# The Impact of Lowering the Payroll Tax on Informality in Colombia

(Draft version)

Cristina Fernandez
Leonardo Villar[1]

*The Colombian government recently reformed the tax law by reducing payroll contributions from 29.5% to 16% and substituting them with a profit tax. The law was passed in December 2012, and two years later the informality rate in the 13 main metropolitan areas diminished from 56% to 51% in December 2014 (using the legal definition of informality). In the whole survey the reduction was a little less pronounced, going from 68% to 64%. This period was also characterized by high, yet also diminishing growth rates; changes in the tax rates, and increasing real minimum wages. It is of the most interest to know how much of this reduction was due to the tax reform. This paper performs this task using a Matching and Difference in Differences methodology. According to the results, the tax reform reduced the informality rate, of the workers affected by the reform in the 13 main metropolitan areas, between 4,3 and 6,8 p.p. which translated in a reduction of the informality rate between 2,0 and 3,1 p.p. given that the treated population was only 45% of the working population of the country in 2012. The impact over the whole survey was between 4,1 and 6,7 which translates into 1,2 to 2,2 p.p. impact on the informality rate of the whole country. Similar results were found using the firm definition of informality. The reform affected mostly salaried workers and employers, males between 25 and 50 years old and workers with low levels of education.*

## I. *Introduction*

The Colombian government recently reformed the tax law by reducing payroll contributions from 29.5% to16% and substituting them with a profit tax. The law was passed in December 2012, and three years later the informality rate had diminished from 63% to 60% in December 2015 [2]. In the 13 main metropolitan areas the reduction was a little more pronounced, going from 51% to 47%. This period was also characterized by high, yet also diminishing growth rates; changes in the tax rates, and increasing real minimum wages. It is of the most interest to know how much of the reduction in the informality rate was due to the tax reform. This paper performs this task using a Matching and Difference in Differences methodology. According to the results, the tax reform

[1] Researcher and Director of Fedesarrollo, respectively. We want to thank Juan Camilo Medellin and Francisco Fernandez for excellent research assistance. We also want to thank the technical support from Anil Sinwal and the comments we received at the workshops organized by Practical Action (PAC) and the Institute of Development Studies (IDS) of the University of Sussex, as well as the comments by Mariano Bosh and Juan Villa, Marcela Eslava, Carlos Medina and by the CAF macro-economic analysis and research groups. A previous and less elaborated version of this article is included as a section in Fernandez and Villar (2016): *Informality and Inclusive Growth in Latin America: The Case of Colombia.* Comments are welcome at cfernandez@fedesarrollo.org.co

[2] Figures based on the Great integrated household survey (Gran Encuesta Integrada de Hogares, GEIH) of the National Department of Statistics, DANE.

reduced the informality rate in the 13 main metropolitan areas of the workers affected by the reform in between 4.3 and 6.8 percentage points (p.p.), which translated in a reduction of the informality rate of the country of between 2.0 and 3.1 p.p., given that the treated population in 2012 was only 45% of the working population in the country. In the total country the impact of the reform on the informality rate of the workers affected by the reform was between 4.1 and 6.7 p.p. that is equivalent to a reduction in the informality rate between 1.4 and 2.2 p.p. Similar results were found using the legal measurement of informality. The reduction in informality rates was stronger for males than for females; for workers in intermediate ages (25 to 50 years old) than for younger or older workers; and for workers with low levels of education.

The paper is structured as follows: Section 2 presents a literature review; Section 3 presents a short analysis of informality in Colombia; Section 4 presents the exercise of differences and differences and Section 5 presents the distributional impact of informality. Section 6 concludes.

## *II. Literature review*

The impact of lowering the payroll tax on informality, with few exceptions, has been studied mainly for the case of emerging countries. The methods to estimate this impact includes General Equilibrium Models, cross country analyses, time series analyses and more recently, some authors have implemented a difference in difference estimations, as the one implemented in this paper.

Using theoretical models, Ulyssea and Reis (2006) found that the reduction of 12% on payroll taxes in Brazil would reduce informality by 5.5%. Similarly, Albrecht et al. (2009) found that payroll taxes increase informality, particularly if firms are small and able to evade controls. The recent tax reform in Colombia has been estimated to increase formal employment by between 3.4 and 3.7%, and to decrease informal employment by between 2.9 and 3.4% (Anton, 2014).

With a cross-country methodology, Hazans (2011) found that European countries with higher payroll taxes show higher levels of informality and Lora and Fajardo (2012) found that payroll taxes increase informality if the workers do not perceive the direct benefits of these contributions, as is often the case in the region.

Using time series analysis, Kugler and Kugler (2009) surveyed a panel of Colombian firms and found that an increase of 10% in payroll taxes leads to an increase in informal employment of between 4% and 5%. Similarly, Mondragon et al. (2010) found, also for the case of Colombia, that an increase of 10% in payroll contributions was correlated to an increased probability of informality ranging between 5 and 8 percentage points.

The technique of differences in differences has been widely used in the labour market. One of the best known papers is Card and Krueger (1994) which analyses the impact of the increase in the minimum wage in New Jersey over employment in fast food restaurants. On informality, Bergolo and Cruces (2011) also applied a difference in differences technique to analyse the impact of an increase in coverage of health services to dependent children of private sector salaried workers over informality rates. The mix of differences and differences and matching techniques has been less used in the literature. One notable exception is the evaluation of training programs as in

Blundell, Costa-Dias, Meghir and Van Reenen (2003) and Bergemann, Fitzenberger and Speckesse (2004. Another paper that should be mentioned is Encina (2013), who analysed the impact of the pension reform on the labour participation outcomes in Chile.

But probably the papers that resemble the most our estimations are Slonimczyk (2011) and Betcherman and Pages (2009). Slonimczyk (2011), using a difference in differences approach, found that a 17% reduction in payroll taxes in Russia in 2001 reduced the informality rate between 2.5% and 4%. Betcherman and Pages (2009), using a synthetic panel, found that a one percentage point decrease (increase) in the labor cost ratio (formal to informal) results in a 2.2 percentage point rise (fall) in the fraction of jobs that are registered.

Recently, the IADB commanded a series of studies (Steiner and Forero (2016), Kugler and Kugler, 2016; and Bernal, Eslava and Melendez, 2016) that used most of the reviewed techniques to estimate the impact of the Colombia 2012 tax reform. They find that the reform increased the absolute number of formal jobs by between 200.000 and 800.000 employments (an increase of the number of formal jobs between 3.1% and 3.4% with respect to December 2012), and also increased the wages from 1.9% to 4.4%. According to the authors, most of the impact of the reform took place among small (less than 10 workers) and medium size enterprises (10 to 50 workers).

## *III. Informality, payroll taxes and other variables affecting this relationship in the case of Colombia*

This section explores the composition and recent behaviour of informality according to different alternative metrics. It also reviews the relationship between informality and payroll taxes in Colombia, and describes the behaviour of other variables that may have mediated such relationship, such as growth and the minimum wage. The next section tries to isolate the impact of payroll taxes on informality from this set of mediating variables.

*Characteristics of the recent decrease of Informality in Colombia*

Figure 1 shows four graphs that illustrate the composition and recent behaviour of informality in Colombia. The data set used in these graphs, as well as in the majority of the ensuing analysis, is from the *Gran Encuesta Integrada de Hogares* (GEIH). When needed, we also used the *Encuesta Continua de Hogares (Continuous Household Survey*, ECH, 2002-2006) Both surveys are provided by the Colombian National Department of Statistics (Departamento Nacional de Estadística de Colombia, DANE).

Figure 1a shows the behaviour of the informality rate across different aggregates and different metrics of informality. According to this figure, around 60% to 64% of the working population in the country and 48% to 50% in the main 13 metropolitan areas is engaged in the informal sector, depending on the definition of informality used. In this figure we can also appreciate that since 2010 informality has diminished in around 5 to 3 percentage points, depending on the aggregate and the metric used.

In most of the exercises that follow, we use both the whole survey and the 13 main metropolitan areas aggregates across the analysis, but with an emphasis on the 13 main metropolitan areas, which is more representative and also more commonly used by the Colombian authorities[3]. Similarly, throughout this analysis we mostly applied the legal definition of informality in which informal workers include those who do not make contributions to either health or pension schemes. However, we checked robustness of the exercises by also applying the so called "firm definition" of informality, in which informal workers include those employed in firms with no more than five employees; unpaid family helpers or housekeepers; self-employed with the exception of independent professionals and technicians; and business owners of firms with no more than five workers.[4]

Figure 1b shows the behaviour of the total number of formal and informal employment in the 13 main metropolitan areas. According to this graph the reduction of the informality rate in the last years was related to an increase in formal jobs rather than a substitution between informal and formal jobs. Between 2012 and 2014, about 871 thousand formal jobs were created, of which 90% were salaried. On the other hand, 134 thousand new self-employers[5] entered the occupied population and 33 thousand salaried informal jobs disappeared.

Figure 1c shows that the drop in informality rates was also more pronounced among the salaried workers and employers than among self-employment in the 13 main metropolitan areas. These two groups also show wide differences in the informality rates: whereas, the informality rate among the salaried workers and employers is about 32%, it amounts to about 83% among the self-employed. The self-employment in Colombia accounts for about 59% of the informal workers.

