

Compliance with Labour Legislation: Evidence from a Natural Experiment

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Abstract: As part of the negotiations for the “US-Colombia Trade Promotion Agreement”, the US Administration required to more than doubling the number of labour inspectors operating in Colombia within four years. This generated exogenous variations in enforcement levels over space and time that I exploit to estimate the effectiveness of inspection in promoting formal employment. Due to delays in the implementation of the policy, I instrument the actual policy change with the theoretical policy target. I find that labour inspection increases formal employment without generating general equilibrium changes in employment levels. However, the positive effect is limited to urban areas and large establishments.

JEL: H10, J38, J58, K31

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I. Introduction

Developing economies are often characterised by restrictive systems of labour legislation, which formally guarantee standards of protection comparable to those granted to workers in developed economies (ILO, 2015). However, the level of protection actually enjoyed by workers depends on how effectively legislation is implemented. In this respect, non-compliance with labour law is often pervasive in developing economies. This can be ascribed to a variety of reasons; ranging from labour market characteristics (e.g. prevalence of small businesses), workers' bargaining power (e.g. limited presence of trade unions) and broader institutional factors (e.g. rule of law). In this context, labour inspection represents the most immediate policy tool used by governments to promote compliance. However, very little is known with respect to the effectiveness of this intervention. The absence of research is mostly connected with the lack of adequate data (on both the treatment indicator and the outcome of interest) as well as problems of econometric identification. On this latter aspect, enforcement and compliance can be stronger in areas generally characterised by more stable political and economic institutions. This is likely to generate problems of omitted variable bias, which will most likely result into an overestimation of the causal impact of inspection on compliance. At the same time, governments may want to reinforce labour inspection in areas characterised by a higher risk of non-compliance. In this case, the presence of a simultaneous relationship between the outcome of interest and the treatment indicator would result into an underestimation of the policy effect.

An exogenous change in treatment intensity (i.e. in the intensity or scale of labour inspection) can therefore be useful to solve the identification problem and several options have been proposed in the literature. Almeida and Carneiro (2009, 2012) use distance to the labour office as an instrument for enforcement in Brazil, under the assumption that this exogenously reduces the probability that an employer is visited by labour inspectors. They find that stricter enforcement makes formal jobs more attractive at the expense of firms' size. Bhorat et al. (2012) use the number of officials working in South African regional labour centres in units different from the labour inspection as an instrument for the number of inspectors working in the same labour centre, under the assumption that non-inspectors will not affect labour law compliance. The results do not show any impact of enforcement on compliance with minimum wage. Ronconi (2010) uses electoral years as instruments for the number of inspectors in Argentina, under the assumption that in the proximity of an election the government is more willing to protect labour rights. The results show how enforcement increases compliance with

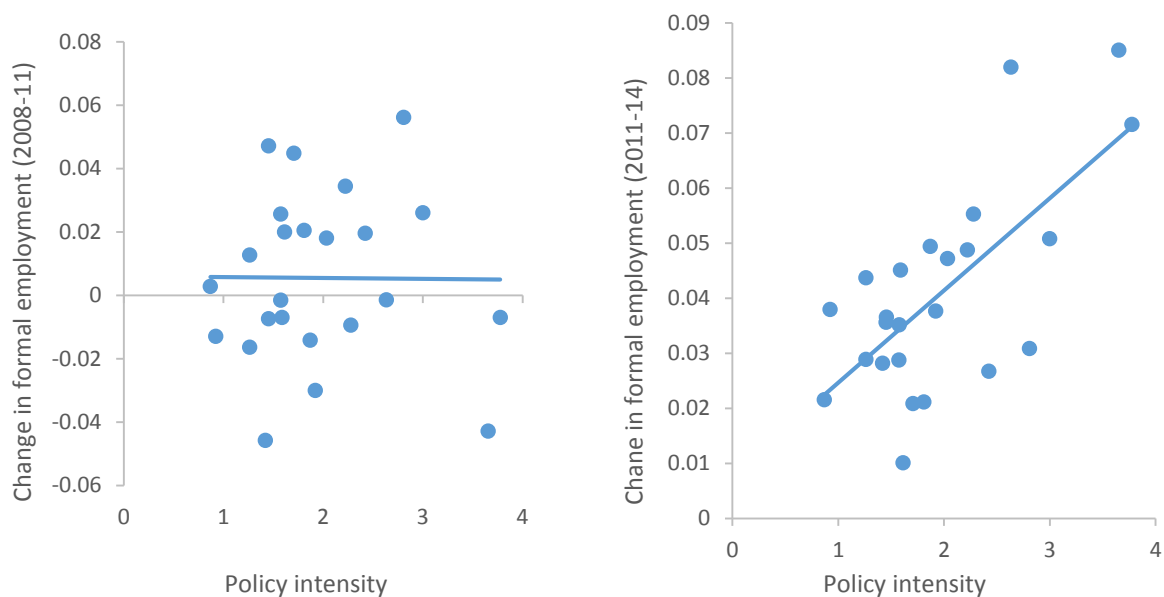
mandated benefits such as minimum wage, maximum hours and health insurance. In the absence of major policy changes in labour inspection, all these studies exploit external shocks that generate presumably exogenous variations in enforcement levels. Of course, in an instrumental variable framework this requires assuming that these external shocks affect the outcomes of interest only through the hypothesised mechanism.

In this paper, I am able to partially relax this assumption by exploiting a drastic and exogenous reform of the system of labour inspection in Colombia. In 2006, Colombia and the US signed a trade agreement (the “US-Colombia Trade Promotion Agreement”, CTPA) aimed at reducing trade tariffs between the two countries. The agreement was immediately ratified by the Colombian Parliament in 2007, while the US Congress did not succeed in ratifying the CTPA by the end of the Congressional session in December 2008 amid concerns over violations of labour rights in Colombia (Stenzet, 2008). A new bargaining round started in 2009, during which the newly elected US Government requested to include in the agreement measures to reinforce compliance with labour legislation in Colombia. This request can be connected to the increasing policy trend of including labour provisions in trade agreements (ILO, 2016) and was specifically motivated by reports of systematic violations of labour rights in Colombia and the low coverage of labour inspection from an international perspective (OECD, 2016). The negotiations led to the ratification in 2011 of the “Colombian Action Plan Related to Labour Rights” (in short, the Action Plan) which became an integral part of the CTPA. One of the main interventions included in the Action Plan was the commitment by the Colombian Government to more than doubling the number of labour inspectors within four years – from 353 inspectors in 2010 to 904 in 2014. The reform needed to be swiftly implemented (100 new inspectors needed to be hired already in 2011) and it contrasted with the lack of investments in labour inspection during previous years (OECD 2016).

Despite the drastic and exogenous nature of the reform, this variation alone would not be sufficient for identification purposes. Indeed, there might be multiple reasons why compliance with labour legislation has varied in Colombia in the period under consideration (i.e. independently from the role played by labour inspection). Instead, central to the identification strategy of this paper is the fact that the rise in the number of inspectors was not uniform across departments. Rather, the central Government set separate targets for the different departments according to policy priorities as set by the Action Plan and ministerial organizational needs (see below for details). This generates differences in programme intensity that can be exploited to estimate causal effects under the (weaker) assumption that trends in the outcomes of interest

would have not systematically differed between departments in the absence of the programme. While the validity of this assumption will be discussed in details in the paper, Figure 1 already reveals the presence of a positive relationship between the treatment indicator and changes in the outcome of interest (formal employment) after (but not before) the implementation of the policy. The presence of a policy change substantially facilitates the identification of a causal effect compared to previous contributions, since working in differences (rather than levels) eliminates the need to control for a series of factors jointly affecting enforcement and compliance (e.g. rule of law, level of corruption) that are generally regarded as unobservable but time-invariant (i.e. can be accounted for by department fixed effects). Similar identification approaches have been used to estimate the impact of schooling on labour market outcomes (Duflo 2001) and the effect of female labour supply on wages (Acemoglu et al. 2004).

Figure 1 – Relationship between policy intensity and formal employment before and after the policy change



Note: The figure present the relationship between the policy intensity in a given department (total theoretical change in the number of inspectors per 100,000 employed individuals between 2010 and 2014) and the percentage point change in the rate of formal employment before (left panel) and after (right panel) the implementation of the Action Plan. The total theoretical change refers to the policy target set by the Action Plan, see below for details.

Compared to these studies, I am able to complement the difference-in-difference framework with an instrumental variable approach that takes into account the possible endogeneity in the implementation of the policy. Indeed, the drastic nature of the reform required the different departments to rapidly scale up their enforcement systems and hire a number of new inspectors following a centrally established timeline. This contrasted with the general absence of trained candidates for the positions and the limited capacities of the public administration (especially

at the local level) to launch and supervise the recruiting process (OECD 2016). As a result, the yearly recruiting targets were systematically missed and the final target of 904 inspectors (originally set for 2014) was still to be met at the end of 2016. This could generate concerns over the exogeneity of the treatment indicator (i.e. total change in the number of inspectors in a department normalised by employment levels) in case that differences across departments in implementing the policy reflect broader (unobservable and time varying) differences that can affect at the same time the outcome of interest (e.g. commitment of the labour office). For this reason, I instrument the total actual change in the number of inspectors that took place in a department as a result of the Action Plan with the total theoretical change that should have formally taken place in that department according to the policy.¹ In this respect, the study follows an instrumental variable approach as implemented by De Giorgi et al. (2015) to study the elasticity of substitution between male and female workers in Italy.

The results indicate that an additional inspector per 100,000 employed individuals increases the probability of being in a formal job by around 2 percentage points. The result is economically significant and in line with previous estimates (Ronconi 2010). Additionally, the positive effect on labour law compliance does not generate any change in (un-)employment levels. This rules out the possible presence of negative general equilibrium effects that have been found in some of the previous studies (Almeida and Carneiro 2009). However, the positive effect on formal employment materialises mostly in urban areas and large establishments. This is consistent with previous findings that have shown how inspectors tend to target registered firms that are easier to find and reach (Almeida and Poole 2017). Overall, the picture that emerges is that strengthening enforcement can increase compliance with the labour law in developing economies – critically contributing to increasing the coverage of institutions. However, simple enforcement strategies risk being ineffective beyond the quasi-formal sector of the economy – where these interventions would be most needed. The remainder of the paper is organised as follows. Section II reviews the literature; section III presents the policy change and the system of labour inspection in Colombia; section IV introduces the datasets used and the descriptive statistics; section V discusses the validity of the estimation strategy; section VI describes the main empirical results and robustness tests; section VII explores how the effect on formal employment materialises; section VIII concludes.

¹ Here and in the rest of the paper, I refer to the actual policy change as the one that was effectively implemented and the theoretical policy change as the one formally prescribed by the Action Plan.

II. Literature review

The empirical economic literature on labour law (and the policy debate) has been traditionally focused on the effects of the strictness of the formal level of employment legislation on labour market outcomes (Botero et al. 2004); while very few studies have examined the role played by the level of legislation effectively enforced. This research gap is particularly significant given the high levels of non-compliance with the legislation (especially in developing economies), which make the formal level of legislation only an imperfect proxy of the degree of regulation faced by workers and enterprises. As stated in Ronconi (2010), there are two key challenges in assessing empirically the effectiveness of labour inspection. First, there is a problem of endogeneity due to the possible presence of omitted variables as well as the potentially simultaneous relationship between enforcement and compliance. Secondly, it is problematic to adequately measure both the treatment indicator (strength of labour law enforcement) and the outcome of interest (compliance with labour legislation). This section will briefly review how previous contributions have dealt with these different estimation problems. All the papers reviewed in this section have some commonalities, such as the focus on a single country and the use of an instrumental variable approach. However, they differ (substantially) on the choice of the instrument and (to a lesser extent) on the measures of enforcement and compliance used.

