÑA	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE
PALMA DE ACEITE	PRODUCCIÓN NACIONAL DE TONELADAS DE PALMA PARA FABRICACIÓN DE ACEITE	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE
PUERTOS MARÍTIMOS	PARTICIPACIÓN DE LOS PUERTOS MARÍTIMOS EN MOVIMIENTO DE TONELADAS DE IMPORTACIÓN Y EXPORTACIÓN	INFORME DE COYUNTURA ECONÓMICA REGIONAL ICER, BANCO DE LA REPÚBLICA Y DANE
COBERTURA DE INSPECCIÓN	NÚMERO INSPECTORES, NÚMERO INSPECCIONES, NÚMERO DE MUNICIPIOS POR TERRITORIAL	JURISDICCIONES DIRECCIONES TERRITORIALES

Source: Colombian Ministry of Labour.

Note: The figure reports the indicators that entered the allocation rule in the second hiring round conducted as part of the Action Plan. The first column reports the criteria, the second one details the specific indicators and the third one specifies the sources from where the information was collected.

In particular, labour inspectors were allocated according to the previous coverage of the system of labour inspection, the number of investigations, consultations and visits conducted as well as the amount of fines imposed. Additionally, inspectors were also allocated according to the reported cases of child labour and indicators of occupational hazards. New indicators were also introduced for the first time in these two last hiring rounds, to assign more inspectors in areas characterised by a higher prevalence of collective bargaining conflicts and greater use of labour subcontracting (Figure B2).

Figure B2: Criteria determining the final allocation rule for the 2013 and 2014 hiring rounds

DEFINICIÓN CRITERIOS DE ESTIMACIÓN		
CRITERIO	VARIABLES	FUENTE
ATENCIÓN AL CIUDADANO	CONSULTAS VERBALES ATENDIDAS Y AUDIENCIAS DE CONCILIACIÓN	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
INVESTIGACIONES ADMINISTRATIVO LABORALES	INVESTIGACIONES ADMINISTRATIVAS POR ACTIVIDAD ECONÓMICA E INVESTIGACIONES ADMINISTRATIVAS TRAMITADAS	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
VISITAS DE INSPECCIÓN Y VIGILANCIA	VISITAS DE CARÁCTER GENERAL REALIZADAS POR ACTIVIDAD ECONÓMICA Y VISITAS DE INSPECCIÓN A SECTORES DE LA ECONOMÍA FOCALIZADOS COMO CRÍTICOS	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
TRABAJO INFANTIL	SOLICITUDES DE NIÑOS Y NIÑAS PARA DESEMPEÑAR ACTIVIDADES REMUNERADAS Y TRÁMITE DE SOLICITUDES DE ADOLESCENTES PARA TRABAJAR	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
TERCERIZACIÓN	ACTUACIONES ADMINISTRATIVAS FRENTE A LAS COOPERATIVAS Y PRECOOPERATIVAS DE TRABAJO ASOCIADO, EMPRESAS DE SERVICIOS TEMPORALES Y AGENCIAS DE COLOCACIÓN DE EMPLEO	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
SANCIONES	NÚMERO DE SANCIONES EJECUTORIADAS INTERPUESTAS POR VIOLACIÓN A LAS NORMAS LABORALES Y POR EVASIÓN Y ELUCIÓN; VALOR DE LAS MULTAS DEBIDAMENTE EJECUTORIADAS	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
CONFLICTOS COLECTIVOS	DENUNCIAS DE PACTOS Y CONVENCIONES COLECTIVAS, HUELGAS REALIZADAS Y ACTAS DE CONSTATAción DE CESES DE ACTIVIDADES	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
RIESGOS LABORALES	VISITAS DE INSPECCIÓN PARA VERIFIAR CUMPLIMIENTO DE NORMAS DE RIESGOS LABORALES Y SALUD OCUPACIONAL, ACTUACIONES ADMINISTRATIVAS FRENTE A LAS JUNTAS DE CALIFICACIÓN DE INVALIDEZ Y ADMINISTRADORAS DE RIESGOS LABORALES	INFORME ESTADÍSTICO MENSUAL DE LAS TERRITORIALES
PLANTA DE PERSONAL: COBERTURA DE INSPECCIÓN	NÚMERO DE INSPECTORES DE TRABAJO, NÚMERO DE INSPECCIONES Y NÚMERO DE MUNICIPIOS POR TERRITORIAL	JURISDICCIONES TERRITORIALES

Source: Colombian Ministry of Labour.

Note: The figure reports the indicators that entered the allocation rule in the third and fourth hiring rounds conducted as part of the Action Plan. The first column reports the criteria, the second one details the specific indicators and the third one specifies the sources from where the information was collected.

C Appendix: Robustness tests

This Appendix presents the robustness tests of the analysis, whose results are also summarised in Section 5.3 in the paper. For ease of exposition, these tests are conducted on the most complete specification (i.e. which controls for the third order polynomial of the decimal part of the final score, allowed to vary on the two sides of the cutoff) unless otherwise specified. Most robustness tests are presented in Tables C1 and C2 for measures of labour law compliance and status in the labour market, respectively. In each of the Tables, I also reproduce in Column 1 the baseline results of the analysis (i.e. corresponding to the results presented in Column 2 in Tables 7 and 8 in the main text).