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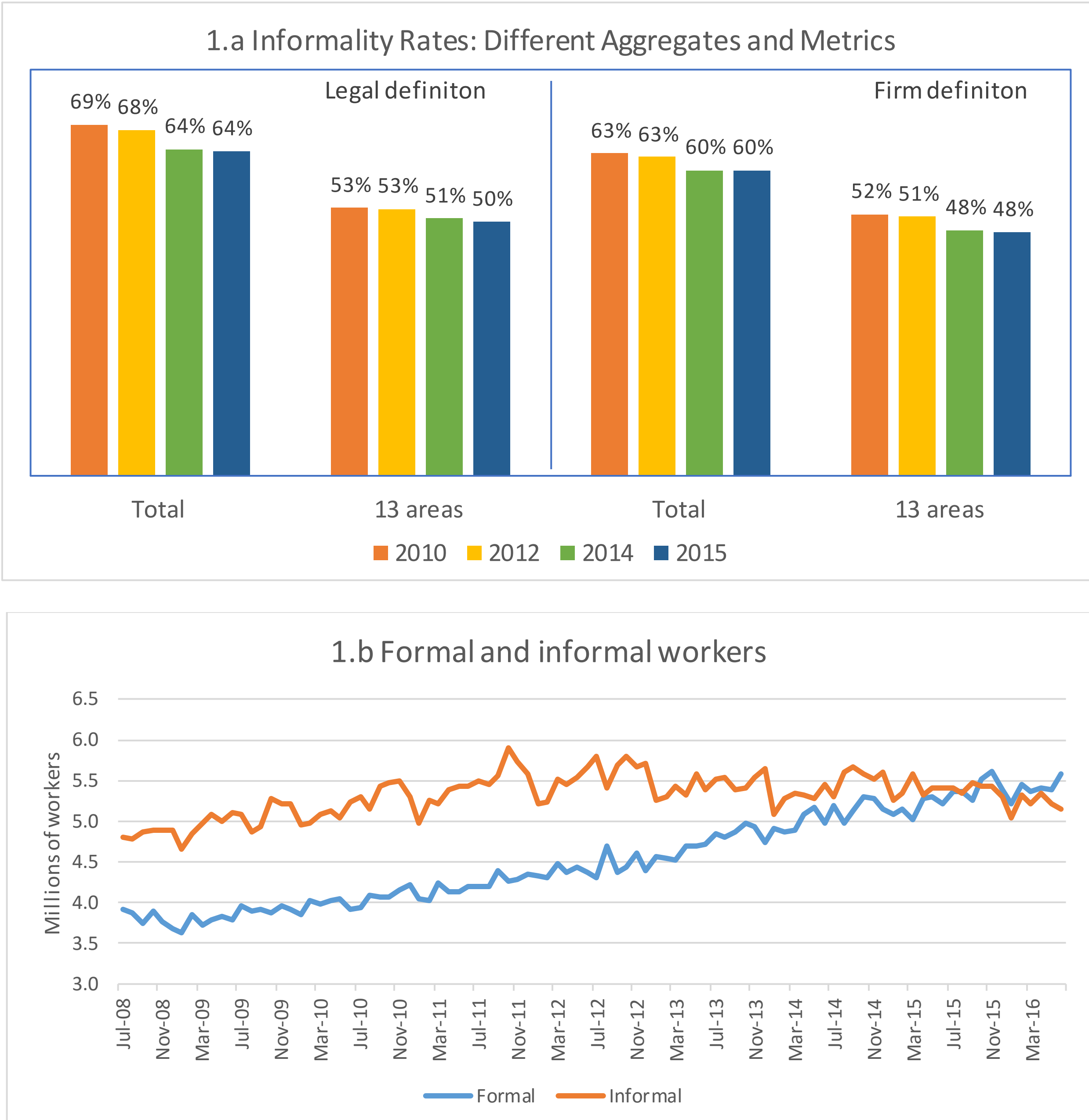
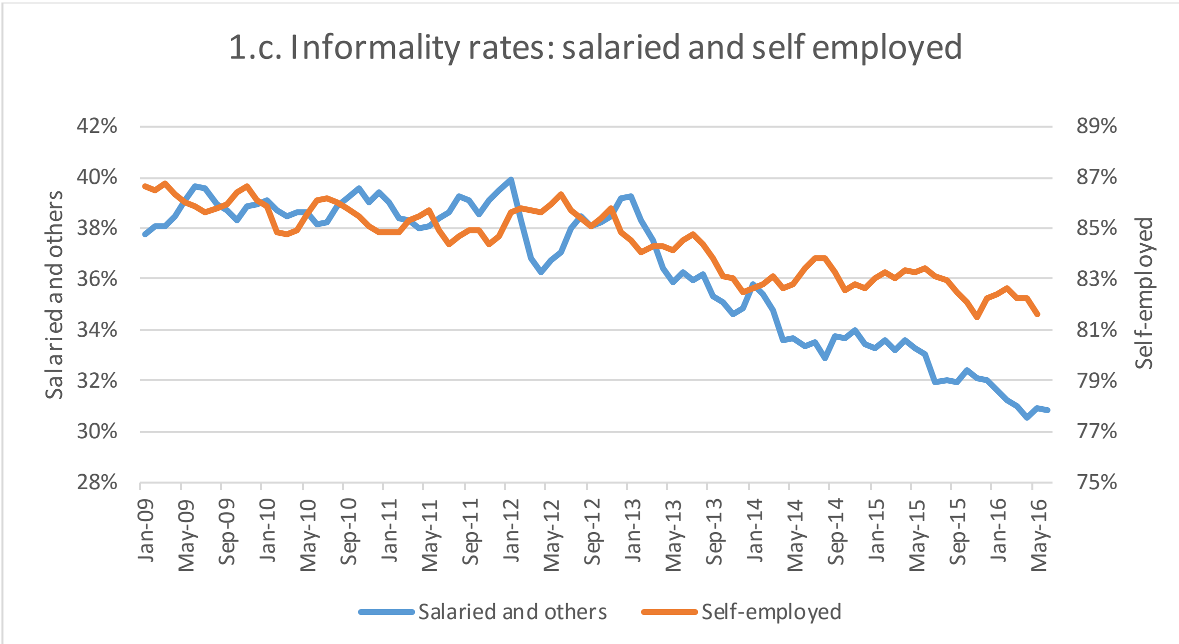
[3] The GEIH total aggregate covers 23 cities with their rural areas, gathering information on more than 62 thousand households per quarter, of which more than 30 thousand households are in the 13 metropolitan areas aggregate. These areas represent 60% of the total urban population according to the 2005 census.

[4] This criterion changed from 10 workers or less (ILO10) to 5 workers or less (ILO5). The ILO5 shows a higher correlation with other measures of informality (Bernal, 2009). Since 1999 the Delhi Group established the ILO5 as the standard measurement of informality (Central Statistical Organization, 1999).

[5] Defining self-employment as workers in unipersonal firms, and workers and employers as workers in firms with more than one worker.

Figure 1. Informality rates. Recent behaviour





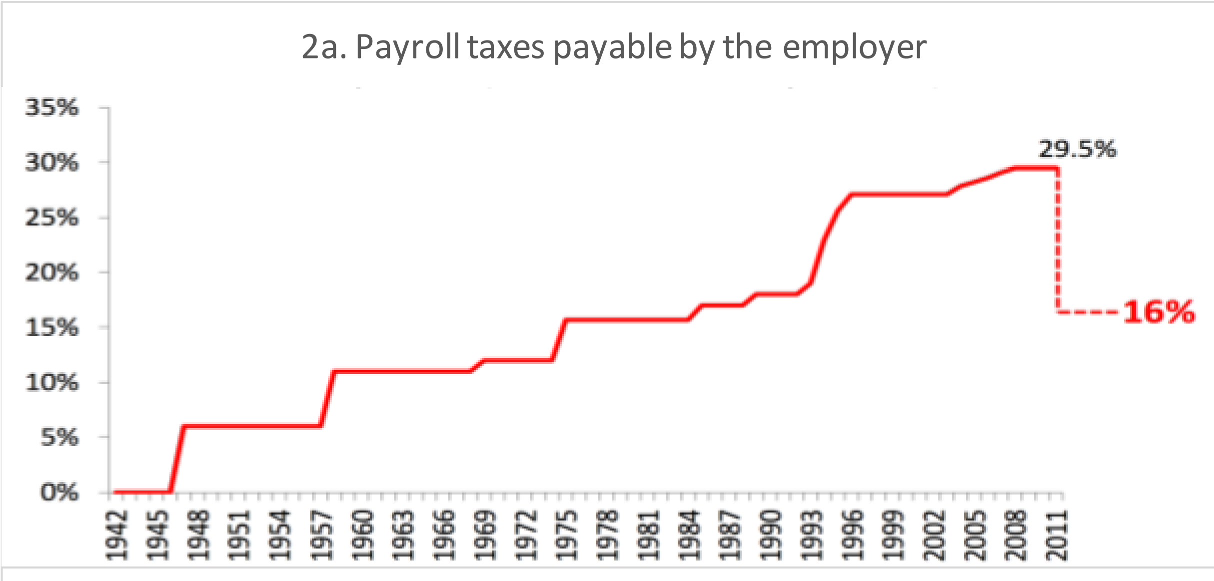

Source: GEIH. 13 Main Metropolitan areas and legal definition of informality if not specified otherwise.

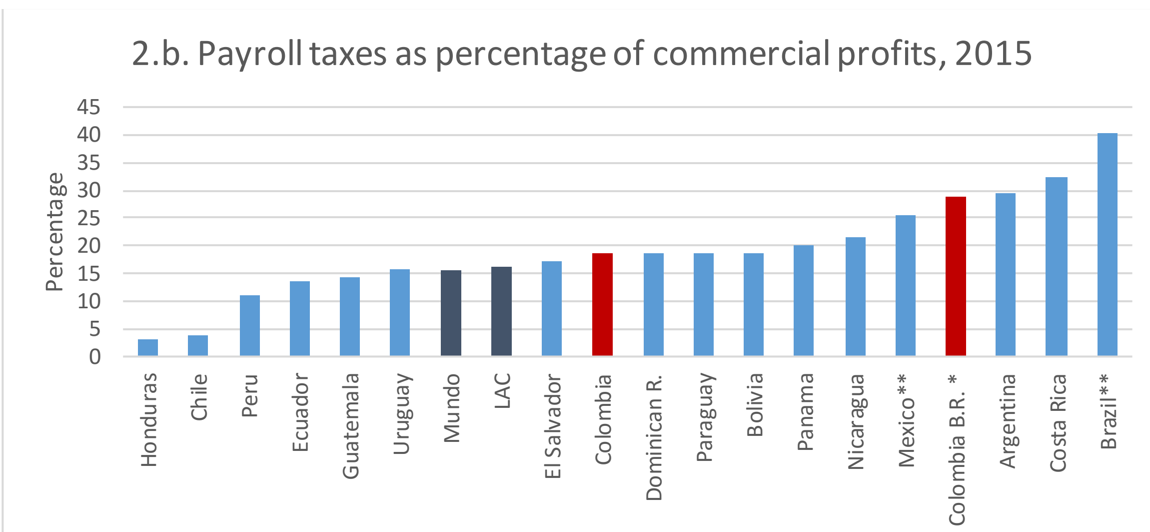
### *The payroll taxes in Colombia and the recent tax reform*

As shown in Figure 2a, payroll taxes raised significantly during the nineties, particularly after the pension and health reform known as Law 100 of 1993. Maloney (2004) and Perry (2007) argue that this increase is associated to the rapid increase of informality during the nineties. In 2012, the Colombian government reformed the tax law (Ley 1607, 2012) by reducing payroll contributions from 29.5% to 16% over the wage (Figure 2a). The reform only affects the payments made by the employers/firms of two or more workers that earn wages between one and ten times the minimum wage, and does not change the amount of taxes or contributions payable by the workers. NGO's, government and self-employment were also excluded from the reform.

From the fiscal point of view, the source of the contributions was substituted with a profit tax (CREE[6]) under the hypothesis that it is preferable to tax capital than to tax labour. Despite of this reduction in the payroll taxes, Colombia continues to show a relatively high payroll tax rates in the Latin American context, as shown in Figure 2b.

[6] *Impuesto sobre la renta para la equidad.*




Sources: Figure 2.a Santa Maria, Garcia and Mujica (2008), Figure 2.b World Bank and Figure 2c Own calculations. * Colombia before the reform ** two main metropolitan areas.

*Other variables impacting informality.*

As we saw in the literature review, there is a close relationship between informality and payroll taxes that has been addressed through several methodologies. However, this relationship is often mediated by other variables. In the case of Colombia, we identified five main circumstances that might have also affected the informality results after the tax reform.

1. **A general change in taxes and in particular the creation of a profit tax (CREE) to substitute tax contributions as a fiscal source.** Some argue that the impact of the reform was offset by the creation of the new tax. However, two arguments run against this claim: First, the substitution was not perfect in the sense that the proceeds obtained by

the government lowered in about 0.2% and 0.5% of the GDP, with respect to what was received in the past by the waived payroll taxes; and second, there is a substitution effect caused by taxing capital intensive firms instead of the labour intensive firms. In this sense the creation of the CREE should be viewed as an independent increase in general taxes. According to Forero and Steiner (2016), the impact would have been marginally higher (around 0.2 pp) using the VAT as an alternative income source rather than the CREE.