A. Econometric identification

Two main sources of endogeneity could bias simple estimations of the relationship between enforcement and compliance – assuming the absence of measurement errors (Ronconi 2010). First, omitted variables could bias the results if the relationship between enforcement and compliance is affected by an unobservable characteristics that influence at the same time the treatment indicator and the outcome of interest. The direction of this source of bias is unclear, even though it is more common to imagine a positive relationship between the omitted variable (e.g. general rule of law) and the strength of enforcement – thus leading to an upward bias of simple impact estimates. Secondly (and more importantly), there could be a simultaneous relationship between the treatment indicator and the outcome of interest. This source of bias generates from the fact that enforcement efforts are likely to be directed towards areas with a higher perceived risk of non-compliance – thus leading to an underestimation of the causal effect in a simple OLS framework. The literature has pretty consistently adopted an instrumental variable approach in order to solve these identification problems. For instance,

Almeida and Carneiro (2012) exploit the fact that inspectors in Brazil are assigned to labour centres and then need to drive by cars to visit firms. As a result, distance to the labour centre will decrease the probability of a firm being inspected. At the same time, the role of distance in determining the probability of being inspected will be greater the fewer inspectors are based in a labour centre. Using this combined indicator of enforcement, the authors find that labour inspection increases formal employment and reduces wage dispersion in the formal sector. However, stronger enforcement increases wages in the informal sector as a result of lower mandated benefits. 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. For this reason, they use the number of non-inspectors in a labour centre as an instrument for the number of inspectors in that same centre – which constitute their proxy for enforcement. 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. Under the assumption that election years are not otherwise correlated with compliance with the legislation, the results show that an additional inspector per 100,000 people increases compliance with mandated benefits in Argentina by 1.4 percentage points. Viollaz (2016a, 2016b) uses the arrival cost of labour inspectors (measured as the logarithm of per capita crossing vehicles per kilometre) as an instrument for enforcement in Peru – under the assumption that this will exogenously decrease the probability that a firm is inspected. The results reveal limited effects on compliance.²

B. Measurement

Challenges related to measurement errors can concern both the treatment indicator (i.e. how to measure enforcement) as well as the outcome of interest (i.e. compliance with the labour legislation). In theory, firms respond to both 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 in

² Other studies use similar methodologies to assess the impact of inspection on other outcomes of interest (i.e. different from law compliance). For instance, Almeida and Carneiro (2009) analyse the impact of enforcement on firms' size; while Almeida and Poole (2017) look at the effect on hiring decisions.

practice – generally covering the threat of being caught.³ In particular, most papers use the number of inspectors (Bhorat et al. 2012, Ronconi 2010) or the number of inspections (Almeida and Carneiro 2012; Almeida and Poole 2017) as a proxy for enforcement. This is normalised by the employment 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; while Almeida and Carneiro (2009) use as treatment indicator an interaction between distance to the labour office and the number of inspectors.⁴

Turning to the measurement of the outcomes of interest, it is generally agreed to use information on compliance reported by the workers – generally as collected in labour force surveys or censuses (Almeida and Carneiro 2009, 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). A second problem concerns the choice of the indicator of labour law compliance. Indeed, labour legislation is generally very complex and several rules regulate working conditions for different groups of workers. Additionally, labour inspectors can be assigned priority areas in certain specific topics. Accordingly, most papers focus on few measures of labour law compliance based on the content of the labour code in the country and the institutional characteristics 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 2016), the right to paid holidays and compliance with the legislation over working hours (Ronconi 2010).

III. Programme

A. Action Plan

The present analysis exploits a drastic and externally induced reform of the system of labour inspection in Colombia, which represents an ideal natural experiment to identify the effect of enforcement on compliance. In particular, the United States and the Colombian governments signed in November 2006 a comprehensive trade agreement aimed at reducing administrative

³ This might generate a downward bias in the estimate of the effectiveness of inspection, as only some components of the policy are taken into consideration.

⁴ Other measures of enforcement used in the literature include the inverse of seignorage (Ihrig and Moe 2004) and the workforce average education (Botero et al. 2004).

and fiscal barriers between the two countries (the “United States-Colombian Trade Promotion Agreement”, CTPA) The strategic importance of this agreement for Colombia can hardly be overstated: the US is Colombian main trade partners, accounting for 39 per cent of the country’s exports and 29 per cent of its imports. On the other hand, Colombia accounts for only around one per cent of US imports and exports. This generated an asymmetry in the importance of the agreement for the two countries, which – combined with existing geopolitical considerations – resulted in a substantial imbalance in bargaining power during the negotiations. The ratification process required the approval of the trade agreement by both countries. Given the strategic importance of the agreement for the Colombian economy, the Colombian Congress approved the bill already in June 2007 with a large majority (84 yes and 3 no in the House Floor). In April 2007, the agreement was also sent for approval to the US Congress (Stenzet, 2008). However and despite the US Administration support for the CTPA, the debate never led to a vote amid concerns raised by the US Congress over violations of labour rights in Colombia.⁵ A legislative rule change to indefinitely delay action on the CTPA was instead approved by the US Congress, after which the Administration committed to include “worker protections in several pending trade accords”.⁶ As a result, the CTPA was not approved by the end of the Congressional session in December 2008.

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 compliance with labour rights in Colombia.⁷ This led to the definition in April 2011 of a set of measures aimed at reinforcing compliance with labour law in the country (the “Colombian Action Plan Related to Labour Rights”, or Action Plan) to be implemented in collaboration with the ILO. In particular, the Action Plan established the creation of the Ministry of Labour (that resulted from the separation from the Ministry of Social Protection) as the appropriate institutional vehicle to promote labour rights and employment policies. Additionally, reforms were agreed to strengthen collective bargaining, prohibit the misuse of cooperatives and prevent the misuse of temporary agency workers. In particular, the criminal code was reformed in 2011 to increase the sanctions in case of anti-union behaviour (e.g. preventing collective bargaining, offering more favourable employment conditions to non-unionised workers) and the labour code was

⁵ It is important to note that since January 2007 the US had a divided Government, with a Republican Administration (in favour of the agreement) and a Democratic Congress (opposing it).

⁶ This was meant to apply to pending trade agreements with Colombia, Panama, Peru and South Korea.

⁷ In 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 (e.g. the major US trade union federation was also opposed to the CTPA). This led to delays in the beginning of the new bargaining round and the definition of the Action Plan.

reformed in 2011 to prevent cooperatives to conduct labour market intermediation services (e.g. recruit temporary agency workers). For the purpose of this analysis, the Action Plan also established that the Ministry of Labour would have hired 480 new inspectors within four years (i.e. by the end of 2014, 100 new inspectors to be hired already in 2011) in a move that would have more than doubled the total number of inspectors in the country. Additionally, the Action Plan included measures aimed at reinforcing citizens' ability to file complaints to labour inspectors (either anonymously or otherwise). This included the institution of a free telephone line as well as the possibility to file complaints online from the website of the Ministry. Awareness campaigns were also launched in order to increase citizens' use of these tools.⁸ 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.

As a result of the Action Plan, the number of labour inspectors in Colombia increased from 353 in 2010 to 726 in 2014.⁹ This represents a substantial improvement in international terms, as the number of inspectors per 100,000 employed individuals has evolved from around 2 to 4 between 2011 and 2015 (ILO Statistics). This value is comparable to the one observed in other countries in the region (e.g. 4 inspectors per 100,000 employed persons in Argentina, 3 in Brazil, Ecuador and Peru, 1 in Paraguay – although Chile and Uruguay have a much higher level), but is still considerably below the value registered in most developed economies – despite the lower perceived risk of non-compliance with labour law in these economies (see Figure 1 in Appendix A).¹⁰ The drastic and exogenous increase in labour inspection is the key starting point for identifying the causal effect of enforcement on compliance in this paper, as it generates strong variations in treatment intensity over (a relatively short period of) time that allow to work in differences (rather than levels). Additionally, two other policy features are worth mentioning. First, the increase in the number of inspectors was not homogeneous across departments. Rather, the central government set different targets at the department level according to Action Plan policy priorities and ministerial organizational needs (see below) and provided the necessary financial resources for the hiring process.¹¹ This generated strong

⁸ However, all other policy changes contained in the Action Plan (i.e. apart from the hiring of new inspectors) were ruled out nationally and they did not set targets that differed across departments.

⁹ In particular, the number of inspectors increased from 353 in 2010 to 412 in 2011, 467 in 2012, 575 in 2013 and 726 in 2014.

¹⁰ For instance, the number of inspectors per 100,000 employed persons is equal to 15 in Germany, 8 in France and 6 in Japan. However, Anglo-Saxon countries all have a lower number of labour inspectors.

¹¹ The hiring process, requirements and conditions of employment were instead common across the country. In particular, the administration had the possibility of first offering the positions to internal qualified candidates. Alternatively (or for the remaining positions), the vacancy was posted through the public employment services and the administration proceeded with the selection process (e.g. CV screening, interviews).

variations in treatment intensity over space (see section V for details) that can be exploited for identification under the assumption that trends in the outcomes of interest would have not systematically differed across departments characterised by different intensity of the policy. Finally and despite the commitment of the Government to rapidly raise the number of inspectors, the hiring process took longer than expected. This was mostly due to organizational delays as well as the lack of enough qualified candidates for the new positions. As a result, the planned target of 904 inspectors (originally set for 2014) was still to be met in 2016.¹² This might generate concerns over the use of the change in the number of inspectors as the main treatment indicator, if delays in the implementation of the policy were not random across departments – but rather reflected observable or unobservable characteristics at the department level (e.g. corruption, motivation). For this reason, I use the target total change in the number of inspectors as an instrument for the actual total change taking place in each specific department.

B. Labour inspection

Labour inspectors in Colombia are in charge of securing law enforcement and guaranteeing the respect of collective bargaining rights. Candidates need to hold a graduate degree in either law, public administration or medicine and have at least seven months of relevant work experience. In their operations, inspectors are asked to follow ILO guidelines as summarised in a manual prepared by the Colombian Ministry of Labour. This contains the professional and personal requirements to be met by labour inspectors and details the administrative procedures and the code of conduct that inspectors should follow. Labour inspectors in Colombia 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, inspectors are responsible for all economic sectors. However, the Action Plan identified specific industries to be prioritised.¹³ To prevent corruption and guarantee employment stability, inspectors can be dismissed only under exceptional circumstances (e.g. disciplinary reasons).

The Colombian system of labour inspection has been recently reformed in collaboration with the ILO – with the aim of adopting a new benchmark of labour inspection that has recently

¹² Apart from hiring new inspectors, the Government also committed to fill existing vacancies. This explains why the final target (904 inspectors) was higher than the existing level in 2010 (353) plus the new hiring (480).

¹³ These sectors corresponded to palm oil, sugar, mines, ports, and flowers.

emerged in the international policy debate. In particular, the new model aims to move away from the traditional labour inspection system centred on sanctioning strategies (i.e. maximise the number of inspections or the amount of fines) towards a strategic compliance model. This new model starts from the recognition that not all employers are motivated to comply with the legislation exclusively on the grounds of a cost-benefit analysis (i.e. comparing the economic benefits of non-compliance with the possible costs of being caught); but they rather respond to a variety of factors such as habits, peer pressure and civic motivations. Under this assumption, sanctioning tools represent only one of the options to be used by inspectors to promote compliance. Rather, inspectors (i) understand the causes of non-compliance, (ii) engage with relevant stakeholders, and (iii) design tailored interventions with multiple arms (ILO, 2017).¹⁴

In practice, in their operations labour inspectors in Colombia can start an enquiry either independently (e.g. decide autonomously the firms to investigate) or respond to complaints raised by workers.¹⁵ Workers can file complaints (anonymously or otherwise) online, by telephone or directly at the labour office.¹⁶ Once inspectors decide to start an enquiry, they open a preliminary investigation with the aim of better understanding the case and verifying the presence of valid grounds for proceeding further. Based on this preliminary assessment, the case can either be archived or alternatively transformed into a formal investigation. The formal investigation generally terminates with an intervention by labour inspectors, which can take different forms. In particular and based on the situation with which they are confronted, 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 workers (e.g. help defining an agreement in case of collective dismissals); (iii) implement initiatives to remove obstacles to the implementation of the labour code (e.g. favour the development of a trade union); or (iii) impose fines and sanctions. Sanctions can be of financial nature (going from 1 to 50,000 times the minimum wage) and/or involve the temporary closure of the enterprise (from 3 to 10 days, up to 30 in case of recidivism) or its immediate shut-down in case of health or security risks.

¹⁴ In particular, interventions undertaken by labour inspectors to promote compliance could include (i) enforcement activities (e.g. inspections and sanctions); (ii) education activities (e.g. training and recommendations); (iii) communication strategies (e.g. name and shame campaigns); (iv) political actions (e.g. proposing a parliamentary commission); and (v) systemic interventions (e.g. certifications, grants).

¹⁵ No information is available on the number enquires started autonomously by inspectors or as a result of a complaint.

¹⁶ In particular, as part of the Action Plan the Government launched the *COLabora* programme to facilitate registering workers' complaints.

This organization of the system of labour inspection implies that there are different tools and strategies available to inspectors. For instance, in 2013 labour inspectors initiated 15,588 preliminary investigations. Of these preliminary investigations, only 5,988 were transformed into formal investigations leading to a final intervention (i.e. the rest of the cases were archived). For these formal investigations, in only 1,494 sanctions and fines were imposed (i.e. in the rest of the cases, other interventions were adopted). As a result, sanctions were imposed in less than ten per cent of the cases initially investigated. During the same year, inspectors conducted a total of 10,438 visits to establishments. Only 1,489 of these visits were conducted as part of a formal investigation – while the remaining visits were done outside of an investigation (Ministry of Labour, 2013). The conciliatory function seems instead much more prominent in the system of labour inspection in Colombia, as in 2013 around 86,000 conciliation hearings were conducted by labour inspectors in the country (OECD 2016).