As explained in Section 4.2, the empirical strategy of the paper differs from a pure RDD for the fact that the change in the number of inspectors and the dummy for being above the .5 cutoff are constructed cumulatively over time in order to have a strong enough first stage relationship. However, this imposes additional assumptions compared to the standard RDD setting. In this sense, it might be interesting to compare the results obtained with this modified specification to the ones of a normal RDD (which would be however plagued by a weak instrument problem, see in particular Table 3 in the paper). Figure 11 in Section 5.3 summarises the results of the RDD analysis with respect to measures of labour law compliance (panel A) and indicators of status in the labour market (panel B). The specification run is the one with no controls for the decimal part of the allocation rule. This is because the F test for the instrument has a low value (i.e. 2.869), which would be even lower in the full specification. The results show how the main findings of the analysis are confirmed even if using a fuzzy RDD design with a weak instrument. In particular, all indicators for measures of labour law compliance are positive and close to significance. The magnitude of the effects is also very similar to the one presented in the main analysis. The same picture emerges when looking at indicators for status in the labour market, where the RDD results point towards an increase in formal employment and the absence of detrimental effects on employment, unemployment and inactivity.²⁸

Additionally, one potential problem relates to the presence of few units of treatment variation (i.e. corresponding to the combination of 23 departments over four hiring rounds, leading to the definition of 92 clusters). In order to test the sensitivity of the results to this issue, I perform the analysis at the department and year level (i.e. the unit of observation becomes the

²⁸ Another test in the same spirit would be to restrict the dimension of the bandwidth considered in the analysis (i.e. the baseline specification uses the entire set of decimal values within each integer). However, the presence of few units of treatment variation in practice makes necessary to use the entire bandwidth. However, this should not be a problem given that observed heterogeneity is accurately accounted for when restricting the analysis to departments within the same integer (see Table 5 in Section 4.1).

department rather than the individual). The results are available in Column 2 of Tables C1 and C2. Despite the low sample size, they confirm the main findings of the analysis. In particular, almost all coefficients have the same sign and similar magnitude than those obtained in the baseline specification and most of them retain statistical significance at conventional levels.²⁹ In the same vein, I return to the baseline specification at the individual level but now cluster standard errors at the department level (i.e. rather than at the department and year level). This represents a more conservative approach that takes into account the possible presence of serial correlation within departments over years, but it relies on the presence of few clusters. The results of this test are reported in Column 3 of Tables C1 and C2 and they show how almost all coefficients retain statistical significance even when clustering at the department level.

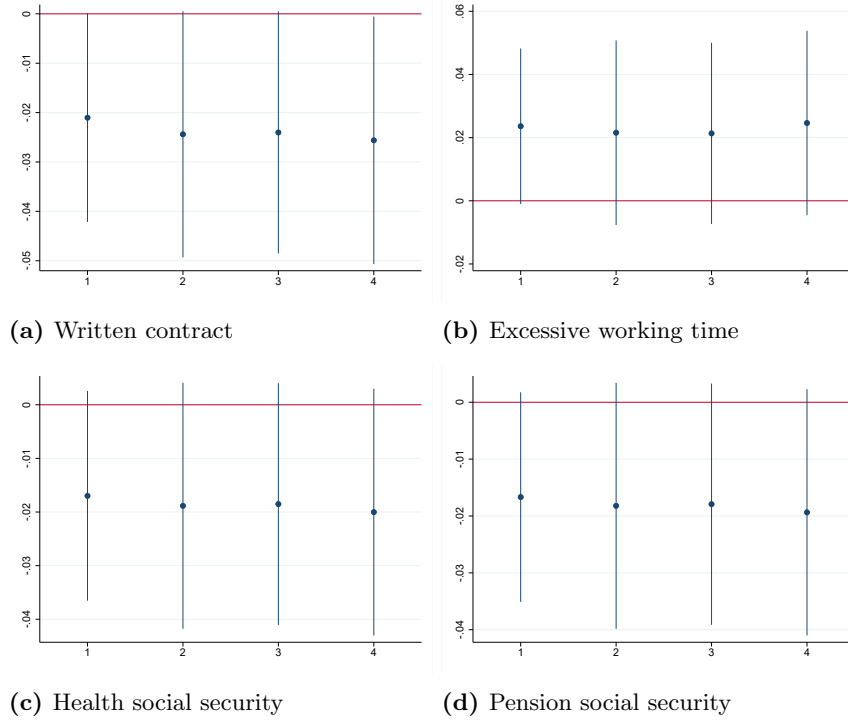
The institutional framework that generates the exogenous variation in treatment intensity gives rise to some testable implications. In particular, the paper exploits discontinuities in the allocation of labour inspectors occurring at .5 within each integer of the final score. This means that the relative 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.³⁰ Figure C1 presents the results of this exercise for selected measures of labour law compliance, while the full set of results is available from the author upon request. 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, treatment effects are smaller for departments characterised by a lower change in enforcement levels. This result is consistent across all the indicators presented, with the coefficient of the interaction terms often close to statistical significance.

A final set of robustness tests aims to check some miscellaneous concerns. First, I run the baseline regression but without individual level controls for sex, age, location of residence (i.e. urban or rural) and highest level of education. This is done to check whether the results discussed in the main text rely on the inclusion of these covariates, which should instead simply improve the precision of the estimates. Results are presented in Column 4 in Tables C1 and C2 and they show how coefficients remain largely unchanged when excluding individual level controls. Additionally, I exclude all departments who in a given hiring round do not have

²⁹Results would be even closer to those discussed in the main text if I would not include the control for the decimal part of the final score, which would increase the strength of the first stage.