2. **The post-reform period also coincided with high but diminishing economic growth rates.** Figure 3a shows that there is a positive relationship between the formality rate[7] and the economic cycle, measured as the relative difference between observed and potential GDP. The correlation coefficient between the formality rate and the output gap is 0.56 for the legal definition of formality until 2013. These results support the hypothesis pertaining to the counter-cyclicality of informal employment in Colombia. The positive relationship between the economic cycle and the formality rate does not necessarily imply causality between economic growth and formality. It can be argued that high rates of economic growth can be a consequence of low informality. In order to isolate this type of double causation, we plotted the relationship between the formality rate and the value of commodity exports as a percentage of GDP trend in Figure 3b. Commodity exports represent a good proxy for the economic cycle since they are exogenous to informality and well correlated with the output gap[8]. The correlation coefficient between formality and commodity exports is 0.73. Therefore, we can claim that the formality rate in Colombia is in general pro-cyclical and the informality rate is in general counter-cyclical, which is consistent with having a significant portion of involuntary informal workers amongst whom informality can increase inclusive growth. It also supports the idea of informality being a buffer to unemployment during crisis.

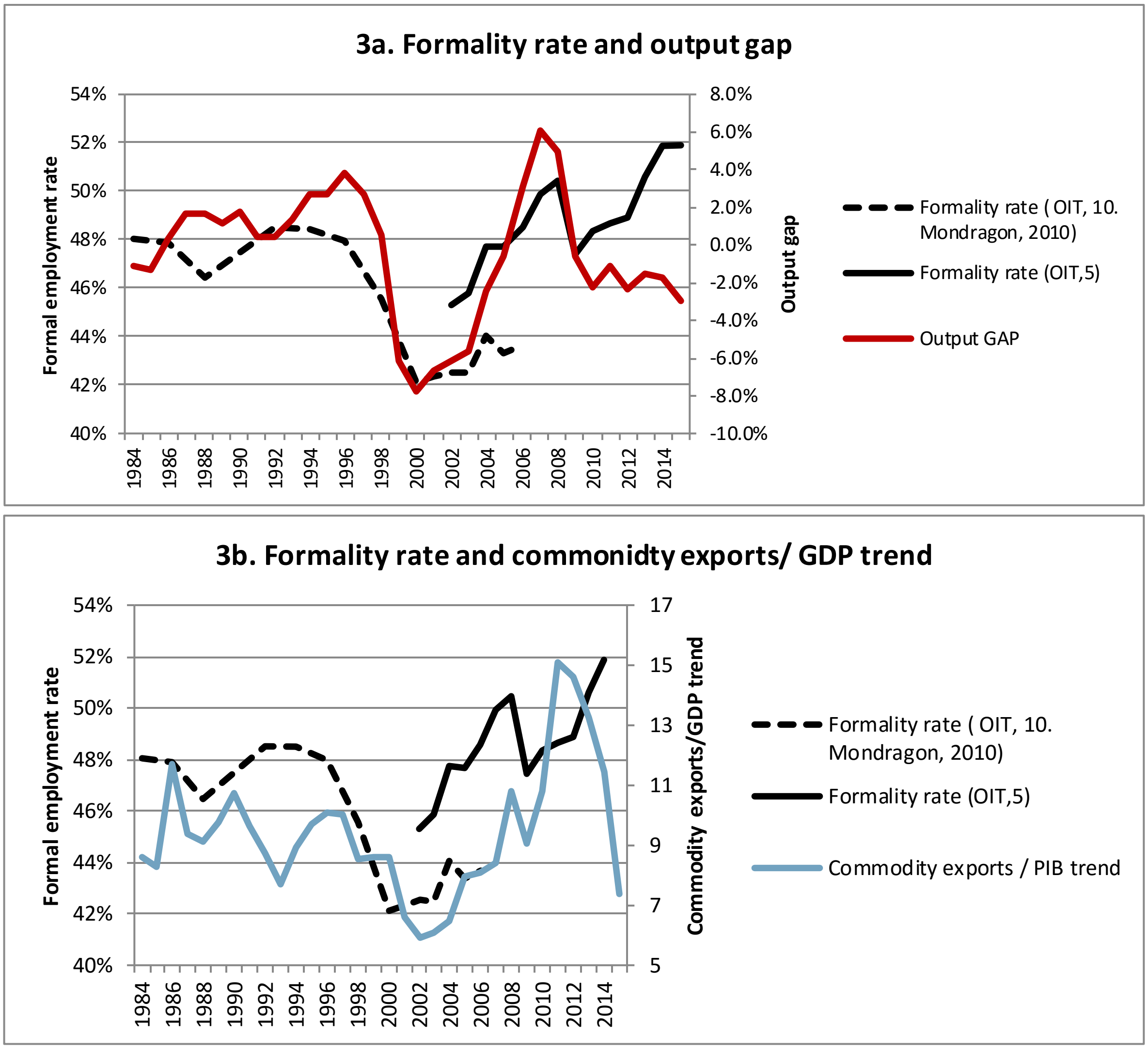
   However, as shown in Figure 3 and Table 1, the most recent years show an important increase in formality rates that cannot be explained by the economic cycle. In fact, the correlation coefficient drop to 0.23 and is not significant if we include the years 2014 and 2015. As we will later demonstrate, this might be related with the 2012 payroll tax reform. In other words, a relaxation of formal market rigidities might have resulted in more pro-cyclical behaviour of the informal sector.

Table 1. Correlation between output gap and formality.

| | Legal definition (2002-2015) | Legal definition (2002-2013) | Firm definition (2002-2015) | Firm definition (2002-2013) | Mondragón et al. Firm definition (1984-2006) |
|---|---|---|---|---|---|
| Output gap | 0.23 | 0.56* | 0.46* | 0.74** | 0.74*** |
| Commodities/ GDP trend | 0.42 | 0.73*** | 0.46* | 0.63** | 0.73*** |

[7]Defined as one minus the informality rate. Note that the formality rate is calculated for two different ILO methodologies/series since one includes firms with less than 10 workers (ILO10, 2010) and the other includes firms with less than 5 workers (ILO5). The legal definition cannot be estimated before 2002.
[8]See Fernandez, Villar & Sánchez,2015

Figure 3. Formal employment rate and the output gap/commodity exports over the GDP trend




Source: Table 1: Own calculations using data from Fedesarrollo and the World Bank. The graphs use the firm definition of informality since the legal definition can only be estimated from 2002.

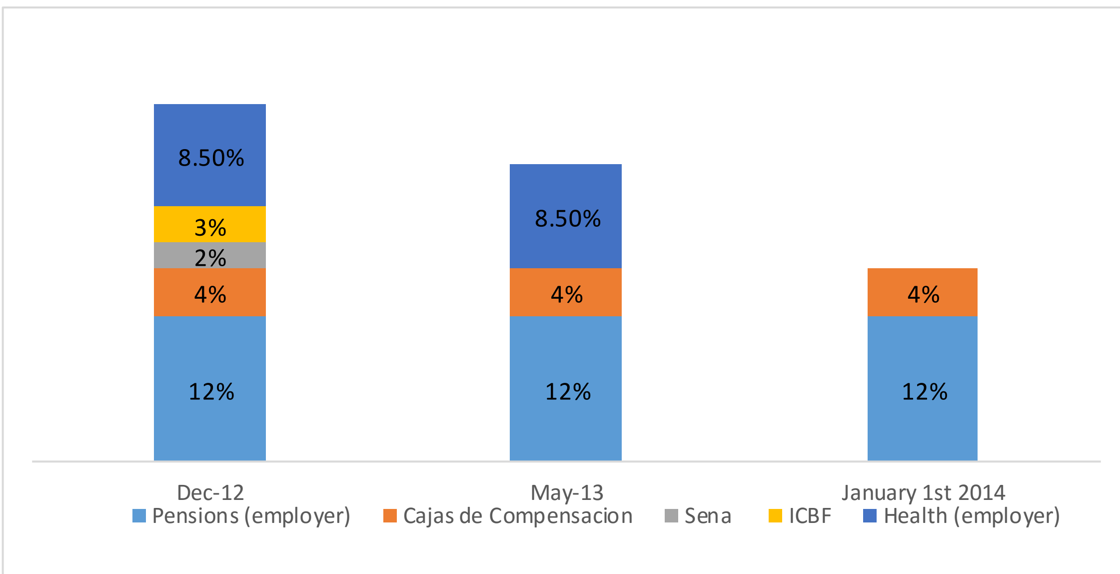
3. **A simultaneous increase in the real minimum wage.** During 2012 and 2014, the minimum wage corrected by productivity increased annually by 1.8% (in comparison with 1.1% between 2007 and 2011). This increase in the minimum wage should have induced an increase in informality. The impact of the minimum wage is rather difficult to isolate from the reform, but according to Forero and Steiner (2016) the impact of the reform would have been one percentage point larger if the minimum wage hadn't increased.

4. **Changes in government employment.** The government employment was excluded from the reform and it might have change the behaviour of the informality rate since by the firm definition all government jobs are formal jobs. However, between 2012 and 2014 the

participation of government employment over total employment diminished from 3.9% to 3.7%. This change is too small to have altered the informality results in a significant way.

5. **Anticipation of the reform.** As shown in Figure 4, the implementation of the Law involved several milestones. Most of the discussions were held between October and November 2012, the Law was approved in December 2012, the waiver over SENA[9] and ICBF[10] contributions became effective in May 2013 and the reform was fully implemented on January 1st 2014, when the waiver over the health contributions became effective (8.5% of wages). Although the reform was approved on December 2012, it was widely discussed during the second semester of 2012, and the firms might have anticipated this policy reducing the impact after 2012. In the next section, we claimed that the reform was not really anticipated, but some of the effect took place in 2013 when it was not yet fully implemented.

Figure 4. Payroll contributions (employer)




Source: own calculations

The next section implements a methodology to isolate the impact of the reform from growth, and other macroeconomic and regulatory conditions, by distinguishing the workers affected by the reform and the workers affected by a widely set of circumstances. In the case of the increase of the minimum wage, the impact is more difficult to be isolated, since it mostly affected the workers targeted by the reform.