IV. Data

A. Database

The present analysis draws from three different data sources. First, 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 current version of the GEIH is operational since 2008; when (i) the sample size and coverage has been increased, (ii) electronic devices have been introduced for data collection, and (iii) the scope of the analysis has been extended. The GEIH has a two-stage stratified sample and interviews every year around 250,000 households. The sample that will be used in this study corresponds to the entire working age population between 2008 and 2014.¹⁷

Using information on the department of residence and the year of the interview, I match the survey data with department-level administrative information on the number of inspectors provided by the Colombian Ministry of Labour.¹⁸ Some remarks concern the matching process between the administrative and survey data. In particular, the Ministry of Labour follows the traditional division of the country into 32 departments to organize the system of labour

¹⁷ The analysis starts in 2008 when the new version of the GEIH was made available and ends in 2014 when the targets set by the Action Plan should have been met. In this way, the analysis includes three years before and after the implementation of the policy in 2011.

¹⁸ Ideally, I would need information on the department where the individual works – rather than the department of residence. Unfortunately, this information is reported in the GEIH only from 2012 onwards. However and given the large size of the departments, this is unlikely to create major issues. In particular, in 2014 only 0.8 per cent of the sample reported living and working in two different departments.

inspection – while the GEIH covers only the 24 main departments in Colombia. Since it is impossible to connect observations in the GEIH to the 32 departments, I conduct the analysis for the 24 departments for which I have both survey and administrative data.¹⁹ In practice, the problem is of limited empirical importance given the small size of the population living in the excluded departments (representing less than 5 per cent of the Colombian population).²⁰

Finally, I obtained information on the quality of the public administration from the Departmental Institutional Environment and Performance Survey (EDID). This is a survey conducted by DANE that interviews randomly selected public officials working in the central administration in the different departments, with the aim of obtaining information on their perception of the institutional environment. In particular, the survey asks detailed information on the official's perception of the management, efficiency and transparency of the public administration – including how tasks and responsibilities are assigned; according to which principle officials are recruited; whether the institution has the necessary resources. Answers to these questions are grouped to compute intermediary indicators (credibility in the rules, credibility in the policies, adequacy of resources, result-based management, accountability, labour welfare, prevention of irregular practices and citizen participation) and then overall indicators (institutional environment and institutional performance). This information is available at the department level from 2009.²¹

B. Outcome of interest and treatment indicators

The main outcome of interest in the analysis corresponds to formal employment as reported by employed persons in the GEIH. The definition of formal employment follows ILO guidelines and the variable has been coded by the ILO Department of Statistics. In particular, employees are defined as formal workers if their employer contributes (either partially or entirely) to their

¹⁹ Starting from 2014, the GEIH covers five additional departments (Arauca, Casanare, Putumayo, San Andrés and Vichada). However, the department variable still reports only 24 values.

²⁰ Another codification problem concerns the departments of Cundinamarca and Bogota. In particular, the system of labour inspection within the Ministry of Labour initially considered the two departments as part of the same administrative unit (named Cundinamarca), so that data on labour inspectors is reported jointly until 2012 and then separately only from 2013 onwards. In order to have the entire series (2008-2014) for both departments, I compute the ratio of inspectors working in Cundinamarca (Bogota) using the two separate series from 2013 to 2015. This ratio is relatively constant for the three years with available information (from 77 to 81 per cent of the total number of inspectors for the region works in Bogota) and its average is used to obtain separate series for those years when only aggregate information is available (2008-2012). A robustness test will be conducted in the empirical analysis to check the validity of the results to the possible incorrect measurement of inspection levels in these departments.

²¹ Unfortunately, it is not possible to trace to which entity of the public administration in a given department the respondents belong to (e.g. Ministry of Labour, anti-corruption authority). This information is recorded, but not made available by DANE due to confidentiality reasons.

pension scheme. Employers and own-account workers are instead included in formal employment if they are registered at the relevant public authority.²² Finally, contributing family workers are always presumed in informal employment. The decision to focus on formal employment is motivated by the explicit policy priority of inspectors in terms of labour formalization (see section III above) as well as the overarching nature of this concept as an indicator of employment quality independently from the status in employment. In particular, the alternative strategy of selecting different indicators of labour law compliance (e.g. minimum wages, maternity leave, paid holidays) would have limited the analysis to specific sub-groups of the working population that are not necessarily representative of the overall labour market (around 43.5 per cent of the employed population was estimated being contributing family workers in Colombia in 2017). Additionally, this would have increased the risk of not taking into account general equilibrium effects (e.g. increase in mandated benefits for employees paralleled by a decrease in the share of employees in the labour force).²³

Turning to the treatment indicator, as mentioned above an ideal measure of enforcement would cover both the threat of being caught (as proxied for instance by the number of inspectors or inspections) as well as the size of the sanction (as proxied by the amount of the sanctions) (Ronconi 2010). In practice, data on labour inspection is generally very scant and previous studies included information only on the threat of being sanctioned (i.e. number of inspectors or number of inspections). I follow these contributions and use as main treatment indicator the change in the number of inspectors operating in each department between 2010 and 2014.²⁴ This is normalised by the number of employed individuals in a given department in order to take into account size effects (Bhorat et al. 2012, Ronconi 2010). The use of the number of labour inspectors (rather than the number of inspections or the amount of the fines) as the treatment indicator is motivated – first – by the fact that this was the policy measure used to establish a (national and regional) target as part of the Action Plan. As such, this is the only parameter whose change over time can be taken as exogenous (i.e. as triggered by the Action Plan) and can be benchmarked against a target (for the instrumental variable analysis, see

²² The official ILO definition would include within formal employment also employers and own account workers that – despite not being registered with the relevant authority – keep track of accounting activities. However, this question is asked in the GEIH only from 2009 onwards, hence the other definition is preferred. In any case, the difference in terms of formal employment is minimal.

²³ The analysis of wage aspects is anyway limited by the absence of adequate information in the GEIH on the number of weeks and hours worked within the month.

²⁴ I do not include in the analysis all labour inspectors operating in Colombia – but only those assigned to a specific department and that can thus be matched to individuals in the GEIH. This leaves out inspectors working in the newly created directorial offices as well as those assigned to special offices in charge of a specific town (i.e. overall corresponding to around 10 per cent of inspectors).

below). Additionally, the choice of the number of inspectors (rather than the number of inspections or the amount of the fines) seems particularly appropriate given the model of labour inspection currently in place in Colombia – as described in Section III. Indeed, labour inspectors operate a number of functions (e.g. conciliation, provision of information, mediation) with a variety of instruments (e.g. peer pressure, visits, investigation) which are no longer limited to the traditional sanctioning mechanisms (i.e. fines and sanctions).²⁵ Given the challenge of measuring the variety of these (material and immaterial, official and unofficial) instruments, the choice of focusing on labour inspectors seems reasonable – as any instrument adopted in the area of labour inspection will be adopted by labour inspectors.

C. Descriptive statistics

For ease of exposition, I divided the 24 departments with available information into quartiles according to the theoretical intensity of the policy in that given department as a result of the Action Plan (i.e. low, medium-low, medium-high and high).²⁶ Descriptive statistics for these groups at baseline are presented in Table 1. An analysis of the data reveals the absence of notable differences in individual characteristics (e.g. average age, share of men and average years of education) across departments characterised by different levels of policy intensity (Panel A). Some differences emerge with respect to the share of individuals in employment (higher in medium-high policy intensity departments), the share of individuals outside the labour market (lower in medium-high policy intensity departments) and the share of employed individuals in a formal job (higher in medium-low policy intensity departments) (Panel B). At the same time, the quality of the public sector (as measured by the EDID) does not systematically differ across different groups of departments (Panel C). Finally, the last panel shows how the theoretical intensity of the policy was substantially different across departments (the theoretical change in the number of inspectors per 100,000 employed ranges from 1.2 to 3.05) and it resulted in sharp differences in the actual change in the strength of enforcement (the actual change in the number of inspectors per 100,000 employed ranges from 0.86 to 2.07). These differences partially match differences in the number of inspectors in 2010 (i.e. before

²⁵ Accordingly and while the number of inspectors substantially increased as a result of the Action Plan, the number of visits remained fairly stable (from 10,253 in 2011 to 10,458 in 2015). As a result, the number of visits per inspector decreased from an average of 25 per year in 2011 to 12.5 per year in 2015 (ILO Statistics). Similarly, the number of fines imposed was roughly constant during the period under consideration – going from 1,749 in 2010 to 1,694 in 2013 (OECD 2016).

²⁶ This is done according to the total theoretical change in the number of inspectors (per 100,000 employed individuals) according to the Action Plan between 2010 and 2014 (first variable in panel D of Table 1). Here and in the continuation of the analysis, the baseline number of labour inspectors refers to the 2010 level as the new hiring process started already in 2011.

the implementation of the policy), suggesting that areas with an initially higher number of inspectors received a proportional increase as part of the Action Plan. Finally, the implementation gap (i.e. difference between target and actual number of inspectors in 2014) is higher in high policy intensity departments – presumably because the higher target created additional challenges in meeting the requirement within the established timeline (Panel D).

Table 1: Descriptive statistics at baseline (2010)

	Low	Medium-low	Medium-high	High
Panel A: Personal characteristics				
Age	39.08 (0.64)	39.47 (0.80)	38.97 (1.48)	37.96 (1.02)
Male	0.49 (0.01)	0.49 (0.01)	0.49 (0.01)	0.49 (0.01)
Years of education	7.62 (0.82)	8.16 (1.09)	7.59 (0.56)	7.16 (0.56)
Panel B: Labour market				
Employed	0.61 (0.05)	0.58 (0.06)	0.63 (0.02)	0.58 (0.03)
Unemployed	0.06 (0.02)	0.07 (0.02)	0.08 (0.03)	0.06 (0.02)
Inactive	0.33 (0.06)	0.35 (0.06)	0.29 (0.02)	0.36 (0.04)
Formal employment	0.24 (0.11)	0.34 (0.13)	0.24 (0.06)	0.22 (0.08)
Rate of employment in priority sector	0.02 (0.02)	0.01 (0.01)	0.01 (0.01)	0.03 (0.06)
Panel C: Quality of public sector (from 1 to 5)				
Institutional environment	3.63 (0.22)	3.64 (0.11)	3.66 (0.13)	3.59 (0.30)
Institutional performance	3.58 (0.19)	3.65 (0.10)	3.67 (0.15)	3.57 (0.24)
Table 9: Policy intensity (per 100,000 employed)				
Total theoretical change in inspectors (2010-14)	1.20 (0.25)	1.58 (0.08)	2.02 (0.19)	3.05 (0.55)
Total actual change in inspectors (2010-14)	0.99 (0.31)	0.86 (0.39)	1.57 (0.55)	2.07 (0.40)
Gap with respect to target (in 2014)	0.21 (0.26)	0.72 (0.39)	0.45 (0.53)	0.98 (0.63)
Inspectors in 2010	1.98 (0.66)	1.94 (0.88)	2.14 (0.55)	2.17 (0.70)

Note: Departments are divided into quartiles (low, medium-low, medium-high and high) according to the intensity of the policy as captured by the total theoretical change in the number of labour inspectors (per 100,000 employed) between 2010 and 2014.

V. Identification strategy

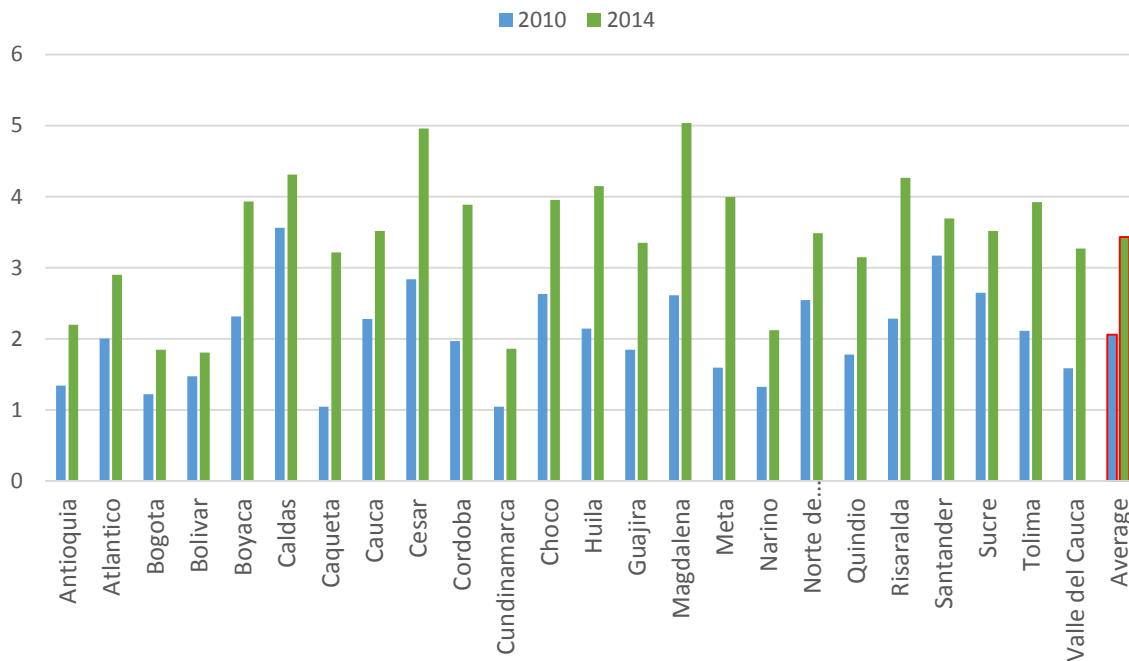
A major challenge in the identification of a causal effect of enforcement on compliance with labour legislation is that the level of enforcement is generally set at the national level and/or it rarely varies over time (Almeida and Carneiro 2009). As a result, it might be difficult to disentangle whether the (positive or negative) relationship between enforcement and

compliance arises from a causal effect or is rather the result of some spurious correlation at the level of the unit of interest (e.g. the municipality). In particular, two main reasons of concern relate to the possible presence of omitted variable bias and the possible simultaneous relationship between enforcement and compliance (Section II).