³⁰For 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.

Figure C1: Difference in treatment effects for departments positioned at different integer values of the allocation rule: selected measures of labour law compliance



Notes: The graph plots points estimates and 90 per cent confidence intervals for the interaction terms between the treatment indicator and dummies for the integer values of the final score. This is done to test whether treatment effects differ across departments characterised by differences in the relative strength of the exogenous variation in public enforcement. The numbers reported on the x axis correspond to the the integer values of the final score (e.g. point estimates plotted above one correspond to the results of the interaction between the treatment indicator and the integer value one). The omitted category is constituted by departments with an integer value equal to zero, so results should be interpreted relative to this group. Departments with a final score equal to or above four have been grouped together in this residual group.

another department that lies on the other side of the .5 cutoff within the same integer. This is done to increase the comparability of departments in the sample. The results are available in Column 5 of Tables C1 and C2 and they show that, despite the reduction in sample size related to the fact that some departments exit the sample, coefficient estimates are very similar to those discussed in the main text both in terms of magnitude and significance. I also exclude departments that in all the four hiring rounds have been always above or below the .5 cutoff used for identification. While Section 4.1 has shown how manipulation of the running variable is unlikely to have happened, this is done to test whether results would still hold even after excluding departments that might have managed to always self-select either above or below the cutoff. The results of this exercise are available in Column 6 of Tables C1 and C2 and once again are in line with those discussed above. Finally, I run the two baseline specifications using as outcomes of interest the individual level controls used in the main analysis. This is supposed to further check that observed heterogeneity does not evolve discontinuously around the cutoff. The results are available in Table C3 and they show than none of the coefficients is statistically significant at conventional levels in the most complete specification.

Table C1: Robustness tests: measures of labour law compliance

	(1)	(2)	(3)	(4)	(5)	(6)
Written contract	0.00256** (0.000996)	0.000988** (0.000500)	0.00256** (0.00121)	0.00215** (0.000905)	0.00145** (0.000714)	0.00147** (0.000650)
Excessive working time	-0.00648* (0.00365)	-0.00535** (0.00220)	-0.00648 (0.00524)	-0.00672* (0.00370)	-0.000594 (0.00161)	-0.00341 (0.00243)
Severance payments	0.00289*** (0.00104)	0.00188*** (0.000524)	0.00289** (0.00143)	0.00256*** (0.000963)	0.00118 (0.000752)	0.00201** (0.000811)
Paid holidays	0.00144 (0.00129)	0.00152* (0.000838)	0.00144 (0.00128)	0.00113 (0.00128)	-0.000620 (0.00157)	0.000731 (0.00119)
Parental leave	-0.000562 (0.00146)	0.00116 (0.00119)	-0.000562 (0.00141)	-0.000753 (0.00149)	0.00172* (0.00103)	-0.000193 (0.00134)
Health social security	0.00200** (0.000872)	0.000532 (0.000466)	0.00200* (0.00110)	0.00162** (0.000805)	0.000729 (0.000748)	0.00150** (0.000712)
Pension social security	0.00213** (0.000857)	0.000422 (0.000558)	0.00213* (0.00112)	0.00177** (0.000793)	0.00131 (0.000851)	0.00153** (0.000688)
N	2,377,658	92	2,377,658	2,377,658	1,482,318	1,955,780
F test	13.4172	4.751	8.62	13.4171	12.5277	15.699
Main controls						
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score*Above	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls						
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time trends	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects*Time trends	Yes	Yes	Yes	Yes	Yes	Yes
Individual level characteristics	Yes	Yes	Yes	No	Yes	Yes

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on different measures of labour law compliance. Reported coefficients correspond to the parameter β_1 in equation 5 in the main text. The definition of the outcomes of interest is available in Table F1 in Appendix F. Column (1) presents the baseline results and corresponds to column (2) in Table 7; Column (2) conducts the analysis at the department level; Column (3) clusters standard errors at the department level; Column (4) does not include individual level controls; Column (5) excludes from the analysis departments that do not have another department on the other side of the cutoff within the same integer; Column (6) excludes from the analysis departments that over the different hiring rounds have always been either above or below the cutoff. Further details on these robustness tests are available in Appendix C. * <10%, ** <5%, *** <1%.

Table C2: Robustness tests: status in the labour market, hours worked and wages

	(1)	(2)	(3)	(4)	(5)	(6)
Employed	0.00257* (0.00150)	-0.000625 (0.00211)	0.00257 (0.00166)	0.00216 (0.00148)	0.00292*** (0.00101)	0.00256* (0.00134)
Unemployed	-0.000512 (0.000868)	0.00197* (0.00118)	-0.000512 (0.00109)	-0.000650 (0.000876)	0.000297 (0.000548)	0.000105 (0.000459)
Inactive	-0.00206 (0.00194)	-0.000848 (0.00223)	-0.00206 (0.00242)	-0.00151 (0.00196)	-0.00322*** (0.00104)	-0.00266* (0.00150)
Formal employment	0.00260** (0.00103)	0.000914 (0.000675)	0.00260* (0.00134)	0.00216** (0.000962)	0.00168** (0.000780)	0.00139** (0.000648)
Formal economy	0.00202** (0.000938)	0.00140** (0.000676)	0.00202* (0.00111)	0.00154* (0.000890)	0.00118** (0.000555)	0.000978 (0.000620)
Formal employment in formal economy	0.00257** (0.00103)	0.000951 (0.000657)	0.00257* (0.00133)	0.00213** (0.000953)	0.00163** (0.000752)	0.00136** (0.000631)
Informal employment in formal economy	-0.000551* (0.000309)	0.000448 (0.000326)	-0.000551 (0.000405)	-0.000592* (0.000321)	-0.000449 (0.000310)	-0.000381 (0.000263)
N	2,377,658	92	2,377,658	2,377,658	1,482,318	1,955,780
F test	13.4172	4.751	8.62	13.4171	12.5277	15.699
Main controls						
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score*Above	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls						
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time trends	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects*Time trends	Yes	Yes	Yes	Yes	Yes	Yes
Individual level characteristics	Yes	Yes	Yes	No	Yes	Yes