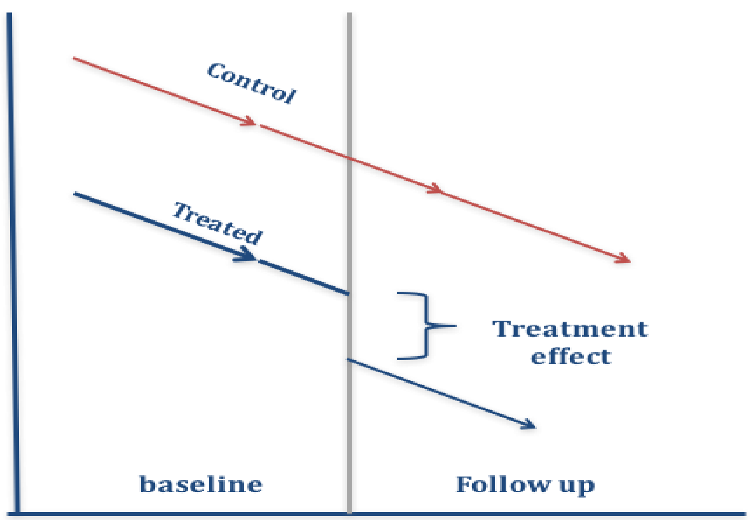
[9] *National Learning Service (Sevicio nacional de Aprendizaje).*
[10] *National Institute of Family Welfare (Instituto Nacional de Bienestar Familiar).*

## IV. Applying a matching difference in differences approach[11]

*Applying a differences in difference approach*

One of the most adequate methodologies for evaluating the impact of the tax reform on informality, while isolating the impact of growth and other macroeconomic variables over time is the Differences in Differences (DID) method, as in Card and Krueger (2006). The method involves dividing the population into two groups: one affected by the reform, the *treated* group, and the other unaffected by the reform, the *control* group. The change in probability of informality within the control group is then compared with the change observed in the probability of informality within the treatment group. By taking the difference between these changes –or the difference in differences- one isolates factors that affect both groups simultaneously, such as macroeconomic conditions, assuming that the impact on informality is evenly spread between both groups. This procedure is summarized in graph one. As Todd (1999) claims, the advantage of this methodology compared to a cross-section analysis is that it allows for time-invariant unobservable differences between the treatment and the control groups.

Diagram 1. Difference in Differences approach




As a first step, we performed this exercise for the case of Colombia, using the following equation and Ordinary Least Squares (OLS).

$$INF_{it} = \beta_0 + \beta_1 Year_t + \beta_2 Treated_i + \beta_3 (Treated_i \times Year_t) + \beta_4 X_{it} + u_{it}$$

Where INF is a binary variable that takes value 1 if person i at time t is an informal worker and zero if he or she is a formal worker; $X_{it}$ refers to the observable characteristics of each individual

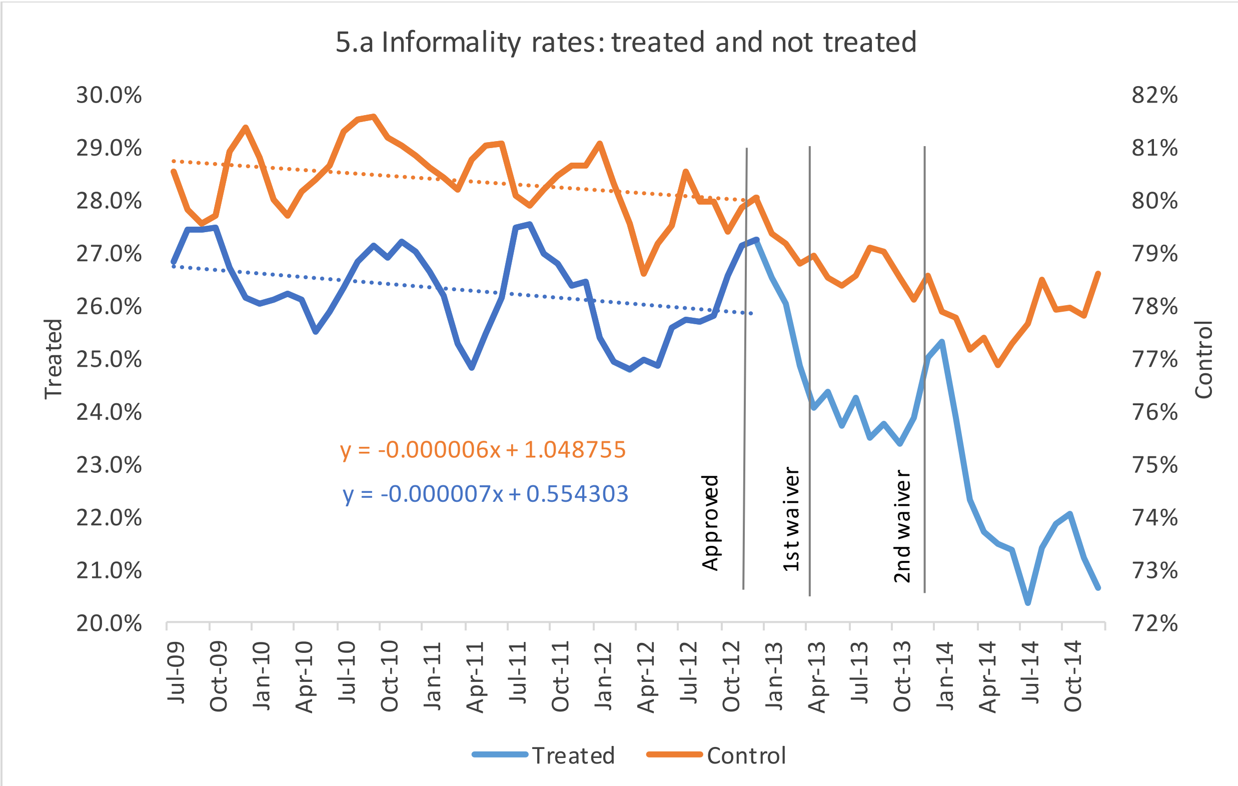
[11] This section is based on Fernandezet al (2016b).

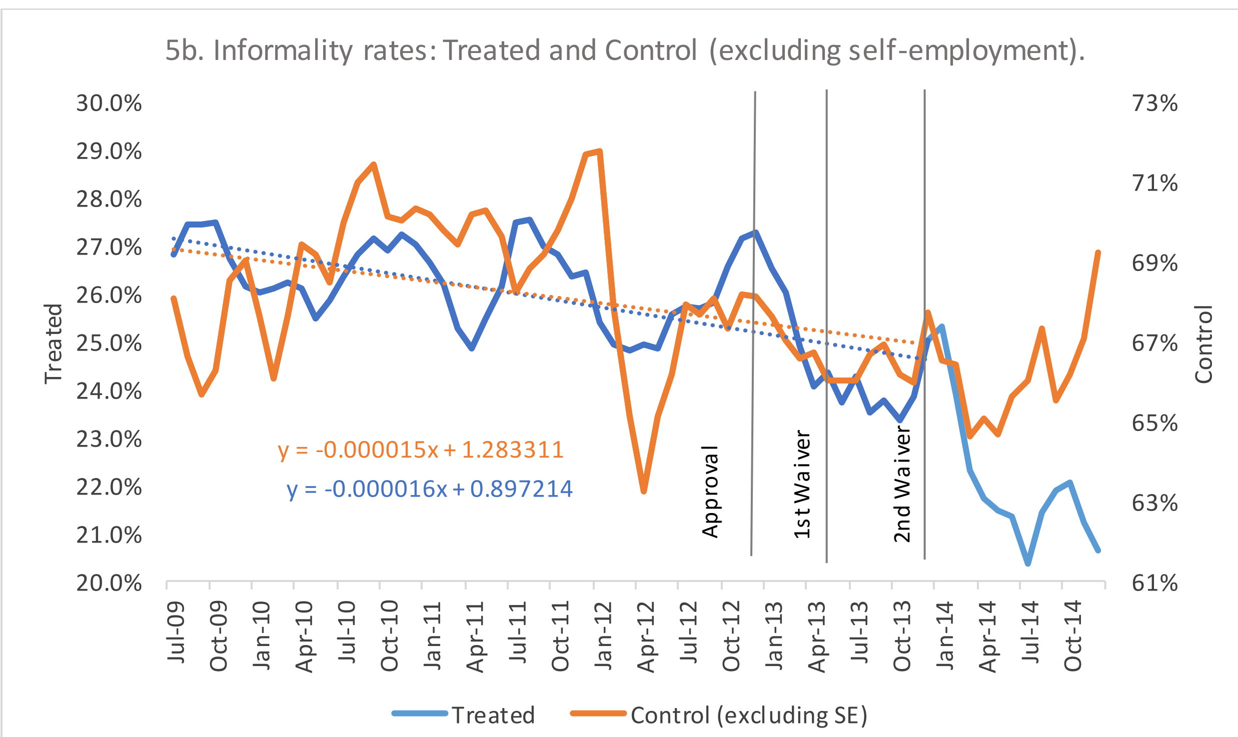
i at time t, Year is a dummy variable that takes the value of zero in the baseline period and a value of one in the period after the reform, and Treated is a dummy that takes the value of one if the individual is from the treatment group and zero if not. $\beta_0$ is the mean outcome of the control group at the baseline; $(\beta_0 + \beta_2)$ the mean outcome of the treated group at the baseline; $\beta_2$ the difference between the treated group and the control group; $(\beta_0 + \beta_1)$ the mean outcome of the control group at the follow up; $(\beta_0 + \beta_1 + \beta_2 + \beta_3)$ the mean outcome of the treated group at the follow up; $(\beta_2 + \beta_3)$ the difference at the follow up; $\beta_3$ the difference in difference estimator;

The treatment group in our exercise includes all workers that were directly impacted by the reduction in payroll taxes. According to the law, this includes workers that earn between one and 10 times the minimum wage excluding government and NGO workers, self-employment and workers in unipersonal firms. We also included those workers who reported an income close enough to the minimum wage or to ten times the minimum wage[12]. The control group includes all other workers. We excluded from the exercise the government workers and those that did not reported a salary. We performed the exercise including and excluding self-employment in the control group.

Figure 5a plot the informality rate for the treatment and control groups for the main 13 metropolitan areas, before controlling by observable characteristics. As can be observed, the reform does not show much anticipation before its approval, but started to have an impact even before being implemented in May 2013. Nevertheless, most of the impact took place after being totally implemented in January 2014. After this period, the model confirms that workers affected by the reform, or the treatment group, were less likely to engage in the informal sector, while this was not the case for workers in the control group. The figures also indicate that relatively long-term moving averages should be considered in this type of analysis since the series demonstrates considerable volatility. Therefore, we defined our period of analysis from 2012, before the implementation of the reform, to 2014, after the implementation of the reform. Figure 5b plots the treated group and the control group, but excludes self-employment from the later, because self-employment tends to show a different behaviour form the salaried workers, and might have being impacted by other measures adopted by the government, as the recent labour monitoring and control policies implemented over independent workers. Therefore, from here on, we tried to perform the exercises including and excluding self-employment from the control group.

[12] In fact, we realised that a number of formal workers who probably earn the minimum wage rounded this figure up to the next ten thousand Colombian Pesos, and therefore we included them in the treatment group.




Source: GEIH. 1st waiver: Reduction in Sena and ICBF contributions. 2nd waiver: Reduction in Health contributions

The results of the OLS (non-weighted) regression are shown on Table 2 The control variables were chosen according to Fernandez and Villar (2016b). Their impact on informality is the following:

- **Gender:** Women are more likely to be informal than men. We separated the impact of women registered as spouses from women that are heads of the household, daughters, grandmothers etc., since both groups have different preferences for formality. We found that informality rates are higher for both groups, disrespectfully of their preferences.
- **Age:** We included in the regression dummy variables for workers younger than 25 and older than 50, leaving workers between 24 and 45 years old as the base group. The younger group tends to show higher rates of informality, probably related to lack of experience, since they show preferences for formality in the survey. The second group also shows higher rates of informality but they seem to be more related to preferences.
- **Education:** This category has the strongest impact over informality. Workers with primary education or less are much more likely to be informal than workers with basic or secondary education without diploma, the base group. Similarly, workers with tertiary education are much less likely to be informal than those with lower education. Accordingly, workers with high school or higher education diploma have a lower probability to be informal than those that do not have a diploma.
- **City:** Workers living in big city are less likely to be informal than those living in smaller cities. On the other hand, those who live in border cities are more likely to be informal, probably because informality often comes in hand with smuggling.
- **Rural/urban:** Workers in the main Metropolitan areas tend to show lower informality rates than workers living in small cities, the base group. As expected, workers in rural areas are more likely to be informal.
- **Weights:** As suggested by Dugoff et al (2014), it is a good idea to include weights as a control variable to the difference in difference estimation since it can account for some variables that may capture relevant factors, such as where individuals live, their demographic characteristics and perhaps their availability to respond to surveys, that might intercede in the estimation of informality, but are burdensome to include in the estimations.
- **Months:** January, February and December were included in the regression to control for seasonality.