The identification strategy of this paper exploits the drastic and externally induced increase in the number of inspectors following the implementation of the Colombian Action Plan. However, this variation alone would not be sufficient for identification purposes. Indeed, there might be multiple reasons why compliance with labour legislation has varied in Colombia in the period under consideration (i.e. independently from the role played by labour inspection). Instead, central to the identification strategy of this paper is the fact that the rise in the number of inspectors was not uniform across departments. While the average increase in the number of inspectors per 100,000 employed individuals was equal to 67 per cent between 2010 and 2014 (from 2.05 to 3.43 inspectors per 100,000 employed); this increase varied from a minimum of 21 per cent (from 3.56 to 4.31 in the department of Caldas) to a maximum of 208 per cent (from 1.04 to 3.21 in the department of Caquetá – see Figure 2). This generates differences in programme intensity that can be exploited to estimate causal effects under the (weaker) assumption that trends in the outcomes of interest would have not systematically differed between departments in the absence of the programme. This assumption requires that there are no other (policy or economic) shocks taking place at the department level that could influence the evolution of the outcome of interest. While there were obviously multiple policy reforms during the time of the analysis (e.g. fiscal reform in 2012, law of first employment in 2011, other measures implemented as part of the Action Plan), there is no evidence that any of them presented systematic differences across departments. This identification strategy has already been adopted in the literature to study the effect of an increase in the number of schools on educational attainments (Duflo 2001), the effect of an increase in female labour supply on the wage structure (Acemoglu et al. 2004) and the effect of a shift in sex-specific income on sex-differential survival rates of children (Qian 2008).²⁷

²⁷ This approach has also been used specifically in studies on labour legislation, adopting either a difference-in-difference framework (Micco and Pages 2007) or an event study approach (Ahsan and Pages 2007, Amin 2007, Autor et al. 2007 Besley and Burges 2004). From a methodological point of view the paper closest to this one is Almeida and Poole (2017), which exploits a policy shock for identification purposes and uses individual level data on compliance matched with administrative measures of enforcement at the city level.

Figure 2 – Actual change in the number of inspectors (per 100,000 employed)



Source: Author's calculations based on GEIH and administrative data.

Compared to these studies, I am able to complement this difference-in-difference framework with an instrumental variable approach that takes into account the possible endogeneity of the implementation of the reform. Indeed and as discussed above, the hiring targets set by the central Government were systematically missed by the departments. This mostly related to the challenges of scaling-up so rapidly the existing system of labour inspection, with strong disparities across departments in the capacity of the public administration to hire and retain civil servants (OECD 2016). Indeed, human resources capacities to oversee competitions and recruitments in the public sector remains limited at the local level and this resulted into large disparities in the capacity to achieve the target number of inspectors within the established timeframe. As a result, the planned target of 904 inspectors (originally set for 2014) was still to be met in 2016. Since the hiring process took place at the department level, this introduces a possible source of endogeneity. In particular, the ability of the different departments to comply with the policy reform might be correlated with time-varying characteristics at the department level that are not accounted for by the department fixed effects. For instance, the motivation to implement the Action Plan might have varied according to the political stance of the local government (or its alignment with the central government). Similarly, the hiring of new inspectors might have changed the behaviour of inspectors that were already working in the labour office. As an extreme, it could be assumed that local governments received pressures from lobby groups not to hire the new inspectors. For this reason, I instrument the change in

the number of inspectors working in a given department between 2010 and 2014 with the theoretical change that should have taken place during the same time in that department according to the Action Plan. The rest of the section discusses the validity of the identification assumptions behind this estimation strategy.

A. Common trends

The main idea behind the identification strategy can be presented using a two-by-two table (Duflo, 2001). Table 2 presents differences in the rates of formal employment for the four groups of departments divided according to the intensity of the policy (low, medium,-low, medium-high and high, as defined above) before and after the implementation of the Action Plan. Panel A shows that before the policy was implemented (2008 to 2011), the rate of formal employment was essentially constant in all four groups of departments – providing suggestive evidence in favour of the common trend assumption. At the same time, Panel B shows that in the period after the policy change (i.e. 2011 to 2014) formal employment was increasing in all groups of departments. However, the rate of increase was higher in the departments characterised by higher policy intensity (i.e. 6 versus 3 percentage points). The difference in this difference can be interpreted as the causal effect of the programme. For instance, the first and fourth quartiles of departments experienced a difference in the intensity of the policy by around one inspector per 100,000 employed individuals (Table 1). Considering pre-treatment trends, this seems to have generated an increase in formal employment by around 2 percentage points – which can be tentatively ascribed to the programme. However, this is only suggestive evidence based on data at the department level and few observations. Section VI will provide more robust tests for the validity of the common trend assumption as well as more precise estimates of the causal effect.

Table 2 – Means of rates of formal employment

Panel A: Control Experiment (2008-2011)				
	Low	Medium-low	Medium-high	High
2008	0.24	0.33	0.24	0.21
2011	0.24	0.34	0.24	0.22
Difference	0.00	0.01	0.00	0.01
Panel B: Experiment of interest (2011-2014)				
	Low	Medium-low	Medium-high	High
2011	0.24	0.34	0.24	0.22
2014	0.27	0.37	0.28	0.28
Difference	0.03	0.03	0.04	0.06

Note: Departments are divided into quartiles (low, medium-low, medium-high and high) according to the intensity of the policy as captured by the total theoretical change in the number of labour inspectors (per 100,000 employed) between 2010 and 2014.

B. Allocation of inspectors

The next step concerns understanding the determinants of the allocation of inspectors across departments made by the central Government (Acemoglu et al., 2004). As this decision was unlikely to be random, this might generate concerns over the validity of the identification strategy if the determinants behind the allocation rule can (directly or indirectly) affect the (trends in the) outcome of interest. Unfortunately, there was no clear rule that was defined (or made public) by the central Government to allocate the newly hired inspectors. The legislation simply stated that the Ministry of Labour would have allocated the new inspectors across departments taking into account the “structure, plans, programmes and necessities of the services of the Ministry” (Government of Colombia, 2011). This complicates the analysis compared to a case in which a policy is implemented gradually across regions for some reasons that can be formally identified in the legislation (e.g. funding availability) and controlled for by the researcher. However, it is important to note the necessary assumption for identification purposes in this paper is not that inspectors have been randomly allocated across departments (this is unlikely to have happened) – nor that the allocation was random with respect to rates in formal employment. Rather, identification requires that the allocation of inspectors across departments was random with respect to trends in formal employment rates. While it is impossible to rule out that this was the case, the analysis presented above has shown how pre-treatment trends in the outcome of interest were not systematically different across departments characterised by different levels of policy intensity.

In order to further investigate the assignment mechanism, I regress the theoretical change in the number of inspectors per 100,000 employed individuals between 2010 and 2014 (the instrumental variable in the rest of the analysis) on department level characteristics at baseline in a single cross section analysis for 2011.²⁸ This is meant to provide suggestive evidence on whether the (future) intensity of the policy is correlated with some department level characteristics at baseline. The covariates in this regression include the number of inspectors per 100,000 employed in 2010 (a proxy for the strength of enforcement in the department before the policy came into effect), the initial rate in the outcome of interest (i.e. formal employment) and its three-year change (to cover the entire period of the analysis), the rate of employment in sectors identified as priority areas in the Action Plan and the two main

²⁸ The analysis conducted using the actual (rather than the theoretical) change yields similar results. However and since the objective is to understand the determinants behind the allocation rule made by the Government, the theoretical change is preferred in this step.

indicators of the quality of the public administration from the EDID. The results are available in Table 1 in Appendix A and they provide suggestive evidence that the allocation of inspectors across departments followed (i) policy priorities as set by the Action Plan; and (ii) internal organizational needs within the Ministry of Labour.

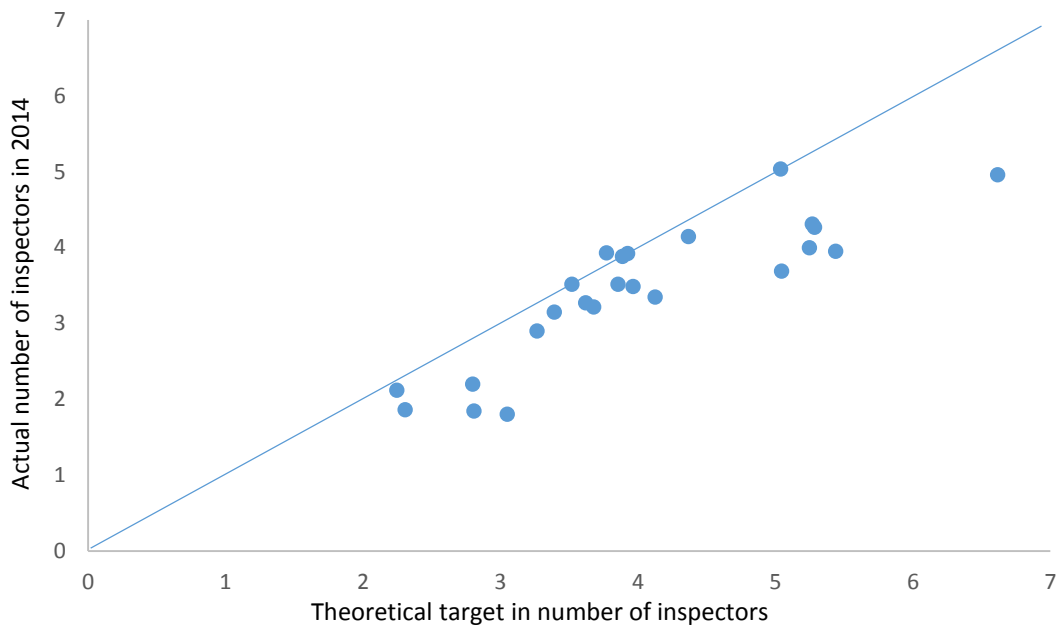
In particular, it appears that the only regressor whose coefficient is (positive and) large in magnitude throughout the different specifications is the share of employment in priority areas – although it never reaches statistical significance. As expected, departments characterised by higher shares of employment in priority sectors as identified by the Action Plan obtained a higher increase in the number of inspectors. Similarly, there is a positive (and close to significant) relationship between the intensity of the policy and the level of inspection in 2010. This suggests that the new inspectors were allocated proportionally across departments based on the initial level of enforcement (and possibly the existing organizational capacities of the Ministry of Labour) in a given department. All other coefficients are far from statistical significance and/or substantially change in magnitude when adding additional covariates. In particular, there is a negative (but weak) association between the intensity of the policy and the rate of formal employment (as well as its three year change, pointing to the fact that the analysis might underestimate treatment effects). Similarly, only very weak relationships are found between the intensity of the policy in a given department and the two measures of the quality of the public administration. Overall, this provides suggestive evidence that the allocation rule – despite not being made explicit – followed determinants (e.g. share of employment in priority areas, initial level of enforcement) that can be controlled for and should not represent a threat to the estimation strategy. However, the analysis has only very few observations and the results are merely suggestive.

C. Instrumental variable

As a final step, I will explicit the rationale of the instrumental variable approach that completes the identification strategy. Indeed and as discussed above, the hiring targets in terms of new inspectors set by the Action Plan were systematically missed by the departments. For this reason, the target of 904 inspectors set for 2014 was still to be met in 2016. Figure 3 plots the relationship between the total hiring target and the actual number of inspectors operating in each department in 2014 – and in this sense represents the first stage relationship of the analysis. As shown in the figure, not all departments missed the hiring target set by the central government. Rather, some departments had met the target and are therefore on the 45 degree

line; while other departments are on the right of the line. This might generate concerns if we were to directly use the number of inspectors operating in a department as the treatment indicator – insofar as missing the target is unlikely to be purely random and it might reflect time-varying and unobservable characteristics at the department level (which would possibly bias the results, as discussed above). For this reason, the paper uses the target change in the number of inspectors set by the Action Plan as an instrument for the actual change in the number of inspectors operating in the department.

Figure 3 – Target and actual number of inspectors (per 100,000 employed)



Note: the figure shows the relationship between the target number of inspectors per 100,000 employed individuals as set by the Action Plan and the actual number of inspectors operating in a given department in 2014. The distance with the 45 degree line therefore measures the implementation gap of the Action Plan.