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on indicators of status in the labour market, hours worked and wages. Reported coefficients correspond to the parameter β_1 in equation 5 in the main text. The definition of the outcomes of interest is available in Table F1 in Appendix F. Column (1) presents the baseline results and corresponds to column (2) in Table 7; Column (2) conducts the analysis at the department level; Column (3) clusters standard errors at the department level; Column (4) does not include individual level controls; Column (5) excludes from the analysis departments that do not have another department on the other side of the cutoff within the same integer; Column (6) excludes from the analysis departments that over the different hiring rounds have always been either above or below the cutoff. Further details on these robustness tests are available in Appendix C. * <10%, ** <5%, *** <1%.

Robustness tests: status in the labour market, hours worked and wages (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
Informal employment in informal economy	0.000528 (0.00148)	-0.00199 (0.00210)	0.000528 (0.00201)	0.000588 (0.00146)	0.00169 (0.00108)	0.00155 (0.00116)
Number of hours worked in main job	-0.110 (0.120)	-0.477*** (0.171)	-0.110 (0.172)	-0.137 (0.125)	0.0314 (0.0680)	-0.0255 (0.0799)
Number of hours worked overall	-0.117 (0.131)	-0.482*** (0.173)	-0.117 (0.187)	-0.143 (0.135)	0.0353 (0.0716)	-0.0166 (0.0838)
Income from main job	-9,478 (6,527)	-9,168* (5,384)	-9,478 (9,154)	-11,257 (7,303)	2,139 (1,301)	-1,691 (2,152)
Overall labour income	-9,749 (7,147)	-2,373 (2,824)	-9,749 (9,801)	-12,181 (8,285)	5,805* (3,160)	-2,693 (3,421)
N	2,377,658	92	2,377,658	2,377,658	1,482,318	1,955,780
F test	13.4172	4.751	8.62	13.4171	12.5277	15.699
Main controls						
Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score*Above	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls						
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time trends	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects*Time trends	Yes	Yes	Yes	Yes	Yes	Yes
Individual level characteristics	Yes	Yes	Yes	No	Yes	Yes

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on indicators of status in the labour market, hours worked and wages. Reported coefficients correspond to the parameter β_1 in equation 5 in the main text. The definition of the outcomes of interest is available in Table F1 in Appendix F. Column (1) presents the baseline results and corresponds to column (2) in Table 7; Column (2) conducts the analysis at the department level; Column (3) clusters standard errors at the department level; Column (4) does not include individual level controls; Column (5) excludes from the analysis departments that do not have another department on the other side of the cutoff within the same integer; Column (6) excludes from the analysis departments that over the different hiring rounds have always been either above or below the cutoff. Further details on these robustness tests are available in Appendix C. * <10%, ** <5%, *** <1%.

Table C3: Robustness tests: running regression on covariates

	(1)	(2)	Mean below cutoff
Male	-0.000719** (0.000284)	-0.00101* (0.000586)	0.457
Age	0.00442 (0.0150)	0.0477 (0.0350)	39.814
Intermediate level of education	-0.000438 (0.000310)	-0.00178* (0.000945)	0.186
Rural	0.000456 (0.000441)	0.00113 (0.000766)	0.906
N	2,377,658	2,377,658	
F test	34.2776	13.4172	
Main controls			
Integer of the final score FE	Yes	Yes	
Polynomial of order 3 for decimal of the final score	No	Yes	
Polynomial of order 3 for decimal of the final score*Above	No	Yes	
Baseline controls			
Department fixed effects	Yes	Yes	
Time trends	Yes	Yes	
Department fixed effects*Time trends	Yes	Yes	
Individual level characteristics	Yes	Yes	

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on the individual level characteristics included as covariates in the baseline specification. Reported coefficients correspond to the parameter β_1 in equation 5 in the main text. Standard errors are clustered at the department and year level, leading to the identification of 92 clusters. The two columns differ for whether the specification includes a control for the third order polynomial of the decimal part of the final score, which is allowed to vary on the two sides of the cutoff. * $<10\%$, ** $<5\%$, *** $<1\%$.