According to this setup, and the results that are summarized on Table 3, in the 13 main metropolitan areas the control group showed an informality rate of 90.2% before the tax reform of 2012, that increased to 90.8% in 2014, after the reform. The treated group reduced its informality rate from 42.7% to 38.6% meanwhile. After controlling for all observable characteristics, the difference between the control and the treated group in the baseline was -47.5 p.p. and in the follow up 52.2 p.p,, meaning that the difference in difference estimator is -4.7 p.p. The difference in difference estimator excluding self-employment from the control group is -6.3. Considering that the treatment group in 2012 is only 45% of the total occupied group[13], the impact of the reform after the first year of implementation was between 2.1 p.p. and 2.8 p.p. In the whole survey, the impact over the treatment group is around 4.7 p.p. including and 6.0 p.p. excluding self-employment from the controls group, resulting in about 1.6 p.p. to 2.0 p.p. of the informality rate. The impact is a little less when the firm definition of informality is used. The weighted results are presented on Annex 1.

Table 2. OLS estimation of difference on differences (firm definition, non-weighted)

| | Legal definition | | | | Firm definition | | | |
|---|---|---|---|---|---|---|---|---|
| | 13-areas | | Total | | Total survey | | Workers and employers | |
| | Total survey | Workers and employers | Total survey | Workers and employers | Total | Total | 13-areas | 13-areas |
| Constant (B0) | 0.902*** | 0.797*** | 0.924*** | 0.819*** | 0.831*** | 0.640*** | 0.864*** | 0.682*** |
| | [391.7] | [207.0] | [600.5] | [280.6] | [351.7] | [162.9] | [528.3] | [222.5] |
| Year (B1) | 0.006*** | 0.023*** | 0.001 | 0.014*** | 0.018*** | 0.027*** | 0.011*** | 0.017*** |
| | [3.6] | [6.2] | [0.6] | [5.5] | [9.4] | [7.3] | [9.0] | [6.4] |
| Treated (B2) | -0.475*** | -0.353*** | -0.472*** | -0.361*** | -0.433*** | -0.277*** | -0.429*** | -0.279*** |
| | [-226.7] | [-113.2] | [-326.1] | [-159.3] | [-201.0] | [-86.8] | [-278.6] | [-117.5] |
| Effect on treated (treated*year) (B3) | -0.047*** | -0.063*** | -0.047*** | -0.060*** | -0.043*** | -0.057*** | -0.043*** | -0.052*** |
| | [-16.2] | [-14.4] | [-23.7] | [-19.2] | [-14.7] | [-12.6] | [-20.4] | [-15.9] |
| Women (spouse) | 0.043*** | 0.033*** | 0.026*** | 0.023*** | 0.047*** | 0.047*** | 0.035*** | 0.039*** |
| | [21.1] | [10.7] | [18.8] | [9.9] | [22.5] | [14.8] | [23.7] | [15.8] |
| Women (other) | 0.014*** | 0.023*** | 0.012*** | 0.030*** | 0.029*** | 0.056*** | 0.025*** | 0.061*** |
| | [8.9] | [10.0] | [11.4] | [17.1] | [17.2] | [23.9] | [21.6] | [33.4] |
| Less than 25 | 0.123*** | 0.165*** | 0.109*** | 0.159*** | -0.011*** | 0.029*** | 0.002 | 0.036*** |
| | [62.3] | [63.8] | [84.5] | [82.3] | [-5.4] | [11.0] | [1.4] | [17.6] |
| More than 50 | -0.031*** | -0.013*** | -0.028*** | -0.010*** | 0.061*** | 0.083*** | 0.050*** | 0.074*** |
| | [-16.5] | [-4.3] | [-22.7] | [-4.5] | [31.7] | [26.0] | [38.6] | [31.0] |
| Primary (-) | 0.033*** | 0.040*** | 0.038*** | 0.055*** | 0.039*** | 0.056*** | 0.035*** | 0.057*** |
| | [14.3] | [10.9] | [26.1] | [21.3] | [16.5] | [14.9] | [22.9] | [20.9] |
| Tertiary (+) | -0.175*** | -0.138*** | -0.189*** | -0.146*** | -0.221*** | -0.128*** | -0.239*** | -0.138*** |
| | [-95.3] | [-56.3] | [-148.0] | [-77.7] | [-117.1] | [-51.1] | [-176.0] | [-70.0] |
| Diploma | -0.128*** | -0.186*** | -0.109*** | -0.170*** | -0.113*** | -0.145*** | -0.102*** | -0.139*** |
| | [-60.1] | [-59.5] | [-76.1] | [-72.9] | [-51.5] | [-45.3] | [-67.4] | [-56.7] |
| Big city | -0.074*** | -0.078*** | -0.066*** | -0.077*** | -0.044*** | -0.040*** | -0.040*** | -0.037*** |

[13] This percentage is estimated over the population. The number of treated workers among those that do not report their income (not included in the exercise) is estimated as those workers with imputed income between one and ten minimum wages.

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | [-33.2] | [-26.1] | [-40.7] | [-32.2] | [-19.3] | [-13.1] | [-23.6] | [-14.9] |
| Border city | 0.032*** | 0.051*** | 0.034*** | 0.048*** | 0.032*** | 0.032*** | 0.018*** | 0.017*** |
| | [14.2] | [14.4] | [20.9] | [17.1] | [13.8] | [8.8] | [10.4] | [5.6] |
| 13 Metropolitan areas | | | -0.023*** | -0.026*** | | | -0.025*** | -0.035*** |
| | | | [-21.3] | [-14.2] | | | [-21.1] | [-18.8] |
| Rural | | | 0.035*** | 0.074*** | | | 0.037*** | 0.073*** |
| | | | [19.6] | [23.0] | | | [19.3] | [21.5] |
| Weights | 0.000*** | 0.000*** | 0.000*** | 0.000*** | 0.000 | 0.000** | -0.000*** | 0.000 |
| | [7.0] | [5.1] | [4.8] | [8.7] | [1.2] | [2.9] | [-5.5] | [1.4] |
| January | 0.011*** | 0.014*** | 0.018*** | 0.026*** | 0.021*** | 0.023*** | 0.021*** | 0.025*** |
| | [4.0] | [3.8] | [10.2] | [9.2] | [7.7] | [5.9] | [11.3] | [8.5] |
| February | -0.001 | -0.006 | 0.000 | -0.004 | -0.004 | -0.008* | -0.003 | -0.007* |
| | [-0.5] | [-1.7] | [-0.1] | [-1.6] | [-1.6] | [-2.0] | [-1.7] | [-2.3] |
| December | 0.003 | 0.006 | 0.004* | 0.010*** | -0.002 | -0.009* | 0.005** | 0.004 |
| | [1.0] | [1.7] | [2.4] | [3.7] | [-0.7] | [-2.3] | [2.8] | [1.3] |
| N | 289151 | 166544 | 590286 | 295770 | 289151 | 166544 | 590286 | 295770 |
| F | 12198 | 4379 | 24248 | 8387 | 11412 | 2853 | 20994 | 5215 |
| df_m | 16 | 16 | 18 | 18 | 16 | 16 | 18 | 18 |
| df_r | 289134 | 166527 | 590267 | 295751 | 289134 | 166527 | 590267 | 295751 |
| r2 | 0.40 | 0.30 | 0.43 | 0.34 | 0.39 | 0.22 | 0.39 | 0.24 |

Source: GEIH and own calculations

Table 3. Difference in difference exercise extracted from the OLS coefficients

| | | Legal definition | | | | Firm definition | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | 13-areas | | Total | | 13-areas | | Total | |
| | Coef. | Total | Workers and salaried | Total | Workers and salaried | Total | Workers and salaried | Total | Workers and salaried |
| Mean outcome of the control group at the baseline | $B_0$ | 90.2 | 79.7 | 92.4 | 81.9 | 83.1 | 64.0 | 86.4 | 68.2 |
| Mean outcome of the treated group at the baseline | $B_0 + B_2$ | 42.7 | 44.4 | 45.2 | 45.8 | 39.8 | 36.3 | 43.5 | 40.3 |
| Difference at the baseline | $B_2$ | -47.5 | -35.3 | -47.2 | -36.1 | -43.3 | -27.7 | -42.9 | -27.9 |
| Mean outcome of the control group at the follow up | $B_0 + B_1$ | 90.8 | 82.0 | 92.5 | 83.3 | 84.9 | 66.7 | 87.5 | 69.9 |
| Mean outcome of the treated group at the follow up | $B_0 + B_1 + B_2 + B_3$ | 38.6 | 40.4 | 40.6 | 41.2 | 37.3 | 33.3 | 40.3 | 36.8 |
| Difference at the follow up | $B_2 + B_3$ | -52.2 | -41.6 | -51.9 | -42.1 | -47.6 | -33.4 | -47.2 | -33.1 |
| Difference in difference | $B_3$ | -4.7 | -6.3 | -4.7 | -6.0 | -4.3 | -5.7 | -4.3 | -5.2 |
| % of treated on occupied | | 45% | 45% | 33% | 33% | 45% | 45% | 33% | 33% |
| Impact over total informality rate | | -2.1 | -2.8 | -1.6 | -2.0 | -1.9 | -2.6 | -1.4 | -1.7 |

Source: GEIH

Although these results are very plausible the methodology of the exercise has some limitations since it assumes common time effects across groups[14], and no changes to the composition of each group. In order to face these drawbacks, it would be ideal to work with panel data (Blundell & Costa Dias, 2009). Unfortunately, we do not have this type of data in Colombia, so we proceed to simulate a panel structure by using a matching approach to partially reduce these limitations.

*Applying a differences in differences and matching approach*

The 'Matching Differences in Differences' (MDID) was initially developed by Heckman et al. (1997). As in the DID approach, the idea is to match the treated individuals after the reform with treated individuals before the reform and the control individuals after and before the reform, and then compare the differences in informality rates between the treated and control groups over time. According to the procedure, for each individual in the treated group, a counter-factual is found in the control group, so the difference among the two groups between before and after the reform provide information about the impact of the reform, isolating other effects that may have affected both the treated and control groups. In contrast with the traditional DID approach, however, the MDID method does not take single individuals, but averages of individuals weighted by their probability of being treated. A detailed description of the procedure is available in Annex 1.