This instrumental variable strategy has several empirical benefits and has been already used in the literature (De Giorgi et al. 2015). First, the first stage relationship between the instrumental variable and the endogenous treatment indicator is generally very strong. This will further be proved in the regression analysis, but it can already be deduced by Figure 3 above. Secondly, the monotonicity assumption is likely to hold since a higher target has generally resulted into more inspectors *ceteris paribus*. A possible concern could be associated with the presence of the so-called always takers (i.e. departments that would have hired additional inspectors also in the absence of the policy), but this was unlikely to happen given that resources should have been made available by the central Government. Finally, the exclusion restriction seems

realistic as issuing a new vacancy can have an impact on formal employment only through the new inspector that is eventually hired.²⁹

The main threat to the violation of the exclusion restriction would be associated to the possible presence of a scare effect (economic agents reacting to the announcement of the policy and therefore anticipating its effects). However, these dynamics are more likely to have taken place for the Action Plan as a whole – rather than for the single new vacancy opened in the specific department. In this sense, adopting a continuous (rather than binary) treatment indicator (and instrumental variable) should alleviate these concerns. Of course, it is still impossible to rule out the possible presence of anticipation effects of the Action Plan. However, some details of the policy could alleviate this concern. In particular, the Action Plan was not debated in the national political arena (but rather between Governments of two different countries) and it was rapidly implemented (first call for applications was released already in April when the Action Plan was signed, with the aim of hiring 100 inspectors already in 2011). In any case, the empirical analysis below will look at the timing of treatment effects in order to (also) investigate the possible presence of anticipation effects.

Despite the fact that the instrumental variable strategy should address concerns over the possibly endogenous implementation of the policy, it is still important to understand the determinants of the policy implementation gap. To this end, I regress the implementation gap (i.e. difference between theoretical and actual number of inspectors per 100,000 employed individuals in 2014) on the intensity of the policy in a given department (as measured by the total theoretical change in the number of inspectors between 2010 and 2014) and some measures of the quality of the public administration in the department in 2014 from the EDID.³⁰ The results (Table 2 in Appendix A) reveal how the implementation gap is positively associated with the intensity of the policy in a given department. Intuitively, departments characterised by higher hiring targets struggled to meet them. This result is in line with qualitative evidence presented in policy reports on the implementation of the Action Plan, which showed how labour offices struggled to rapidly increase their staff in accordance with the Action Plan and many vacancies remained unfilled (and were re-posted) due to the absence of available candidates

²⁹ As a final point, it is worth mentioning that this instrumental variable approach allows only for unilateral deviations of the endogenous regressor with respect to the instrument. While some departments have missed the target; there is no department that has exceeded it (i.e. all dots – but one – lie on or to the right of the 45 line in Figure 3). This is a case similar to Angrist (2006), but here the treatment indicator takes a continuous form.

³⁰ In particular, I include the two main indicators of institutional environment and institutional performance as well as their different sub-indicators.

(Government of Colombia, 2013). Additionally, the analysis reveals a negative (but insignificant) relationship between the implementation gap and the two main indicators of quality of the public administration (i.e. institutional environment and institutional performance). Intuitively, these results would indicate that less efficient public administrations were less likely to meet the hiring targets set by the Action Plan. Even in this case however, the analysis is based on few observations and the results are only suggestive.

VI. Estimation results

A. Basic results

The identification strategy introduced in the section above can be generalised to a regression framework (Duflo 2001). Consider the difference in the probability of being in a formal job between a cohort of employed individuals exposed to the programme (i.e. interviewed between 2011 and 2014) and a cohort of employed individuals not exposed to the programme (i.e. interviewed in 2008 and 2010). If the hiring of additional inspectors led to higher labour law compliance, this differences will be positively associated with the number of inspectors hired in a particular department. In practice, I will run the following regression:

$$FE_{i,s,t} = c + \alpha_t + \delta_s + \beta * (X_{i,s,t} * Post) + \gamma * (C_s * Post_t) + \theta * (P_s * Post_t) + \varepsilon_{i,s,t} \quad (1)$$

where $FE_{i,s,t}$ is a dummy for formal employment for individual i , employed in department s , in year t (i.e. taking the value of one if the individual is in formal employment); c is a constant, α_t is the set of year dummies, δ_s represents the department dummies and $Post_t$ is a dummy taking the value of one after the policy has been implemented (from 2011 onwards in all regions). The vector C_s contains department specific variables that the analysis presented above has shown that could affect the estimation of the casual effect in the present context. In particular, I include the strength of labour inspection at baseline and the rate of employment in Action Plan priority sectors at baseline – as the analysis has shown that these were two possible determinants of the allocation of inspectors as part of the Action Plan. Similarly, I include the rate of formal employment (always at baseline) due to initial differences across departments characterised by different future intensity of the policy (as reported in Table 1 above). $X_{i,s,t}$ is instead a vector of individual characteristics that – according to previous studies – might affect compliance with the labour legislation (Almeida and Carneiro 2009, Borat et al. 2012). In particular, I add dummy variables for living in rural areas (that might decrease the probability of being visited by labour inspectors), the size of the establishment where the individual works

(as inspectors are more likely to target large firms) and the economic sector of employment (labour inspectors tend to target sectors with higher risk of non-compliance). Finally, P_s represents the intensity of the policy (actual change in the number of inspectors per 100,000 employed individuals in a given department between 2010 and 2014) and is the main regressor of interest. As discussed above, this is instrumented by the planned change in the number of inspectors that should have taken place in that department between 2010 and 2014 according to the Action Plan.

A linear probability model is preferred to discrete choice models in order to avoid the risk of inconsistent estimates in case of misclassification of the dependent variable (Hausman et al. 1998). The Heckman correction for sample selection is included to take into account that the observed outcome of interest (i.e. formal employment) is recursively defined for individuals that are already in employment.³¹ Since the treatment indicator is constant over time for each specific department, standard errors are clustered at the department level (Abadie et al. 2017).³²

Looking at the baseline specification, Table 3 presents the results of different estimations of equation (1) – with additional sets of covariates added sequentially (columns 1 to 3) and OLS and IV results (left and right panel, respectively). The only coefficient reported in the Table refers to the effect of the policy (θ in the notation of the equation above). The estimates suggest that (in the most complete specifications), an additional inspector per 100,000 employed individuals increases the probability of being in a formal job by 1.3 percentage points in the linear probability model and 2.1 percentage points in the instrumental variable results. These estimates are economically significant and in line with the findings obtained by previous studies. For instance, Ronconi (2010) finds that an additional inspector per 100,000 people increases compliance with mandated benefits by 1.4 percentage points in Argentina. Additionally, the point estimates do not substantially change when adding additional (department or individual) covariates – despite the R-squared substantially increases, especially in the third specification. This suggests that treatment effects are neatly identified and the analysis does not suffer from omitted variable bias. First stage results show a very strong relationship between the instrument and the endogenous regressor – as expected given

³¹ This correction uses as exclusion restriction whether the individual is paying or not for the house where she lives (e.g. rent, mortgage). In the robustness tests, the model is also run without the Heckman correction.

³² The robustness tests will control for the presence of few clusters (i.e. corresponding to the 24 departments) and also change the level at which standard errors are clustered.

the nature of the instrument – and the test of exogeneity generally confirms the soundness of opting for an instrumental variable approach.³³

The difference between linear probability and instrumental variable results point towards an underestimation of results obtained from the linear probability model (of around one percentage point). As suggested in previous studies, this might be explained by the fact that enforcement efforts are generally focused towards areas characterised by a higher risk of non-compliance (Ronconi 2010). This is in line with (i) the fact that the policy target of the Action Plan was higher in those departments already characterised by a higher number of inspectors (and possibly a higher risk of non-compliance, as measured by the lower rate of formal employment and the higher rate of employment in priority sectors) before the implementation of the reform (Section V.B); and (ii) the evidence presented above that the implementation gap was higher in departments with a higher policy target (Section V.C). In this sense, the implementation gap was probably not random but rather positively associated with the strength of the policy in a given department and (consequently) with the risk of non-compliance in that department. For this reason, simple OLS estimates of the effect of inspection risk underestimate the causal effect of the intervention.

Table 3 – Treatment effects on formal employment

	LPM			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
θ	0.0184** (0.00672)	0.0212*** (0.00735)	0.0135* (0.00775)	0.0303*** (0.00620)	0.0306*** (0.00637)	0.0211*** (0.00777)
R-squared	0.061	0.062	0.336			
First stage (F)				56.4659	56.1937	56.2705
Test of exogeneity (p)				0.0087	0.0225	0.1263
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

³³ The first stage equation contains the full set of covariates included in the second stage and the results can be seen in Table 5 in Appendix B.

B. Interaction term analysis

The results discussed above show how estimates are robust to the inclusion of additional covariates – thus potentially ruling out that they are spuriously driven by differences in (the levels of) either individual or departmental characteristics. However, they do not shed light on the possible presence of differential trends in the outcomes of interest before the implementation of the policy change (section V.A provided some suggestive evidence at the department level in this respect). For this reason, the continuation of the analysis will use interaction terms with single years before and after the policy change.³⁴ In particular, the identification strategy presented before can be extended to a specification with multiple interactions (Duflo, 2001). This has the advantage of allowing to test the common trend assumption as well as providing more insights on the effect of the policy over the years after its implementation. In particular, I run the following specification:

$$FE_{i,s,t} = c + \alpha_t + \delta_s + \beta_t * \sum_{t=2009}^{2014} (X_{i,s,t} * d_t) + \gamma_t * \sum_{t=2009}^{2014} (C_s * d_t) + \theta_t * \sum_{t=2009}^{2014} (P_s * d_t) + \varepsilon_{i,s,t} \quad (2)$$

where d_t is a dummy that indicates the year in which the individual appears in the sample (a year dummy); while all other variables are indexed as before. Individuals interviewed in 2008 (first year in the sample) constitute the control group and that year dummy is omitted. Figure 4 plots the estimates of the interaction term between the dummy of being in a given year and the policy intensity in a particular department – while the left panel of Table 4 reports the results with the three different specifications as presented before (i.e. with no controls in the first column, adding departmental controls in the second column and individual controls in the third column). There are no notable differences between the instrumental variables and linear probability estimations and for ease of expositions only the former are reported in Table 4. The coefficients of the effect of the policy remain around zero until 2010 and start increasing afterwards, becoming statistically significant in 2012 and stabilising in 2013.³⁵ As hypothesised, the programme had no effect on cohorts of individuals not exposed to it – while it had a positive effect on subsequent cohorts.

³⁴ Estimating the year-by-year effects can also shed light on the possible presence of anticipation effects if economic agents react to the announcement (rather than the implementation) of the reform.

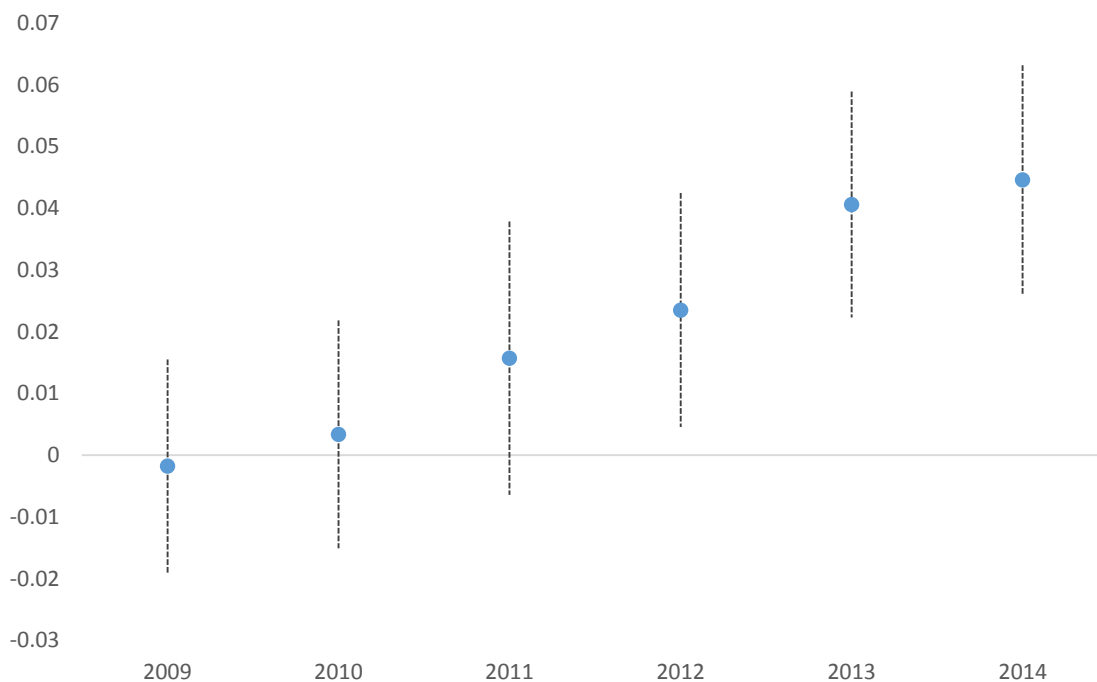
³⁵ The year of 2011 was a year of only partial implementation. Indeed, the first call for applications for labour inspectors was issued in April 2011 and it is also reasonable to expect some delays before the first newly hired inspectors started operating (e.g. recruitment process, training).