D Appendix: A simple theoretical framework

This Appendix introduces a simple theoretical framework to interpret the empirical findings of the analysis. The main intuition behind this exercise is summarised in Section 6 in the paper. To start with, I follow [Almeida and Carneiro \(2012\)](#) in studying the effects of an increase in public enforcement within a competitive labour market with no search frictions. The labour market is composed of two sectors (i.e. the formal and the informal) characterised by different wages (W_f and W_i), labour demands (L_f^D and L_i^D) and labour supplies (L_f^S and L_i^S). Employers can hire both formal and informal workers as in [Ulyssea \(2018\)](#) and in equilibrium there is no unemployment.³¹ In addition to the wage, employers that hire formal workers need to pay a tax T . This is used to finance the provision of mandated benefits (e.g. social security, severance payments). Workers value the provision of these benefits at a value vT , with $v > 0$. Since it is illegal to hire workers informally, employers that do so face a penalty P . This can be thought as the product between the amount of the fine paid and the probability of being fined. This labour market is characterised by the following equations:

$$L_f^D = \alpha - \beta(W_f + T) + \gamma(W_i + P)$$

$$L_i^D = \delta + \epsilon(W_f + T) - \zeta(W_i + P)$$

$$L_f^S = \eta + \theta(W_f + vT) - \iota W_i$$

$$L_i^S = \kappa - \lambda(W_f + vT) + \mu W_i$$

$$L_f^D = L_f^S$$

$$L_i^D = L_i^S$$

As in [Almeida and Carneiro \(2012\)](#), I model the increase in public enforcement as an increase in T . This makes sense given that the empirical results have shown how the assignment of an additional inspector increases the probability that workers will receive a number of mandated benefits. In theory, an increase in public enforcement might also raise the penalty faced by employers for hiring informal workers (P). However, the empirical findings have shown that increasing public enforcement does not have any effect on the informal sector of the economy and it only marginally decreases the probability of being employed informally in a formal enterprise. Additionally, the share of informal workers in the formal sector is quite small in Colombia compared to other countries in the region. For ease of exposition, I therefore discuss

³¹In line with the empirical part of the paper, the focus is on workers' formal status within a formal or informal enterprise. I ignore the decision of the enterprise to operate in the formal or the informal sector.

only the case in which an increase in public enforcement translates into an increase in T . I equate demand and supply in both sectors and take the derivative of the solutions with respect to T . In order to solve the system, I need to make some simplifying assumptions. Following [Almeida and Carneiro \(2012\)](#), I assume that $\gamma = \epsilon = 0$. This implies that there are no linkages between the formal and the informal sector in labour demand, which is unrealistic but does not fundamentally change the mechanics of interest. Denoting derivatives with lower case letters, the effects on an increase in T on equilibrium wages and employment in the two sectors correspond to the following expressions:

$$\begin{aligned} l_f^D &= l_f^S = \frac{(1-v)\beta(\iota\lambda - \theta\mu - \theta\zeta)}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \\ l_i^D &= l_i^S = \frac{(1-v)\lambda\beta\zeta}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \\ w_f &= \frac{v\iota\lambda - (\beta + v\theta)(\mu + \zeta)}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \\ w_i &= \frac{(v-1)\lambda\beta}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \end{aligned}$$

In order to study the sign of these derivatives, additional assumptions are needed. First, I assume that all parameters are positive. This is quite conventional, provided that formal and informal labour are substitutes. Secondly, I need to assume that $\theta\mu > \iota\lambda$. This is also not particularly problematic, as the assumption would fail only if cross effects on wages on labour supply (i.e. from one sector to the other) are stronger than own effects (i.e. within sectors). Under these assumptions, I obtain the following conditions for the sign of the derivatives:

$$\begin{aligned} l_f^D, l_f^S &> 0 \text{ if } v \geq 1; < 0 \text{ if } v < 1 \\ l_i^D, l_i^S &> 0 \text{ if } v \leq 1; < 0 \text{ if } v > 1 \\ w_f &< 0 \\ w_i &> 0 \text{ if } v \geq 1; < 0 \text{ if } v < 1 \end{aligned}$$

This shows that the empirical results presented in the paper would be consistent with a perfectly competitive labour market only under the assumption that $v > 1$. This means that workers value one unit of benefit more than its cost to employers. Under this condition, they would be willing to accept a reduction in wages that more than compensates the increase in labour costs resulting from the increased provision of benefits. In the absence of any frictions, the strengthening of public enforcement has made possible the emergence of an equilibrium characterised by lower wages but higher benefits which is preferred by both employers and workers. However, this equilibrium would have not been emerged naturally in the absence of

perfect commitment by the enterprise to provide these benefits.

It is theoretically possible to assume a value of v greater than one. For instance, in most countries the provision of benefits (unlike profits) is untaxed so their cost for employers is smaller than the monetary transfer received by workers (Almeida and Carneiro, 2012). However, it is difficult to think that such a high rate of pass through exists in practice. This is because workers are only imperfectly informed about the benefits they are entitled to, so they might prefer to receive higher wages instead. Additionally, the probability of receiving these benefits is generally smaller than one, it lies on a distant future and there is a component of insurance whose value depends on each individual's risk profile. Accordingly, there is a vast empirical literature, including studies on Colombia, showing that employers are not able to entirely pass the costs of mandated benefits (Arulampalam et al., 2012; Crucesa et al., 2010; Fuest et al., 2018; Gruber, 1994, 1997; Kugler, 2005; Kugler and Kugler, 2009; Serrato and Zidar, 2016).