Table 4 shows the results of applying the Matching and Difference in Difference methods between 2012, before the reform, and 2014, after the reform. According to the results, informality rates of the 13 main metropolitan areas among the treatment group fell by 4.3 percentage points in the analysis period due to a shock impacting the control but not the treatment group, which we interpret as the effect of the reduction in payroll taxes. This means an impact over the total informality rate of -2.0 p.p., considering that the participation of the control group in the population is 45%. This impact is lower in the whole survey (-1.4 p.p.). The higher impact obtained when self-employment is excluded from the control group (-3.1 in the 13 metropolitan areas and -2.2 in the whole survey) might be related to the positive results of the monitoring and control policies applied over self-employment. These outcomes are comparable to the OLS non weighted estimation of Table 3 to what previous estimations predicted, but rather in the lower range.

Table 4. DID matching results (baseline=2012, follow up=2014)

| | Firm definition | | | | Legal definition | | | |
|---|---|---|---|---|---|---|---|---|
| | 13 areas | | Total survey | | 13 areas | | Total survey | |
| | Total | Salaried and employers | Total | Salaried and employers | Total | Salaried and employers | Total | Salaried and employers |
| Mean outcome of the control group at the baseline | 76% | 60% | 79% | 65% | 66% | 47% | 69% | 51% |
| Mean outcome of the treated group at the baseline | 28% | 28% | 31% | 31% | 24% | 24% | 27% | 27% |

[14] In fact, the model can control for non-observable individual specific effects and non-observable macroeconomic effects because they cancel one another out, but not for non-observable temporary individual specific effects.

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Difference at the baseline | -48%*** | -33%*** | -48%*** | -34%*** | -42%*** | -23%*** | -42% | -23% |
| Mean outcome of the control group at the follow up | 75% | 62% | 77% | 66% | 67% | 49% | 69% | 52% |
| Mean outcome of the treated group at the follow up | 23% | 23% | 25% | 25% | 20% | 20% | 23% | 23% |
| Difference at the follow up | -53%*** | -39%*** | -52%*** | -40%*** | -46%*** | -29%*** | -43% | -29% |
| MDID-Difference in difference (p.p.) | -4.3*** | -6.8*** | -4.1*** | -6.7*** | -4.0*** | -5.7*** | -4.1 | -5.7 |
| Equivalent in number of formal jobs (DID times treated population, thousands) | 195 | 305 | 280 | 458 | 180 | 256 | 282 | 389 |
| % of treated on occupied (weighted) | 45% | 45% | 33% | 33% | 45% | 45% | 33% | 33% |
| Impact over total informality rate (p.p.) | -2.0 | -3.1 | -1.4 | -2.2 | -1.8 | -2.6 | -1.4 | -1.9 |
| R2 | 0.26 | 0.13 | 0.25 | 0.14 | 0.20 | 0.07 | 0.19 | 0.07 |
| Common support | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| # observations (thousands) | 289 | 167 | 590 | 296 | 289 | 167 | 590 | 296 |
| Control at baseline (thousands) | 90 | 28 | 207 | 56 | 90 | 28 | 207 | 56 |
| Treated at baseline (thousands) | 57 | 57 | 94 | 94 | 57 | 57 | 94 | 94 |
| Control at follow up (thousands) | 83 | 23 | 192 | 49 | 83 | 23 | 192 | 49 |
| Treated at follow up (thousands | 59 | 59 | 97 | 97 | 59 | 59 | 97 | 97 |

*Testing the results*

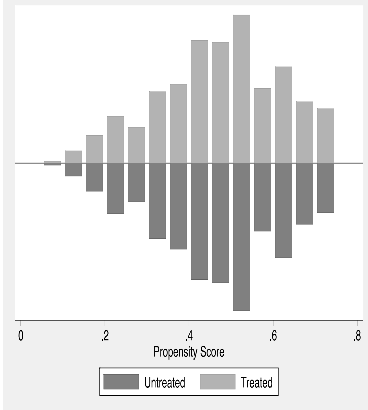
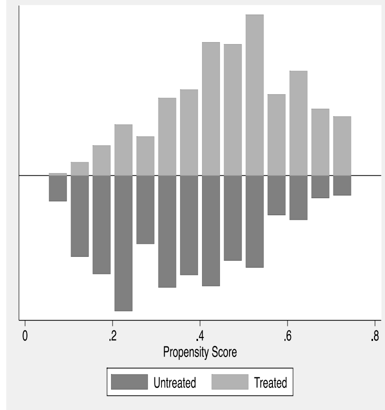
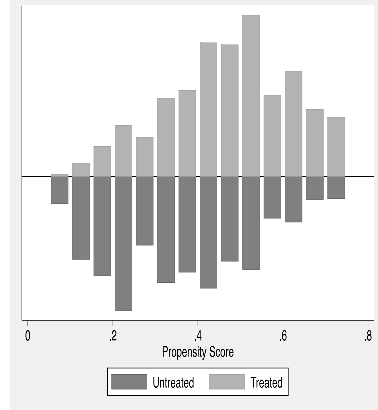
In what follows we analyse five possible issues that may create bias and inconsistencies in the exercise we summarized in the previous subsection[15]: common support, parallel trends, quality of the matching, and exogeneity of the treatment. For this purpose, we will make reference only to the tests performed on the 13- Main Metropolitan Areas Survey using the legal definition of informality, which is the most significant aggregate.

1. **Common support.** A key assumption of the Matching Differences in Differences procedure is the overlap of the region of common support between the treatment and the control group. It rules out the perfect predictability of the treatment, given that workers with same characteristics ($X_{it}$) might have a positive probability of being both participants and non-participants (Heckman, LaLonde, and Smith, 1999). In other words, we need that $0 < P(D_{it}= 1|X_{it}) < 1$.

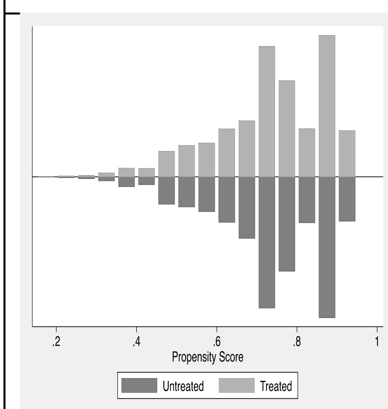
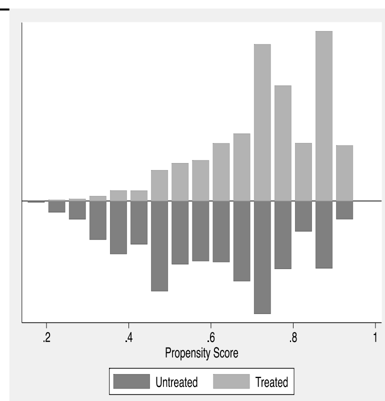
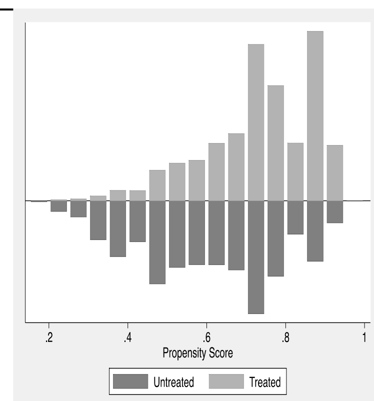
   In order to prove common support, a visual analysis is suggested by Caliendo et al (2005). Figure 6 shows that the p-score regions of the treated and the non-treated actually overlap, so overlap condition holds, and the concerns on perfect predictability of the treatment given the observables characteristics are ruled out[16]. According to Blundell (2009) in the MDID model the p-score distribution after the reform should be compared with the three other control groups (treatment before the reform and control before and after the reform).

[15] See Blundell (2009) and Lechner (2011) on MDID assumptions.
[16] The last tw o distributions are almost equal because the distribution does not change much across years

Figure 6a. P-score distribution.

| Treated at the follow-up and treated at the baseline | Treated at the follow-up and control at the baseline | Treated at the follow-up and control at the follow-up |
|---|---|---|
|   |   |   |

6b. P-score distribution excluding self-employment

| Treated at the follow-up and treated at the baseline | Treated at the follow-up and control at the baseline | Treated at the follow-up and control at the follow-up |
|---|---|---|
|   |   |   |

Source: own calculations. 13 metropolitan areas. Legal definition of informality.

In order to verify the former result, we applied the difference and differences and matching exercises with and without common support, and the number of observations excluded was minimal (0.01%). The differences in the outcome were only reflected in minimal changes in the standard errors and no differences were observed in the coefficients.

2. **Parallel trends.** Another key assumption of the Matching Differences and Differences approach is parallel trends. This feature ensures that, in the post treatment period, the impact is caused by the reform and not by other factors or trends linked to the fact of belonging to the treated or to the control group. According to this assumption, unobservable variables such as growth, should affect the outcome variable, informality, of treatment and control groups in a parallel (but not equal) way. In other words, if parallel trend holds, in the absence of the treatment (the tax reform) both populations would have experienced the same time trends conditional on X.

   We implemented a Placebo Test to check the parallel trend assumption. For the Placebo Test, the Matching and Differences in Differences Methods are applied to any other year with similar external characteristics, faking the existence of a tax reform or a similar shock, with the expectation that the results will not be affected. We performed this exercise using as alternative period 2012/2009. In contrast with the years for which we performed our base exercise (2014/2012), this alternative should reflect the impact of an inexistent tax reform. Indeed, we obtained no significant differences between the treatment and control groups in the results on informality, as shown in Table 5.

Table 5. Placebo Test. (Excluding self-employment)

| | 2012/2009 | | 2012/2009 (excluding self-employment | |
|---|---|---|---|---|
| | Informality | p-valule | Informality | p-value |
| Mean outcome of the control group at the baseline | 78% | 0.00 | 63% | 0.00 |
| Mean outcome of the treated group at the baseline | 30% | 0.00 | 30% | 0.00 |
| Difference at the baseline | -48% | 0.00 | -33% | 0.00 |
| Mean outcome of the control group at the follow up | 76% | 0.00 | 60% | 0.00 |
| Mean outcome of the treated group at the follow up | 28% | 0.00 | 28% | 0.00 |
| Difference at the follow up | -48% | 0.00 | -33% | 0.00 |
| Difference in difference | -0.2% | 0.53 | 0.1% | 0.75 |

Source: own calculations. 13 metropolitan areas. Legal definition of informality.

3. **Quality of the matching.** How robust are the results also depend on how successful was the matching on creating a contra-factual. One way to do this is to compare the mean of each of the variables in the treated group and in the untreated group. According to Rosenbaum and Rubin's (1985) the standardized difference of means should be less than 5%[17]. Tables 7a and 7b show that, after matching, all the control variables complied with this criterion[18]. However, it should be taken into account that since we are working with standardized bias, the average bias in the p-score is actually what matters the most when using p-scores is -0.7.

---

[17] The standardized bias for the dummy co-variables was estimated as:

$SB = 100 * (p_c - p_t) / [\{ p_t (1 - p_t) + p_c (1 - p_c) \} * 1/2 ]^{1/2}$, where $p_c$ is the proportion of each co-variable in the control group and $p_t$ is the proportion of each co-variable in the treatment group.