Rather than testing whether $\theta_t = 0$ for the years before the implementation of the policy, I can impose this restriction and study the effects of the policy in the years after its approval. This is more efficient and allows to obtain more precise estimates of the impact of the policy (Duflo 2011). In practice, I run the following regression:

$$FE_{i,s,t} = c + \alpha_t + \delta_s + \beta_t * \sum_{t=2011}^{2014} (X_{i,s,t} * d_t) + \gamma_t * \sum_{t=2011}^{2014} (C_s * d_t) + \theta_t * \sum_{t=2011}^{2014} (P_s * d_t) + \varepsilon_{i,s,t} \quad (3)$$

where all year dummies up to 2010 are set equal to zero. The results (Table 4, right panel) show that the estimates of the interaction between the intensity of the policy in the department and the year dummies are statistically significant and do not substantially change with respect to the previous specification (i.e. they are slightly smaller in magnitude). However, in this model the coefficients capturing the effect of the policy are significant already from 2011.

Figure 4 – Coefficients of the interactions between year dummies and the programme intensity in the department (and 95 per cent confidence intervals): Micro Analysis



Note: The figure shows the estimated coefficients of interaction terms between year dummies and the intensity of the programme (θ in the equation above) for regressions having as dependent variable formal employment. The blue points correspond to point estimates and the lines are the 95 per cent confidence intervals. The coefficients correspond to the IV estimations with only year and department dummies (IV Model 1 in Table 4).

Table 4 – Treatment effects on formal employment by year

	IV			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
⊕ 2009	-0.00179 (0.00882)	-0.00134 (0.00981)	0.00417 (0.00554)			
⊕ 2010	0.00335 (0.00941)	0.00260 (0.00976)	0.00384 (0.00337)			
⊕ 2011	0.0157 (0.0113)	0.0144 (0.0102)	0.00908** (0.00441)	0.0150** (0.00637)	0.0138*** (0.00521)	0.00621** (0.00280)
⊕ 2012	0.0235** (0.00966)	0.0250*** (0.00916)	0.0146*** (0.00318)	0.0228*** (0.00717)	0.0239*** (0.00743)	0.0114*** (0.00338)
⊕ 2013	0.0406*** (0.00934)	0.0409*** (0.00897)	0.0226*** (0.00488)	0.0400*** (0.00782)	0.0407*** (0.00823)	0.0199*** (0.00466)
⊕ 2014	0.0446*** (0.00945)	0.0449*** (0.00906)	0.0297*** (0.00591)	0.0440*** (0.00879)	0.0454*** (0.00923)	0.0272*** (0.00541)
Test of exogeneity (p)	0.000	0.000	0.000	0.000	0.000	0.000
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the year dummies from 2009 to 2014. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

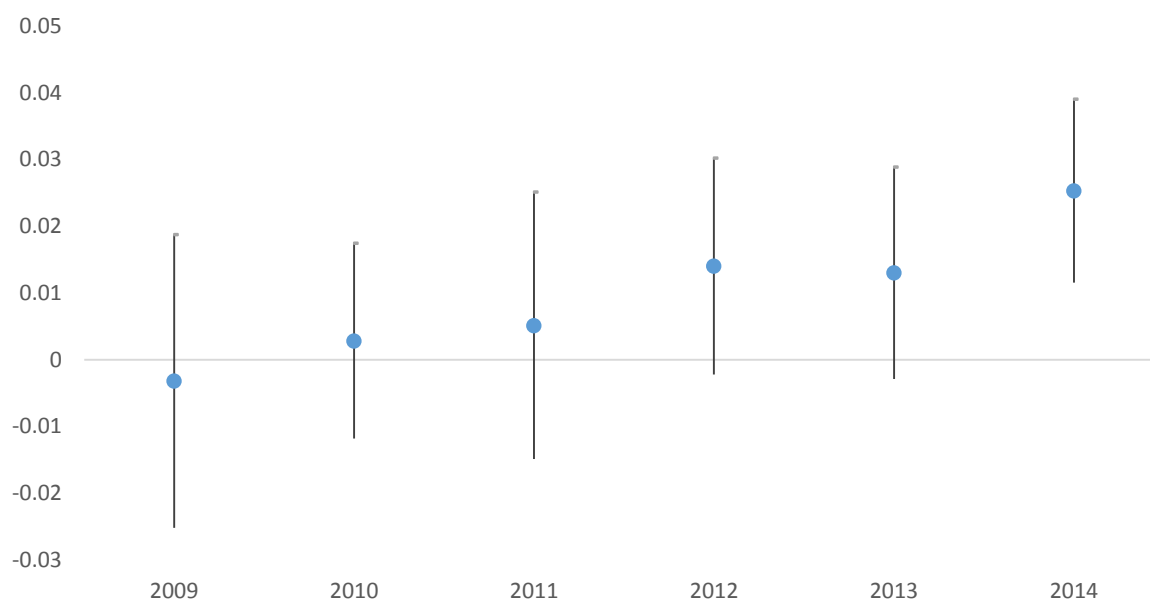
C. Robustness tests

The main concern on the estimation strategy presented above refers to the limited number of clusters (corresponding to 24 departments). Ideally, we would like to have a more detailed level of disaggregation that would better capture the level of enforcement to which individuals and enterprises are faced. For instance, Almeida and Carneiro (2012) use information on 5,242 Brazilian cities. However, labour inspection in Colombia is organised along the 32 departments and the GEIH provides information only for the 24 main departments (see section IV for details).³⁶ For this reason, more disaggregated information is unavailable neither for the outcome of interest nor for the treatment indicator. However, other studies have faced similar issues – generally owing to the traditional scarcity of information on labour law enforcement. In particular, Ronconi (2010) conducts the analysis at the provincial level using information on the 24 Argentinian provinces over successive years. Similarly, Borat et al. (2010) have data on enforcement disaggregated only at the level of the nine South African provinces – which they match with individual data from the labour force survey.

³⁶ The GEIH formally collects more detailed information on the place of residence of the individual, but only information on the department is publicly released.

In order to deal with this issue, I conduct two separate exercises. First, I re-run the baseline specification presented above but now with bootstrapped standard errors following the procedure proposed by Cameron et al. (2008). Indeed, the traditional adjustment of clustering standard errors assumes that the number of clusters tends to infinity. However, with few clusters the standard errors are downward biased. Table 1 in Appendix B presents the results of the baseline specification with this adjustment and shows how the estimates do not significantly change. As a second exercise, I conduct the analysis at the departmental level – where variations in treatment intensity actually occur. This is also useful to benchmark the results to those obtained by previous studies, which mostly conduct the analysis at the macroeconomic level – the province in the case of Ronconi (2010) and the city in Almeida and Carneiro (2012). To this end, I construct a database at the department level between 2008 and 2014 and run similar regressions as those presented above (e.g. formal employment of an individual i in department s in year t becomes the rate of formal employment in department s and year t). In particular, I present three different specifications (adding different set of controls) both with a simple post-policy dummy and in an interaction term setting (corresponding to Tables 3 and 4 above). Even in this case, the treatment indicator is instrumented with its planned change according to the Action Plan. The results of the post-policy dummy (Table 5, panel A) are still positive and significant and with a magnitude similar to the one presented above. Additionally, the interaction term analysis (left side of panel B and Figure 5) confirms the absence of effects before the implementation of the policy and a positive effect afterwards. The coefficients are however less precisely estimated and they gain statistical significance when setting all year dummies before 2011 equal to zero (panel B of Table 5, right side).

Figure 5 – Coefficients of the interactions between year dummies and the programme intensity in the department (and 95 per cent confidence intervals): Macro Analysis



Note: The figure shows the estimated coefficients of interaction terms between year dummies and the intensity of the programme (θ in the equation above) for regressions having as dependent variable formal employment. The blue points correspond to point estimates and the lines are the 95 per cent confidence intervals. The coefficients correspond to the IV estimations with only year and department dummies (IV Models 1 in Table 5).

Additional tests are then performed to check the robustness of the results to small changes in the estimation strategy. First, the baseline regression is run without the Heckman correction for sample selection. Indeed, there is no consensus in the literature on the choice of the exclusion restriction to account for the fact that job characteristics (such as formal employment) are observed only for individuals that are in employment. In this sense, running the model without correction for sample selection could provide for a benchmark on the validity of the results to changes in the choice of the exclusion restriction. The results are available in Table 2 in Appendix B (which follows the same structure as Table 3 above) and they reveal how the main conclusions generally hold even without the Heckman correction. In particular, the coefficients are slightly lower in magnitude (and in the linear probability model they lose significance) – which is in line with the fact that the analysis does not control any more for the non-random nature of the employed sample. In the second robustness test, I use standard errors clustered at the year and department level (rather than simply at the department level) to account for the fact that the analysis uses repeated cross-sections over multiple years. The results obtained with this correction (Table 3 in Appendix B) are once again in line with the baseline results discussed above. Additionally, I run the regression without data from the departments of Cundinamarca and Bogota – for which information on labour inspection needed to be reconstructed for the

first years in the sample (see section IV.B). However, even in this case the results are of similar magnitude and retain statistical significance (Table 4 in Appendix B). Finally, I run the regressions with all constitutive terms (rather than just their interactions with the post policy dummy). This is important to check for the possible presence of omitted variables (i.e. if the control variable by itself has a joint effect on the outcome of interest and the treatment indicator). The results are available in Table 6 in Appendix B and once again they confirm the main findings of the analysis.

Table 5 – Macroeconomic analysis on formal employment

Panel A: Post-policy dummy						
	LPM			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
θ	0.00980** (0.00442)	0.00834 (0.00525)	0.0101* (0.00502)	0.0146*** (0.00359)	0.0123*** (0.00434)	0.0132*** (0.00509)
R-squared	0.9929	0.9933	0.9925			
First stage (F)				41.9006	40.6554	54.376
Test of exogeneity (p)				0.186	0.3122	0.5232
Panel B: Interaction term analysis						
	(1)	(2)	(3)	(1)	(2)	(3)
Θ 2009	-0.00322 (0.0112)	-2.53e-05 (0.0133)	-0.000829 (0.0148)			
Θ 2010	0.00280 (0.00745)	0.00438 (0.00857)	0.00528 (0.00968)			
Θ 2011	0.00510 (0.0102)	0.00206 (0.0107)	0.00246 (0.0110)	0.00523 (0.00558)	0.000574 (0.00595)	0.000943 (0.00633)
Θ 2012	0.0140* (0.00826)	0.0128 (0.0100)	0.0127 (0.00950)	0.0141*** (0.00446)	0.0113** (0.00548)	0.0112** (0.00543)
Θ 2013	0.0130 (0.00809)	0.0133 (0.00919)	0.0138 (0.00922)	0.0131*** (0.00482)	0.0118** (0.00484)	0.0123** (0.00557)
Θ 2014	0.0253*** (0.00701)	0.0269*** (0.00749)	0.0299*** (0.00789)	0.0254*** (0.00388)	0.0254*** (0.00476)	0.0284*** (0.00610)
Test of exogeneity (p)	0.0003	0.0029	0.0005	0.0001	0.0008	0.0049
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	168	168	168	168	168	168

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the post policy dummy (Panel A) and year dummies between 2009 and 2014 (Panel B). Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in priority areas (all computed in 2010). Individual characteristics are the rate of residence in urban areas, the rate of employment in small businesses and the rate of employment in agriculture. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

VII. Heterogeneous effects

The key result that emerges from the analysis is that labour inspectors are effective in promoting compliance with the labour legislation (in terms of formal employment) and this result is both economically significant (around 2 percentage points for any additional inspector per 100,000 employed individuals) and in line with previous studies. In this section, I try to understand the mechanisms through which this effect operates and the groups of employed individuals that are more likely to be affected by the policy intervention. I will do so by looking at treatment effects on other outcomes of interest (i.e. different from formal employment) or by analysing the effects on formal employment for different groups in the population. In doing so, the analysis will also further discuss the validity of the identification strategy and results presented above (e.g. are pre-treatment trends parallel for different groups?).