I then need to depart from this model of a perfectly competitive labour market if I want to interpret my results without assuming an unrealistically high value of v . Almeida and Carneiro (2012) do so by including downward wage rigidities due to the presence of minimum wages. However, that behaviour, although possibly taking place also in the present context, is not consistent with the increase in formal employment and the lack of overall employment adjustment that I observe in the data. For these reasons, I augment the model by including a parameter ρ , which multiplies the amount of tax paid by the employer in the equations for labour demand. This parameter captures how much the demand for formal employment decreases following an increase in taxes. In the model of the labour market with no frictions, this adjustment was supposed to be perfect. I instead assume that $\rho \in (0, 1)$, meaning that the demand for formal employment decreases less than one following a one unit increase in the tax paid by employers. A value of ρ smaller than one is consistent with (but not limited to) the presence of search frictions as in Meghir et al. (2015). In this case, the lower the value of ρ (i.e. approaching zero), the larger the role played by search frictions. The model becomes as follows:

$$L_f^D = \alpha - \beta(W_f + \rho_f T) + \gamma(W_i + P)$$

$$L_i^D = \delta + \epsilon(W_f + \rho_i T) - \zeta(W_i + P)$$

$$L_f^S = \eta + \theta(W_f + vT) - \iota W_i$$

$$L_i^S = \kappa - \lambda(W_f + vT) + \mu W_i$$

$$L_f^D = L_f^S$$

$$L_i^D = L_i^S$$

To note that I still assume that demand and supply equate in the two labour markets, but this could be easily relaxed without changing the results which are relevant to study the effects of an increase in T . Following the same steps and assumptions outlined above, I obtain the following solutions for the effect of an increase in enforcement on equilibrium wages and employment in the two sectors:

$$\begin{aligned} l_f^D &= l_f^S = \frac{(\rho_f - v)\beta(\iota\lambda - \theta\mu - \theta\zeta)}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \\ l_i^D &= l_i^S = \frac{(\rho_f - v)\lambda\beta\zeta}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \\ w_f &= \frac{v\iota\lambda - (\beta\rho_f + v\theta)(\mu + \zeta)}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \\ w_i &= \frac{(v - \rho_f)\lambda\beta}{(\beta + \theta)(\mu + \zeta) - \iota\lambda} \end{aligned}$$

Which generate this set of conditions for the sign of the derivatives:

$$\begin{aligned} l_f^D, l_f^S &>= 0 \text{ if } v >= \rho_f; < 0 \text{ if } v < \rho_f \\ l_i^D, l_i^S &>= 0 \text{ if } v <= \rho_f; < 0 \text{ if } v > \rho_f \\ w_f &< 0 \\ w_i &>= 0 \text{ if } v >= \rho_f; < 0 \text{ if } v < \rho_f \end{aligned}$$

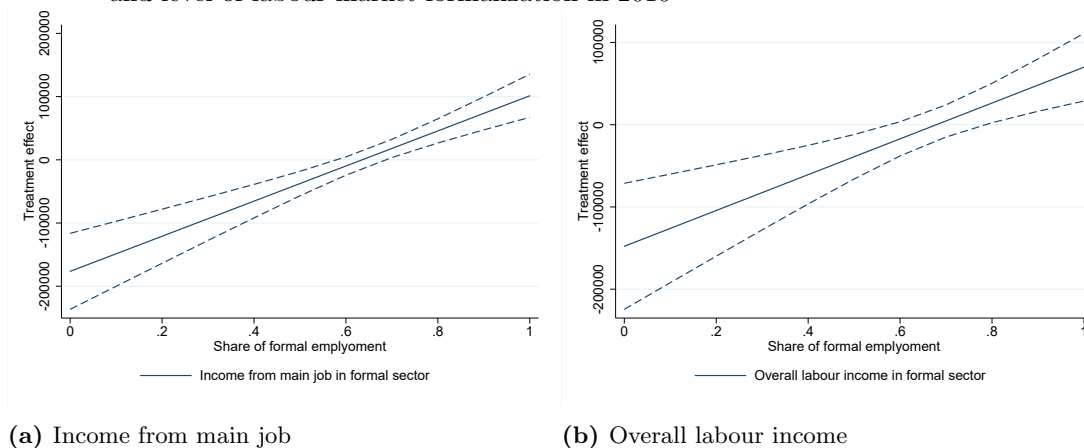
The empirical results obtained in the paper are consistent with this model provided that $v >= \rho_f$. This represents a less binding condition than before and it implies that the value placed by workers on the provision of benefits is greater than the reduction in the demand for formal employment following a one unit increase in the amount of taxes paid on formal employment. Intuitively, if labour demand can perfectly adjust (i.e. $\rho_f = 1$), then I would need to assume an unrealistically high value placed by workers on the benefits to explain the shift in employment towards the formal sector despite the increase in labour costs (i.e. wages in the formal sector would need to decrease by a lot for this to happen). The larger the extent of search frictions (i.e. as ρ_f approaches zero), the less likely employers are to reduce labour demand following an increase in taxation. In the extreme case in which labour demand in the formal sector cannot adjust ($\rho_f = 0$), the results presented in the paper would be consistent with any non-negative valuation of the benefits by workers (i.e. $v > 0$).

This modified version of the model has some testable implications. In particular, the solution for w_f shows that the wage decrease will be smaller in labour markets characterised by a lower value of ρ_f . While I do not directly observe ρ_f , I can approximate it with the

degree of formalization of a given labour market. To do so, I compute the share of formal sector employment within a two digit sector in a given department. The assumption is that workers and enterprises operate in a labour market which is defined by their economic sector of activity within a broad geographical region (i.e. the department). This level of formalization is measured in 2010, in order to avoid any confounding effect from the increase in public enforcement. The intuition is that, in labour markets characterised by higher shares of formal sector employment, employers will be less likely to reduce formal wages following an increase in taxes because workers can more easily threaten to move to a different formal job in case of wage cuts.