[18] With exception of the variable for young workers, in the case where self-employment is excluded, that is on the limit

Table 7a. Quality of the matching. Including self-employment

| | Mean in treated | Mean in untreated | Std error | | Mean in treated | Mean in untreated | Std error |
|---|---|---|---|---|---|---|---|
| Women (spouse) | 0.13 | 0.13 | -0.0% | October | 0.08 | 0.08 | -0.1% |
| Women (other) | 0.27 | 0.27 | -0.3% | November | 0.09 | 0.09 | 0.3% |
| January | 0.07 | 0.08 | 0.3% | December | 0.08 | 0.08 | -0.2% |
| February | 0.08 | 0.08 | -0.1% | Less than 25 | 0.17 | 0.18 | -0.9% |
| March | 0.08 | 0.08 | -0.0% | More than 55 | 0.11 | 0.11 | -0.9% |
| April | 0.08 | 0.08 | -0.1% | Primary (-) | 0.13 | 0.13 | 1.3% |
| May | 0.08 | 0.08 | 0.2% | Tertiary (+) | 0.37 | 0.37 | 0.9% |
| June | 0.08 | 0.08 | -0.4% | Diploma | 0.72 | 0.72 | -0.2% |
| July | 0.09 | 0.08 | 0.4% | Big city | 0.37 | 0.36 | 3.5% |
| August | 0.09 | 0.09 | 0.3% | Border city | 0.09 | 0.08 | -0.9% |

Table 7b. Quality of the matching. Excluding self-employment

| | Mean in treated | Mean in untreated | Std error | | Mean in treated | Mean in untreated | Std error |
|---|---|---|---|---|---|---|---|
| Women (spouse) | 0.13 | 0.13 | -1.7% | October | 0.08 | 0.09 | -0.8% |
| Women (other) | 0.27 | 0.27 | -0.1% | November | 0.09 | 0.09 | -0.1% |
| January | 0.07 | 0.07 | 0.5% | December | 0.08 | 0.09 | -0.4% |
| February | 0.08 | 0.08 | -1.2% | Less than 25 | 0.17 | 0.19 | -5.1% |
| March | 0.08 | 0.07 | 0.3% | More than 55 | 0.11 | 0.12 | -2.8% |
| April | 0.08 | 0.08 | -0.5% | Primary (-) | 0.13 | 0.12 | 3.3% |
| May | 0.09 | 0.08 | 0.5% | Tertiary (+) | 0.37 | 0.38 | -3.7% |
| June | 0.08 | 0.09 | 0.6% | Diploma | 0.72 | 0.74 | -3.9% |
| July | 0.09 | 0.08 | 0.4% | Big city | 0.37 | 0.36 | 2.9% |
| August | 0.09 | 0.08 | 0.3% | Border city | 0.09 | 0.09 | -0.5% |

Source: GEIH own estimations. 13 metropolitan areas and legal definition of infomrality

4. **Exogeneity of the treatment. (Ashenfelter's dip).** A common critique to the difference in difference models with matching, and particularly over MDID with cross-section surveys, is to have a treated/no-treated variable endogenous to the policy implemented. This identification problem has been largely analysed by the literature (Abbring and Van den Berg (2003), Blundell (2009) and Lechner (2011) and is one of the downsides of using matching differences in differences that does not control for unobserved individual-specific shocks that influence the participation decision. A similar problem in another context might be easier to understand: A benefit program is implemented in two neighbouring towns and individuals migrate to the town where the program is implemented in order to get the benefits.

The percentage of formal workers in the control group indeed diminished from 2012 to 2014 and this might be biasing our results, but unfortunately we don't have a panel data to observe the number of formal workers that transit from control to treatment specifically. However, the direction of the bias that they create goes in the same direction of the spirit of the Law. In the case of the lower bound the impact on the lower threshold was not only positive but exactly the purpose of the reform: to reduce the labour cost and to make it more affordable to earn the minimum wage. It is, in a way, a channel through which the reform reduced informality. This is a desirable result, since "quasi-formal workers" that work in the formal sector but earn less than a minimum wage moved to be fully formal and are likely contributing to health and pensions. This problem is very different from cases in which, for example, the individual does not accept a job in order to qualify to get an employment benefit or to what can happen in the upper limit of the Colombian reform (more the 10 minimum wages): workers reporting to earn less than 10 minimum wages to get access to the benefits. In the case of Colombia, only the 0.8% of the workers earn more than 10% the minimum wage, so the movements in this segment caused by the reform are not significant.

In sum, we found that the informality rate of the treated group in comparison with the non-treated group decreased between 4.3 and 6.8 p.p. (depending if self-employment is excluded or not) of which some was made effective through the channel of formal workers earning less than a minimum wage that moved to be fully formal workers earning at least a minimum wage[19].

## *V. Impact of the Payroll Tax Reform on Income Distribution*

In order to be able to apply lessons from the Colombian case to other contexts and to analyse the impact of the reform on income distribution, we explored the characteristics of the workers most affected by the reform. We began by analysing the behaviour of the informality rate per income quintile for the first semester of 2012 and the first semester of 2014. As shown in Graph 7, informality lowered during the period of analysis primarily amongst the middle-income quintile which includes minimum wage earners. When we performed the Matching Differences in Differences exercise per socio-economic group, shown in Table 9, we also found that the workers with secondary education or less, benefited most from the reform. This can be explained by the fact that the reform removed a constraint that was bigger for minimum wage earners compared to workers receiving higher levels of income where wages are more flexible. The reform also benefited more males and adults 25-50.

[19] We also found that the change on the treated group w as overestimated by the survey, meaning that if w e get to use the matching differences in differences procedure using weights we will find a low er but more robust impact of the reform. According to Dugoff (2014) estimations can incur in a considerable bias if the survey bias is not respected. However, there is not a clear procedure to include w eights in the MDID method.

Graph 7. Lorenz curves before and after the reform

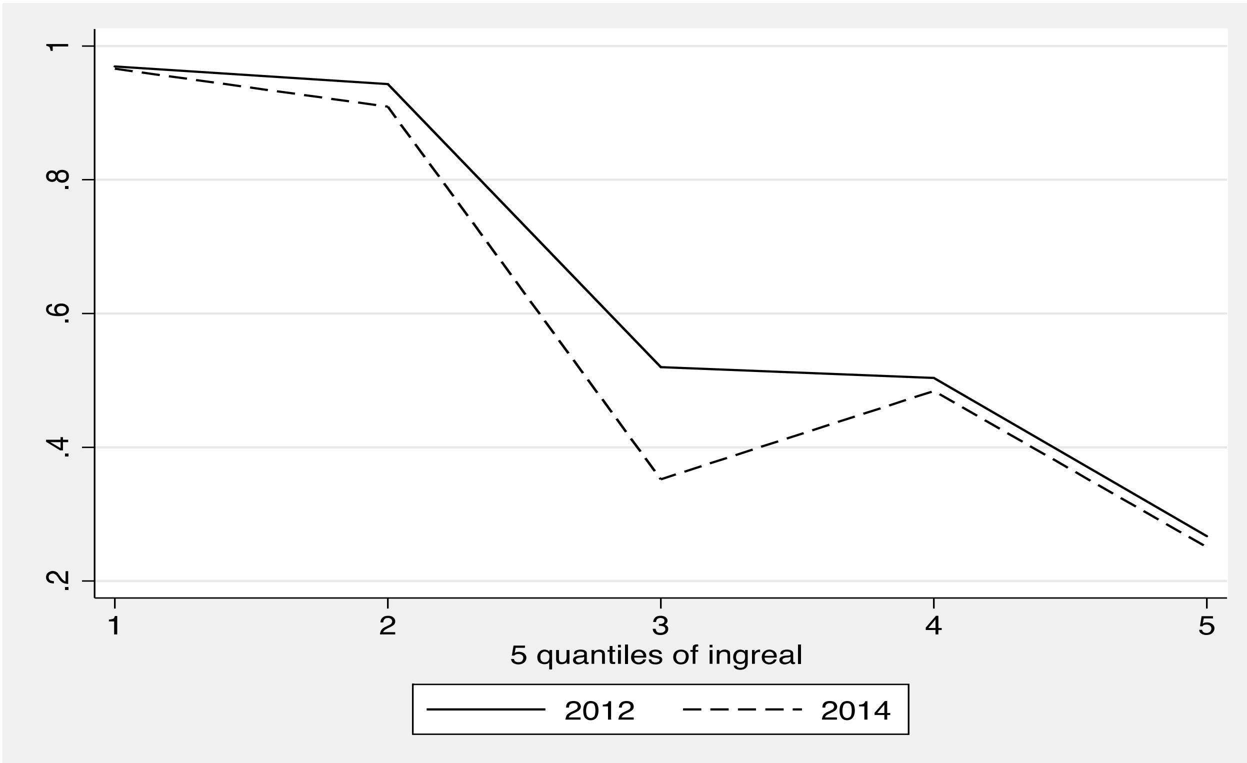



Table 9. Impact of the Tax Reform by population group.

| | | Baseline | | Follow up | | DID |
|---|---|---|---|---|---|---|
| | Outcome | Control | Treatment | Control | Treatment | |
| **Low Educated (Primary or less)*** | Informality | 91% | 49% | 93% | 39% | -10.4% |
| **High school*** | Informality | 81% | 28% | 84% | 24% | -6.9% |
| **Tertiary education or higher*** | Informality | 56% | 14% | 54% | 11% | -1.42% (n.s) |
| **Male 25-45 years** | Informality | 74% | 26% | 74% | 21% | -5.1% |

Source: Own calculations, based on GEIH 2007-2015. Excludes self-employment. *Male 25 – 45 years old. All results are significant up to 99% unless specified.

## VI. Conclusions

The Colombian government recently reformed the tax law by reducing payroll contributions from 29.5% to16% and substituting them with a profit tax. The law was passed in December 2012, and three years later the informality rate had diminished from 68% to 64% in 2015 (GEIH). In the 13 main metropolitan areas the reduction was a little more pronounced, going from 56% to 51% (Gran Encuesta Integrada de Hogares legal definition of informality, GEIH). This period was also characterised by high, yet also diminishing growth rates; changes in the tax rates, and increasing real minimum wages. It is of the most interest to know how much of this reduction was due to the tax reform.

This paper performs this task using a Matching and Difference in Differences methodology. The tax reform had a significant impact on the informality rate of the treatment group after controlling for observable and some unobservable characteristics. In fact, according to the results, the tax reform reduced the informality rate in the 13 main metropolitan areas, of the workers affected by the reform, in between 4.3 p.p.to 6.8 p.p. which translated in a reduction of the informality rate of the country between 2.0 and 3.1 p.p. given that the treated population was only 45% of the working population in the country. This result reflects the most common findings in other studies. However, new increases can be expected as a result of the current stage in the economic cycle. We also found that the payroll tax reform had the most impact on the middle-income population because workers in this group earn close to the minimum wage where the constraint was released. The reform also showed a higher impact on males between 25 and 50 years old.