A. General equilibrium effects

The discussion in Section IV has shown how increased enforcement of labour legislation increases formal employment. At the same time, it is important to analyse whether increasing labour law enforcement generates any general equilibrium effect on labour market outcomes. For instance, it could be hypothesised that better employment conditions might come at the expense of fewer employment opportunities. This trade-off between employment quality and quantity has been largely studied for both advanced and developing economies and it has also been explicitly taken into consideration in the area of labour inspection (Almeida and Carneiro 2012; Almeida and Poole 2017). In order to shed light on the possible presence of general equilibrium effects, I run the analysis discussed before but now using as outcome variables the binary variables for status in the labour market (i.e. employment, unemployment and inactivity). For ease of exposition, here and in the continuation of the paper I present only the instrumental variable results (with the three specifications adding one set of controls after the other, as explained above) and only the results of the year-by-year interaction between the treatment indicator and the year dummies – corresponding to Table 4 in the main analysis. Indeed, this specification allows at the same time to check for parallel trends and estimate the policy effect. The results of the analysis for the status in the labour market do not report any evidence of an effect of labour inspectors on employment, unemployment or inactivity (Table 3 in Appendix A).³⁷ In particular, the impact estimates for employment and unemployment

³⁷ Compared to the other estimations, the model now includes all the working age population (i.e. employed, unemployed and inactive) and the regressions do not present the Heckman correction for sample selection.

reveal slight differences in pre-treatment trends in 2009 (that become non-significant already in 2010) and no treatment effects after the programme implementation in 2011. At the same time, the estimates on unemployment are statistically non-significant and close to zero during the entire period under consideration. These results rule out the possible presence of negative employment effects arising from the increase in formal employment. At the same time, they are reassuring in excluding the possibility that the positive effect on formal employment was resulting simply from differing trends across departments in labour market trajectories.³⁸

B. Geographical targeting

In this sub-section, I try to analyse whether the positive treatment effect is homogenous across the economy – or if rather concerns only specific parts of the labour market. From a policy perspective, this is essential in order to understand which forms of labour law violations can be expected to be tackled by increased enforcement through labour inspection. At the same time, it allows to check if the parallel trend assumption was holding also for different subgroups in the population of interest. To this end, I conduct two simple exercises. First, I divide the sample between individuals resident in urban as compared to rural areas.³⁹ Indeed, previous studies have shown how travel distance represents one of the main constraints to the activities of labour inspectors (Almeida and Carneiro 2009 and 2012, Viollaz 2016a and 2016b), which might generate concerns over their ability to reach scarcely populated areas. This could be particularly important in Colombia, as labour inspectors do not have access to service vehicles – but they rather need to seek assistance from employers, workers or trade unions to finance their travel expenses (OECD 2016). The results of the analysis confirm findings from previous studies, as the positive treatment effect of labour inspection on formal employment materialises mostly in urban areas – while the coefficient is substantially smaller and only significant in 2014 in rural areas (Table 6). Another dimension along which it is interesting to examine the operations of labour inspectors is represented by firms' size. Indeed, previous studies have shown how inspectors tend to disproportionately focus on large establishments (Almeida and Carneiro 2009). This might be motivated by the fact that large enterprises are easier to find (especially if they operate in the informal sector). Additionally, by visiting large establishments labour inspectors might expect to maximise the number of workers covered by inspections – and eventually the amount of sanctions (Viollaz 2016b). For these reasons, I run the analysis

³⁸ I have also run similar regressions with dummies for status in employment (i.e. employee and self-employment status) to check if general equilibrium effects occurred in these dimensions. However, the results (available upon request) do not find any significant treatment effects on these variables.

³⁹ The definition of urban and rural areas is directly taken from the GEIH.

separately for individuals working in small (less than 10 employed individuals), medium (from 10 to 50) and large establishments (50 and above). The results of this exercise are available in Table 4 in Appendix A, which shows how indeed treatment effects are larger in medium and large enterprises compared to small ones (the coefficients are from two to three times smaller in small enterprises compared to medium and large establishments). However, treatment effects are positive and statistically significant in the three classes of establishments.⁴⁰

Table 6 – Treatment effects on formal employment in urban and rural areas

	Urban areas			Rural areas		
	(1)	(2)	(3)	(1)	(2)	(3)
⊕ 2009	-0.00700 (0.00848)	-0.00728 (0.00845)	0.000334 (0.00798)	0.00252 (0.0246)	0.00264 (0.0246)	0.00527 (0.0246)
⊕ 2010	-0.00110 (0.00981)	-0.00138 (0.00982)	0.00493 (0.00892)	-0.00405 (0.0228)	-0.00386 (0.0228)	-0.00140 (0.0226)
⊕ 2011	0.0131 (0.0116)	0.0139 (0.0102)	0.0222* (0.0124)	-0.00721 (0.0215)	-0.00758 (0.0215)	-0.0135 (0.0240)
⊕ 2012	0.0212** (0.00927)	0.0219*** (0.00826)	0.0258*** (0.00997)	8.48e-05 (0.0216)	-0.000276 (0.0217)	-0.00797 (0.0244)
⊕ 2013	0.0360*** (0.00917)	0.0368*** (0.00867)	0.0325*** (0.0106)	0.0243 (0.0201)	0.0239 (0.0201)	0.0132 (0.0247)
⊕ 2014	0.0382*** (0.0100)	0.0389*** (0.00960)	0.0376*** (0.0117)	0.0487** (0.0208)	0.0485** (0.0208)	0.0360 (0.0265)
Test of exogeneity (p)	0.001	0.002	0.007	0.000	0.000	0.013
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	2,031,900	2,031,900	2,031,900	219,267	219,267	219,267

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the year dummies from 2009 to 2014. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

C. Individual characteristics

The last dimension that is worth examining is the possible presence of a differential effect of the policy by individual characteristics. In theory, I should not expect any differential effect along personal characteristics as inspectors target and visit firms (rather than individuals) and they are obviously expected to offer an equal treatment to the different cases independently from personal and demographic characteristics. However, we might still think that specific

⁴⁰ I also run the regression separately for individuals employed in economic sectors identified by the Action Plan as priority areas (results available upon request). Contrary to the predictions, treatment effects are non-significant in priority sectors. However, it is worth mentioning that no information is available on the inspectors allocated to those sectors specifically (but only on those assigned to a given department) – so that the exact policy intensity for these sectors is measured with error.

groups of individuals (e.g. youth, women) self-select into firms characterised by a different probability of complying with the legislation (or of being inspected). This could refer for instance to firms in specific economic sectors or geographical areas. The present analysis has already controlled for some of these characteristics that potentially jointly determine self-selection into a specific firm and the probability of being inspected (e.g. economic sector, department dummies and rural areas). In this sense, if the model is properly specified I should not expect to obtain any substantially different policy effects over personal characteristics. The results of the analysis are presented in Table 7, where I split the sample with respect to gender – while results by educational attainments are available in Table 5 in Appendix A. According to the predictions, treatment effects are positive and significant for both men and women as well as for low- and high-educated individuals (where the distinction is based on having completed the high-school). Additionally, the magnitude of the coefficients is very similar – with slightly larger effects for women and high-educated individuals.

Table 7 – Treatment effects on formal employment by gender

	Men			Women		
	(1)	(2)	(3)	(1)	(2)	(3)
⊕ 2009	-0.0147 (0.0143)	-0.0148 (0.0144)	0.00317 (0.00928)	-0.0227 (0.0146)	-0.0230 (0.0148)	-0.00136 (0.00913)
⊕ 2010	-0.00795 (0.0167)	-0.00807 (0.0169)	0.00731 (0.00944)	-0.0138 (0.0214)	-0.0142 (0.0216)	0.00497 (0.0100)
⊕ 2011	0.0103 (0.0153)	0.0119 (0.0146)	0.0152 (0.0118)	0.0125 (0.0182)	0.0139 (0.0175)	0.0245** (0.0110)
⊕ 2012	0.0234 (0.0184)	0.0250 (0.0183)	0.0204** (0.00978)	0.0276 (0.0247)	0.0289 (0.0249)	0.0305*** (0.0113)
⊕ 2013	0.0528** (0.0207)	0.0543*** (0.0208)	0.0351*** (0.0112)	0.0585** (0.0243)	0.0598** (0.0250)	0.0386*** (0.0108)
⊕ 2014	0.0587*** (0.0194)	0.0602*** (0.0198)	0.0439*** (0.0109)	0.0617*** (0.0218)	0.0629*** (0.0228)	0.0430*** (0.0109)
Test of exogeneity (p)	0.001	0.003	0.046	0.0001	0.001	0.0486
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	No	Yes	No	No	Yes
Individual covariates	No	Yes	Yes	No	Yes	Yes
N	1,243,431	1,243,431	1,243,431	1,007,736	1,007,736	1,007,736

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

VIII. Conclusions

Labour inspection represents one of the main policy instruments available to governments in order to ensure compliance with labour legislation and the respect of rights at work. This is especially the case in developing and emerging economies, where mostly informal labour markets are characterised by systematic breaches of the terms of the employment relation (e.g. lack of social protection coverage, non-respect of minimum wage legislation) while other institutional characteristics (e.g. prevalence of small businesses, absence of trade unions) limit workers' capacity to voice their concerns. Despite the widespread reliance on labour inspection, few available studies exist to assess its effectiveness. This can (at least partially) be connected to econometric identification problems as well as the lack of adequate data on both labour inspection and labour law compliance.

In order to assess the effectiveness of labour inspection, this paper uses a natural experiment generated by the Colombian Action Plan Related to Labour Rights, which was approved as part of the negotiations over a bilateral trade agreement between the US and Colombia. The Action Plan was the single most drastic change in labour inspection in the country, following which the number of inspectors more than doubled in four years. Critical to the identification strategy of this paper, the intensity of the policy differed markedly across departments. Combining this geographical variation with time differences in the exposition to the policy for subsequent cohorts of employed individuals, I find that an additional inspector per 100,000 employed individuals increases formal employment by around 2 percentage points. In order to take into account the possible endogeneity in the implementation of the policy, I instrument the actual change with the planned change. The results reveal that simple OLS estimates risk to downward bias the parameter of interest, potentially due to the simultaneous relationship between the strength of enforcement and the risk of non-compliance.

I also find that the positive effect on formal employment does not generate general equilibrium consequences on indicators of employment quantity (i.e. employment, unemployment and inactivity). This result partially contradicts findings from previous studies that found evidence of a trade-off between employment quality and quantity as a result of stricter enforcement (Almeida and Carneiro 2009). However, the coverage of labour inspection seems concentrated in urban areas (labour offices are generally placed in cities and travel costs represent a major constraints for inspectors, according to previous studies) and in large establishments (that maximise the number of workers covered with an inspection). In this sense, inspectors seem to

be relatively more effective in promoting labour law compliance in sectors of the economy that are already (at least partially) covered by labour market institutions. For this reason, inspectors might miss the most severe forms of non-compliance with the legislation.

While this paper identifies the effect of labour inspectors on compliance, several possible extensions are worth considering. First, with the available data I am not able to understand which particular type of labour inspection instrument is effective in ensuring compliance (e.g. visits, fines, technical assistance). Secondly, a more precise measure of enforcement would require having data disaggregated at a lower geographical level (e.g. municipality, province) compared to the department. Finally, the analysis does not shed light on whether the positive effect of inspectors on compliance occurs only as a result of the reform or if it would be rather sustained also in equilibrium.

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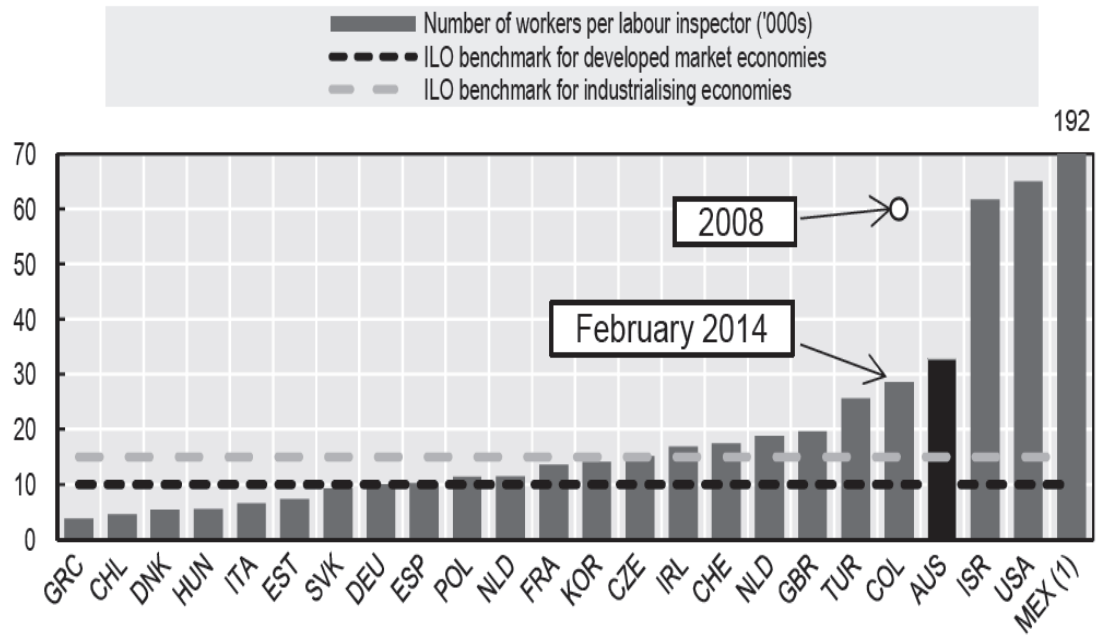
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Appendix A

A. Additional figures

Figure 1: Number of workers per labour inspectors



Source: OECD (2016)

B. Additional results

Table 1 – Determinants of the allocation of inspectors across departments

	Total theoretical change in the number of inspectors per 100,000 employed individuals (2010-2014)					
	(1)	(2)	(3)	(4)	(5)	(6)
Rate formal employment	-1.291 (1.375)					-0.513 (1.624)
Δ Rate formal employment (2008-11)		-0.235 (6.821)				
Rate of employment in priority areas			3.218 (3.579)			3.663 (6.388)
Inspectors (per 100,000)				0.451 (0.277)		0.427 (0.351)
Institutional environment					-0.653 (1.664)	-0.626 (1.859)
Institutional performance					0.735 (1.652)	0.883 (2.013)
Constant	2.297*** (0.453)	1.964*** (0.178)	1.896*** (0.189)	0.943 (0.570)	1.674 (3.213)	0.137 (4.763)
Observations	24	24	24	24	24	24
R-squared	0.027	0.000	0.017	0.133	0.005	0.161

Note: Robust standard errors in parenthesis. The dependent variable is the change in the number of inspectors per 100,000 employed individuals between 2010 and 2014. * p<0.1, ** p<0.05, *** p<0.01.