In order to empirically test this hypothesis, I augment the baseline specification by including an interaction term between the treatment indicator and the measure of labour market formalization introduced above. The effects of this interaction term on overall labour income and income from the main job in the formal sector are plotted below. As a reminder, the analysis has shown that wages in the formal sector decrease following the strengthening of public enforcement, possibly because employers pass some of these costs to their workers (see Section 5.2). This exercise confirms the predictions of the model, notably that wage decreases in the formal sector are particularly strong in labour markets characterised by low levels of formalization. In a labour market where around 65 per cent of employment occurs in the formal sector, employers are instead not able to pass any costs of the increased provision of mandated benefits to their workers (Figure D1). However, most workers are employed in labour markets characterised by a lower level of formal sector employment (i.e. the median is 43 per cent).

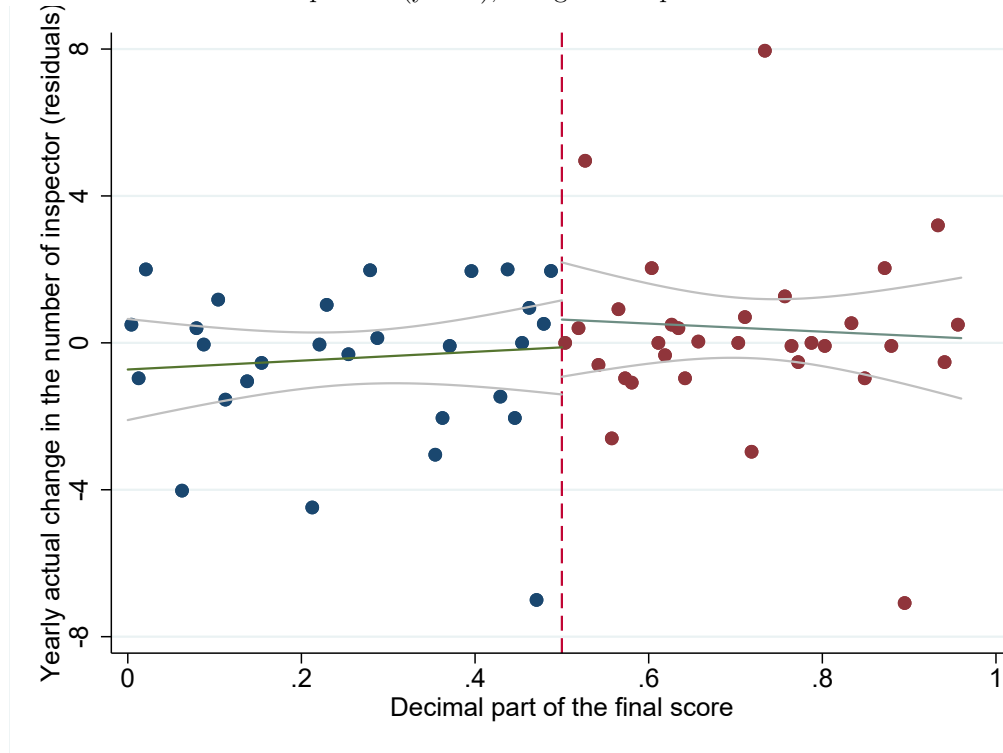
Figure D1: Treatment effects on wages in the formal sector: interaction between treatment indicator and level of labour market formalization in 2010



Notes: The graph plots treatment effects and 90 per cent confidence interval of an augmented specification, where I include the interaction term between the treatment indicator (i.e. actual change in the number of inspectors in a given department and year) and the level of formalization of the labour market within which the individual operates. This is proxied by the share of employment in the formal sector of the economy in a given two digit sector and department, measured in 2010. The results show that wage decreases in the formal sector (when considering both overall labour income or income from the main job) are larger in labour markets characterised by an initially low share of formal sector employment.