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Annex 1

The method used to perform this match in this paper is the kernel propensity score matching following Heckman, Ichimura, and Todd (1997, 1998), and Blundell and Costa Dias (2009). The steps of the model that we implement in this paper, following the Stata code designed by Villa (20166) and the paper that accompanies the code, are the following:

1. To select the co-variables that might have an impact on the probability of either being treated or being informal [20]. All these variables should affect both the treatment and the outcome variable, without predicting it perfectly and satisfy the requirement of being independent from the treatment, or the anticipation of it. We included months as a covariable to avoid seasonality problems.
2. To estimate the propensity score, or the likelihood of being treated for all the four groups[21]. As suggested by Rubin and Rosembaum (1983), matching on the propensity score is equivalent to matching on co-variables, having the advantage or reducing the curse of dimensionality.
3. Depending on the researches preferences, the procedure can make use of all the p-score information available, or trim it to an area where there is available information for both the treated and the control groups (common support). We used common support in most of the estimation, since it reduces the chances of the kernel matching observations out of the common support area (Caliendo and Kopeinig, 2005). In our case, results were very similar, with and without common support.
4. To estimate the kernel distance between the propensity score of each observation in the treated group after the reform and the propensity score of each observation of a range in the three other groups (the control groups before and after the reform and with the treated group before the reform) giving the highest weight to those with p-scores closest to the treated individual[22]. The treated group after the reform is assigned a value of 1.
5. The kernel weights are used to estimate the differences in differences equation as follows[23]:

$$\mathrm{DID} = \{E(Y_{it=1}|D_{it=1} = 1, Z_i = 1, X_{it}) - w^{c}_{it=1} \times E(Y_{it=1}|D_{it=1} = 0, Z_i = 0, X_{it})\} -$$

$$w^{t}_{it=1} \{E(Y_{it=0}|D_{it=0} = 0, Z_i = 1, X_{it}) - w^{c}_{it=1} \times E(Y_{it=0}|D_{it=0} = 0, Z_i = 0, X_{it})\}$$

Where DID, is the difference in difference estimation; $Y_{it}$ is the outcome; $D_{it=1}$ is the existence (1) or absence (0) of a treatment and $Z_i$ is the group at which each observation

[20] W used all the months, in order to match both samples month by month.
[21] We used a logit model There is no much difference in using a logit or a probit model As suggested by Leuven and Sianesi (2006) The logit model used with odds ratio, with odds ratio has the advantage of correcting the bias of using the wrong weights, or as in this case – no weights. Unfortunately, the odds ratio is not available in the DIFF command
[22] The kernel method that has the advantage of reducing variance and making use of most available information. In this case we used the *Epachnikov* kernel.
[23] Unfortunately, we weren't able to use survey weights to extrapolate the results to the whole country's population since it is not clear how to use weights in the matching procedures (Leuven and Sianesi, 2003).

belongs being $Z_i = 1$; the treated group at the follow up and $0$ any of the three other groups; $w^{c}_{it=0}$, $w^{c}_{it=1}$ and $w^{t}_{it=0}$ are the kernel weights for the control in the baseline, the control group at the follow up, and the treatment at the follow up, respectively; $X_{it}$, the co-variables used in the exercise and $E(Y_{it}|D_{it}, Z_i)$ is the average outcome by group (On this case the probability of being informal conditional on the individual participation in the program, and on the group to which the individual belongs to).

By mixing the methodologies, Matching and Differences in Differences, we can control for differences in composition of the treated group before and after the treatment. Similarly, under this procedure, the assumption of common time effects on un-observables becomes common trend time effects on un-observables, and the linear assumption is no longer required (Blundell, 2009).

Annex 2. OLS estimation (non weighted)

| | Legal definition | | | | Firm definition | | | |
|---|---|---|---|---|---|---|---|---|
| | 13 areas | | Total | | 13 areas | | Total | |
| | Total | Workers and salaried | Total | Workers and salaried | Total | Workers and salaried | Total | Workers and salaried |
| Constant (B0) | 0.9136*** | 0.7825*** | 0.9305*** | 0.8453*** | 0.8269*** | 0.6366*** | 0.8587*** | 0.6984*** |
| | [268.1] | [133.6] | [329.7] | [168.1] | [236.7] | [107.4] | [268.5] | [121.2] |
| Year (B1) | -0.0090** | 0.004 | -0.0052** | 0.001 | 0.005 | 0.011 | 0.0060** | 0.002 |
| | [-3.1] | [0.7] | [-3.2] | [0.4] | [1.6] | [1.8] | [3.2] | [0.4] |
| Treated (B2) | -0.4752*** | -0.3380*** | -0.4576*** | -0.3631*** | -0.4259*** | -0.2810*** | -0.4249*** | -0.2950*** |
| | [-138.9] | [-68.2] | [-162.9] | [-98.7] | [-123.1] | [-56.4] | [-146.3] | [-71.7] |
| Effect on treated (treated*year) (B3) | -0.0235*** | -0.0384*** | -0.0340*** | -0.0409*** | -0.0223*** | -0.0331*** | -0.0282*** | -0.0279*** |
| | [-5.1] | [-5.5] | [-9.3] | [-8.6] | [-4.8] | [-4.7] | [-7.5] | [-5.0] |
| Women (spouse) | 0.0450*** | 0.0382*** | 0.0275*** | 0.0260*** | 0.0531*** | 0.0469*** | 0.0343*** | 0.0355*** |
| | [14.0] | [8.3] | [12.6] | [7.1] | [16.4] | [10.3] | [14.2] | [8.8] |
| Women (other) | 0.0056* | 0.0158*** | 0.0077*** | 0.0203*** | 0.0325*** | 0.0530*** | 0.0238*** | 0.0519*** |
| | [2.1] | [4.6] | [4.2] | [7.3] | [12.6] | [15.5] | [12.2] | [17.5] |
| Less than 25 | 0.1350*** | 0.1768*** | 0.1051*** | 0.1529*** | -0.0108** | 0.0283*** | -0.0086*** | 0.0216*** |
| | [43.8] | [44.8] | [49.1] | [48.9] | [-3.3] | [7.3] | [-3.4] | [6.2] |
| More than 50 | -0.0264*** | -0.0118* | -0.0181*** | -0.006 | 0.0673*** | 0.0857*** | 0.0566*** | 0.0822*** |
| | [-8.4] | [-2.3] | [-9.0] | [-1.5] | [21.7] | [16.5] | [27.3] | [20.2] |
| Primary (-) | 0.0295*** | 0.0291*** | 0.0344*** | 0.0485*** | 0.0460*** | 0.0525*** | 0.0384*** | 0.0491*** |
| | [8.4] | [4.8] | [15.4] | [11.6] | [13.1] | [8.7] | [15.2] | [10.5] |
| Tertiary (+) | -0.1717*** | -0.1329*** | -0.1837*** | -0.1407*** | -0.2124*** | -0.1298*** | -0.2218*** | -0.1344*** |
| | [-57.1] | [-36.7] | [-72.4] | [-44.2] | [-69.9] | [-36.2] | [-84.0] | [-41.3] |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Diploma | -0.1340*** | -0.1821*** | -0.1128*** | -0.1601*** | -0.1134*** | -0.1402*** | -0.0979*** | -0.1240*** |
| | [-39.6] | [-36.3] | [-45.8] | [-40.7] | [-32.5] | [-27.8] | [-35.8] | [-28.9] |
| Big city | -0.0643*** | -0.0673*** | -0.0634*** | -0.0650*** | -0.0406*** | -0.0256*** | -0.0397*** | -0.0235*** |
| | [-20.7] | [-17.5] | [-29.6] | [-21.7] | [-12.6] | [-6.6] | [-17.4] | [-7.4] |
| Border city | 0.0353*** | 0.0590*** | 0.0372*** | 0.0531*** | 0.0437*** | 0.0380*** | 0.0374*** | 0.0310*** |
| | [15.2] | [14.5] | [18.7] | [14.8] | [17.7] | [8.8] | [17.5] | [8.1] |
| 13 Metropolitan areas | | | -0.0288*** | -0.0551*** | | | -0.0283*** | -0.0545*** |
| | | | [-14.6] | [-17.1] | | | [-13.1] | [-15.6] |
| Rural | | | 0.0285*** | 0.0444*** | | | 0.0347*** | 0.0545*** |
| | | | [13.0] | [10.3] | | | [13.3] | [10.8] |
| Weights | 0.000 | 0.000 | 0.000 | 0.0000* | 0.000 | 0.000 | -0.0000*** | 0.000 |
| | [0.4] | [1.6] | [-0.3] | [2.5] | [-1.3] | [0.8] | [-3.5] | [0.6] |
| January | 0.006 | 0.006 | 0.0162*** | 0.0235*** | 0.0155*** | 0.0148** | 0.0164*** | 0.0199*** |
| | [1.4] | [1.1] | [5.7] | [5.2] | [3.7] | [2.7] | [5.3] | [4.1] |
| February | -0.008 | -0.0167** | -0.005 | -0.0130** | -0.0097* | -0.0139** | -0.0122*** | -0.0200*** |
| | [-1.9] | [-3.1] | [-1.9] | [-3.1] | [-2.4] | [-2.6] | [-4.0] | [-4.3] |
| December | 0.005 | 0.009 | 0.005 | 0.0104* | -0.001 | -0.004 | 0.0083** | 0.009 |
| | [1.1] | [1.6] | [1.8] | [2.4] | [-0.1] | [-0.8] | [2.8] | [1.9] |
| N | 289151 | 166544 | 590286 | 295770 | 289151 | 166544 | 590286 | 295770 |
| F | 7103.73 | 2366.34 | 12448.00 | 5802.36 | 6559.07 | 1452.10 | 9372.93 | 2337.00 |
| F | 16 | 16 | 18 | 18 | 16 | 16 | 18 | 18 |
| df_m | 289150 | 166543 | 590285 | 295769 | 289150 | 166543 | 590285 | 295769 |
| df_r | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| r2 | 0.40 | 0.30 | 0.43 | 0.34 | 0.39 | 0.22 | 0.39 | 0.24 |

Annex 3. OLS estimation results (non weighted)

| | | Legal definition | | | | Firm definition | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | 13-areas | | Total | | 13-areas | | Total | |
| | Coefficient. | Total | Workers and salaried | Total | Workers and salaried | Total | Workers and salaried | Total | Workers and salaried |
| Mean outcome of the control group at the baseline | $B_0$ | 91.4 | 78.3 | 93.1 | 84.5 | 82.7 | 63.7 | 85.9 | 69.8 |
| Mean outcome of the treated group at the baseline | $B_0 + B_2$ | 43.8 | 44.5 | 47.3 | 48.2 | 40.1 | 35.6 | 43.4 | 40.3 |
| Difference at the baseline | $B_2$ | -47.5 | -33.8 | -45.8 | -36.3 | -42.6 | -28.1 | -42.5 | -29.5 |
| Mean outcome of the control group at the follow up | $B_0 + B_1$ | 90.5 | 78.7 | 92.5 | 84.7 | 83.2 | 64.7 | 86.5 | 70.0 |
| Mean outcome of the treated group at the follow up | $B_0 + B_1 + B_2 + B_3$ | 40.6 | 41.0 | 43.4 | 44.3 | 38.3 | 33.3 | 41.2 | 37.7 |
| Difference at the follow up | $B_2 + B_3$ | -49.9 | -37.6 | -49.2 | -40.4 | -44.8 | -31.4 | -45.3 | -32.3 |
| Difference in difference | $B_3$ | -2.4 | -3.8 | -3.4 | -4.1 | -2.2 | -3.3 | -2.8 | -2.8 |
| % of treated on occupied | | 45% | 45% | 33% | 33% | 45% | 45% | 33% | 33% |
| Impact over total informality rate | | -1.1 | -1.7 | -1.1 | -1.3 | -1.0 | -1.5 | -0.9 | -0.9 |