Table 2 – Determinants of the implementation gap

	Gap between theoretical and actual number in inspectors per 100,000 employed individuals in 2014					
	(1)	(2)	(3)	(4)	(5)	(6)
Total theoretical change in inspectors (2010-14)	0.403*** (0.0813)					0.392*** (0.0926)
Institutional environment		-0.387 (0.523)				-0.857 (0.965)
Credibility in the rules			0.571 (1.201)			
Credibility in the policies			-0.0338 (1.452)			
Adequacy of resources and predictability			-0.486 (1.050)			
Institutional performance				-0.111 (0.404)		0.693 (0.712)
Result based management					-0.353 (1.734)	
Accountability					0.895 (1.332)	
Labour welfare					-2.443 (1.786)	
Prevention of irregular practices					0.568 (0.805)	
Development planning and citizen participation					0.294 (1.226)	
Constant	-0.202 (0.178)	2.044 (1.967)	0.271 (2.069)	0.981 (1.440)	4.703 (2.774)	0.579 (1.888)
Observations	24	24	24	24	24	24
R-squared	0.336	0.023	0.027	0.003	0.134	0.360

Note: Robust standard errors in parenthesis. The dependent variable is the difference between the theoretical and actual number of inspectors per 100,000 employed individuals in 2014. * p<0.1, ** p<0.05, *** p<0.01.

Table 3 – Treatment effects on employment indicators

	Employment			Unemployment			Inactivity		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Θ 2009	0.0202** (0.00957)	0.0184** (0.00902)	0.0200** (0.00929)	-0.00165 (0.00669)	0.00106 (0.00596)	4.77e-05 (0.00611)	-0.0186** (0.00882)	-0.0194** (0.00909)	-0.0200** (0.00911)
Θ 2010	0.0181 (0.0133)	0.0158 (0.0129)	0.0183 (0.0134)	-0.00231 (0.00740)	0.000146 (0.00714)	-0.00124 (0.00757)	-0.0158 (0.0116)	-0.0160 (0.0121)	-0.0170 (0.0120)
Θ 2011	0.00696 (0.0102)	0.00387 (0.00943)	0.00654 (0.0103)	0.000224 (0.00553)	0.00107 (0.00585)	0.000429 (0.00616)	-0.00719 (0.00966)	-0.00494 (0.00957)	-0.00697 (0.00985)
Θ 2012	0.00202 (0.0142)	-0.000913 (0.0135)	0.000497 (0.0138)	0.00163 (0.00544)	0.00322 (0.00554)	0.00263 (0.00589)	-0.00364 (0.0124)	-0.00231 (0.0128)	-0.00313 (0.0127)
Θ 2013	-0.0112 (0.0160)	-0.0138 (0.0156)	-0.0124 (0.0159)	-0.000852 (0.00491)	4.16e-05 (0.00535)	-0.000315 (0.00559)	0.0120 (0.0133)	0.0138 (0.0134)	0.0127 (0.0135)
Θ 2014	-0.0123 (0.0147)	-0.0133 (0.0150)	-0.0130 (0.0152)	4.35e-05 (0.00534)	0.00248 (0.00556)	0.00165 (0.00599)	0.0123 (0.0121)	0.0108 (0.0132)	0.0114 (0.0133)
Test of exogeneity (p)	0	0.0001	0	0.4714	0.6926	0.6567	0.0008	0.0017	0.0016
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes	No	No	Yes
N	4,144,701	4,144,701	4,144,701	4,144,701	4,144,701	4,144,701	4,144,701	4,144,701	4,144,701

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the year dummies from 2009 to 2014. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

Table 4 – Treatment effects on formal employment by firm size

	Small firms			Medium firms			Large firms		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
⊕ 2009	0.00161 (0.00513)	0.00158 (0.00513)	0.00198 (0.00519)	0.0273 (0.0169)	0.0276 (0.0172)	0.0280 (0.0173)	0.0158* (0.00889)	0.0163* (0.00899)	0.0165* (0.00903)
⊕ 2010	0.00465 (0.00359)	0.00463 (0.00364)	0.00519 (0.00376)	0.0167 (0.0148)	0.0172 (0.0151)	0.0179 (0.0152)	0.0168 (0.0136)	0.0174 (0.0138)	0.0178 (0.0139)
⊕ 2011	0.00456 (0.00479)	0.00516 (0.00505)	0.00323 (0.00477)	0.0305 (0.0204)	0.0249 (0.0156)	0.0337** (0.0167)	0.0334* (0.0182)	0.0315* (0.0163)	0.0316** (0.0161)
⊕ 2012	0.00848* (0.00506)	0.00906** (0.00453)	0.00732 (0.00452)	0.0498** (0.0227)	0.0440*** (0.0167)	0.0523*** (0.0181)	0.0323*** (0.0115)	0.0304*** (0.0104)	0.0306*** (0.0104)
⊕ 2013	0.0143*** (0.00440)	0.0150*** (0.00408)	0.0130*** (0.00454)	0.0583*** (0.0181)	0.0524*** (0.0128)	0.0615*** (0.0132)	0.0498*** (0.0184)	0.0478*** (0.0174)	0.0477*** (0.0169)
⊕ 2014	0.0203*** (0.00466)	0.0209*** (0.00390)	0.0188*** (0.00436)	0.0763*** (0.0183)	0.0709*** (0.0141)	0.0779*** (0.0143)	0.0545** (0.0213)	0.0526*** (0.0201)	0.0527*** (0.0198)
Test of exogeneity (p)	0.3673	0.4636	0.5372	0.0051	0.0076	0.0128	0.0151	0.0092	0.0134
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes	No	No	Yes
N	1,564,643	1,564,643	1,564,643	168,858	168,858	168,858	517,666	517,666	517,666

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the year dummies from 2009 to 2014. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

Table 5 – Treatment effects by education levels

	High-educated			Low-educated		
	(1)	(2)	(3)	(1)	(2)	(3)
⊖ 2009	0.0106 (0.00908)	0.00984 (0.00865)	0.00908 (0.00842)	-0.00124 (0.00736)	-0.00108 (0.00743)	0.00541 (0.00691)
⊖ 2010	0.0172 (0.0140)	0.0164 (0.0134)	0.0158 (0.0134)	0.00473 (0.00695)	0.00494 (0.00706)	0.0107* (0.00632)
⊖ 2011	0.0324* (0.0190)	0.0341* (0.0192)	0.0426** (0.0167)	0.0138 (0.00876)	0.0139* (0.00760)	0.0143* (0.00860)
⊖ 2012	0.0310** (0.0148)	0.0327** (0.0140)	0.0495*** (0.0163)	0.0199*** (0.00755)	0.0200*** (0.00737)	0.0166** (0.00746)
⊖ 2013	0.0446*** (0.0165)	0.0462*** (0.0163)	0.0567*** (0.0169)	0.0361*** (0.00764)	0.0362*** (0.00762)	0.0244*** (0.00768)
⊖ 2014	0.0442** (0.0180)	0.0458** (0.0181)	0.0579*** (0.0176)	0.0400*** (0.00752)	0.0401*** (0.00727)	0.0316*** (0.00776)
Test of exogeneity (p)	0.0016	0.0002	0.0026	0	0.0004	0.0324
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	312,219	312,219	312,219	1,938,948	1,938,948	1,938,948

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

Appendix B: Robustness tests

Table 1 – Treatment effects on formal employment: LPM with adjustment for few clusters (Cameron et al 2008)

	(1)	(2)	(3)
θ	0.0184** (0.00712)	0.0212* (0.01242)	0.0135 (0.0143)
R-squared	0.065	0.153	0.153
Department dummies	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Individual covariates	No	Yes	Yes
Department covariates	No	No	Yes
N	2,086,677	2,086,677	2,086,677

Note: Bootstrapped standard errors (1000 replications) corrected with the procedure proposed by Cameron et al (2008) for few clusters are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. * p<0.1, ** p<0.05, *** p<0.01.

Table 2 – Treatment effects on formal employment: Specification without Heckman correction for sample selection

	LPM			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
θ	0.00716 (0.00490)	0.00782 (0.00577)	0.00977 (0.00655)	0.0161** (0.00628)	0.0149*** (0.00486)	0.0150** (0.00702)
R-squared	0.030	0.030	0.313			
First stage (F)				56.6405	56.1354	56.2375
Test of exogeneity (p)				0.0199	0.0368	0.2397
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	2,412,226	2,412,226	2,412,226	2,412,226	2,412,226	2,412,226

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

Table 3– Treatment effects on formal employment: Specification with standard errors clustered at the department and year level

	LPM			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
θ	0.0184** * (0.00403)	0.0212** * (0.00427)	0.0135** * (0.00384)	0.0303** * (0.00481)	0.0306** * (0.00508)	0.0211** * (0.00431)
R-squared	0.061	0.062	0.336			
First stage (F)				327.196	329.231	329.684
Test of exogeneity (p)				0.0003	0.0027	0.0053
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167

Note: Standard errors clustered at the department and year level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

Table 4 – Treatment effects on formal employment: Specification without the departments of Cundinamarca and Bogota

	LPM			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
θ	0.0182** (0.00730)	0.0210*** (0.00736)	0.0147* (0.00801)	0.0315*** (0.00640)	0.0317*** (0.00643)	0.0236*** (0.00724)
R-squared	0.058	0.058	0.337			
First stage (F)				54.4145	54.7409	54.7878
Test of exogeneity (p)				0.0071	0.0103	0.0737
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	2,100,676	2,100,676	2,100,676	2,100,676	2,100,676	2,100,676

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: First stage results

	(1)	(2)	(3)
Total theoretical change in the number of inspectors	0.6144176*** (0.081)	0.6153764*** (0.0820)	0.6154386*** (0.082)
Rate of formal employment in 2010		-1.77836*** (0.660483)	-1.778758*** (0.662337)
Rate of inspection in 2010		-0.136158 (0.131859)	-0.1360993 (0.13199)
Rate of employment in priority sectors in 2010		-4.3543*** (1.4497)	-4.358291*** (1.4449)
Working in small business			-0.0030116 (0.0037)
Living in rural areas			0.0022175 (0.00814)
Working in agriculture sector			0.0061358 (0.0082)
Year dummies	Yes	Yes	Yes
Department covariates	No	Yes	Yes
Individual covariates	No	No	Yes
N	2,251,167	2,251,167	2,251,167

Note: Standard errors clustered at the department level are in parenthesis. The table reports the first stage results for the IV specification with the simple post-policy dummy (Table 3 in the main text, adding different sets of controls), with the dependent variable being the endogenous regressor in the analysis (i.e. total actual change in the number of inspectors). All covariates reported in the table are interacted with a post-policy dummy taking the value of 1 in the years after the policy change. * p<0.1, ** p<0.05, *** p<0.01.

Table 6 – Treatment effects on formal employment with all constitutive terms

	LPM			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
θ	0.0184** (0.00672)	0.0212*** (0.00735)	0.00852** (0.00375)	0.0303*** (0.00620)	0.0306*** (0.00637)	0.0157*** (0.00323)
R-squared	0.061	0.062	0.501			
First stage (F)				56.4659	56.1937	56.4417
Test of exogeneity (p)				0.0087	0.0225	0.0004
Department dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Department covariates	No	Yes	Yes	No	Yes	Yes
Individual covariates	No	No	Yes	No	No	Yes
N	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167	2,251,167

Note: Standard errors clustered at the department level are in parenthesis. The coefficients reported are those of an interaction between the treatment indicator (i.e. change in the number of inspectors per 100,000 employed in a given department between 2010 and 2014) and the dummy taking the value of 1 after the policy change. Department characteristics include the (normalised) number of inspectors, the rate of formal employment and the rate of employment in Action Plan priority sectors (all computed in 2010). Individual covariates include dummies for rural areas, agriculture sector and small business. The IV analysis instruments the change in inspectors per thousands of employed individuals with its planned change. * p<0.1, ** p<0.05, *** p<0.01.