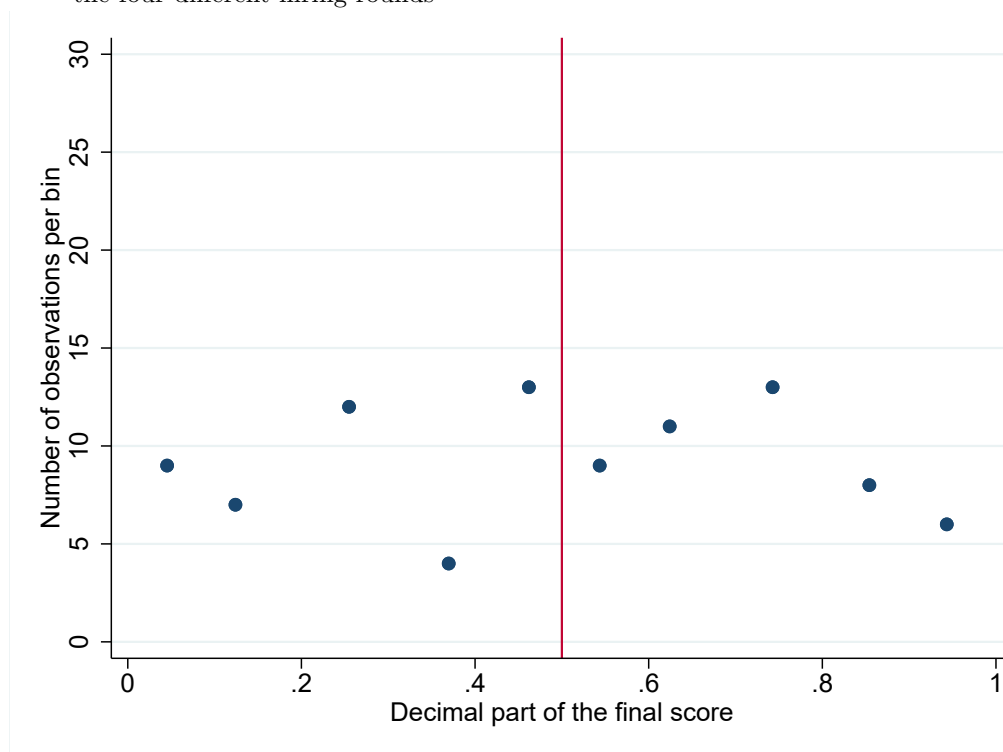
E Appendix: Additional figures

Figure E1: Relationship between decimal part of the final score (x axis) and the yearly actual change in the number of inspectors (y axis), using contemporaneous values



Note: The graph shows the relationship between the decimal part of the final score and the actual yearly change in the number of inspectors in a given department between 2011 and 2014, controlling for the integer part of the allocation rule (i.e. plotting residuals). The figure also includes the line of best fit and the 95 per cent confidence interval. Since controls for the integers of the allocation rule are included, in theory (i.e. if the policy was perfectly implemented) all departments to the left of the cutoff should report a value of zero and all departments to the right should report a value of one.

Figure E2: Number of departments whose decimal part of the final score falls within a given bin, over the four different hiring rounds



Note: The graph shows the number of units of treatment variation (i.e. corresponding to 23 departments over four 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.

F Appendix: Additional tables

Table F1: Definition of the outcomes of interest used in the analysis

Measures of labour law compliance	
Variable	Definition
Written contract	The individual has a written employment contract
Excessive working time	The individual works more than 48 hours per week (i.e. maximum allowed by the legislation)
Severance payments	The employment contract entitles the individual to severance payments
Paid holidays	The employment contract entitles the individuals to paid holidays
Parental leave	The employment contract entitles the individual to maternity/paternity leave
Health social security	The individual has health social security coverage, with the employer contributing partially or fully to it
Pension social security	The individual has pension social security coverage, with the employer contributing partially or fully to it
Status in the labour market, hours worked and wages	
Variable	Definition
Employed	The individual works at least one hour in the reference week
Unemployed	The individual does not work in the reference week, is looking for work and available to start working
Inactive	The individual is neither employed nor unemployed, but in the working age
Formal economy	Public sector employees, self-employed with registered activity, employees with social security contributions at least partially paid by the employer or individuals with no social security contributions paid by the employer that however work in a fixed place that employs at least 5 people. All individuals working in Personal and Household Services are considered in the informal economy, independently from their employment status
Formal employment	Employee with social security contributions at least partially paid by the employer, self-employed working in the formal sector and employers working in the formal sector
Number of hours worked in main job	Number of hours worked during the last week in the main job
Number of hours worked overall	Number of hours worked during the last week in all the jobs the individual has
Income from main job	Monthly labour income from the main job, 2015 real values
Overall labour income	Monthly labour income from all the jobs the individual has, 2015 real values

Table F2: Baseline results: hours worked and wages, by formal sector of the economy

	Formal economy				Informal economy			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Number of hours worked in main job	-0.121 (0.137)	-0.123 (0.134)	-0.140 (0.139)	-0.138 (0.138)	-0.535** (0.229)	-0.528** (0.228)	-0.480** (0.224)	-0.446** (0.224)
Number of hours worked overall	-0.125 (0.149)	-0.128 (0.146)	-0.145 (0.152)	-0.143 (0.151)	-0.554** (0.242)	-0.547** (0.242)	-0.498** (0.238)	-0.465* (0.237)
Income from main job	-25,880** (10,859)	-21,979** (9,063)	-19,476** (7,706)	-19,476** (7,706)	7,485*** (2,833)	7,073** (2,792)	6,804** (2,734)	6,804** (2,734)
Overall labour income	-34,035** (15,776)	-31,715** (14,381)	-31,151** (13,442)	-25,540** (12,177)	9,154** (4,096)	9,187** (4,049)	9,229** (3,896)	10,012** (4,005)
N	554,096	554,096	554,096	554,096	862,167	862,167	862,167	862,167
F test	12.4843	12.4847	12.4865	12.4859	13.4028	13.4027	13.4046	13.4046

Main controls

Integer of the final score FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial of order 3 for decimal of the final score*Above	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Baseline controls

Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects*Time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual level characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Additional controls

Occupation dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Sector dummies	No	No	Yes	Yes	No	No	Yes	Yes
Status dummies	No	No	No	Yes	No	No	No	Yes

Note: The table reports coefficient estimates and standard errors for the effect of an additional inspector on hours worked and wages in the formal and informal sectors. Reported coefficients correspond to the parameter β_1 in equation 5 in the main text. The columns differ for whether additional controls for labour market characteristics are included. In particular, column 1 corresponds to the baseline specification used in the rest of the analysis (i.e. controlling for the decimal part of the final score, allowed to vary on the two sides of the cutoff) while the other columns consecutively add a different set of labour market dummies. 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 Table F1 in Appendix F. * <10%, ** <5%, *** <1%.