Assessing the Effect of Payroll Taxes on Formal Employment: The Case of the 2012 Tax Reform in Colombia[*]

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Abstract

In 2013 Colombia implemented a tax reform which, among other things, reduced payroll taxes by a total of 13.5 percentage points of wages. In this paper we evaluate the effects of this component of the 2012 Colombian tax reform on firms' formal employment and average wages. We construct a panel of firms based on their employees' administrative records. In order to account for the endogeneity of the treatment, we use an instrumental variables technique that exploit the exogenous variation from the decisions of firms that are similar to each other in several dimensions, but belong to different economic sectors. Based on our preferred specification, we estimate a positive and significant increase in formal employment, as a result of the implementation of the reform, of a proximately 213k jobs in existing pre-reform firms. In the long run, these effects will increase to more than 600k jobs. The effect of the reform on the average wages paid by firms was also found to be positive for some sizes of firms, but the overall effect in the short run is rather small.

***Keywords*:** fiscal policy, payroll taxes, formal employment, formal wages

***JEL Codes*:** E62, H25, J21, J3

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1. Introduction

Payroll taxes have been at the center of a debate over their impact on formal employment and wages and have often been blamed for the high levels of informality that characterize the labor market in developing countries. Colombia is a special case in this matter because it has high levels of payroll taxes and high levels of informality as well. On the one hand, Colombia has one of the highest informality rates in the region: the informality rate reached a maximum of 54% for its main 23 cities in May 2009 (see Graph 1), which means that more than half their employees had an informal job. The informality rate for small cities was even higher and reached a maximum of 64% in 2010. On the other hand, regarding non-wage labor costs (payroll taxes assumed by both the employee and the employer), before 2012 these represented more than 60 percent of the wage rate (Hernández, 2012; Moller, 2012) (see Table 1).

Based on these facts, in 2013 Colombia implemented a reform of the tax code which, among other things, substantially reduced payroll taxes. The main purpose of this tax reform was the creation of formal jobs. The idea was that the reduction of payroll taxes would boost formal employment because it would cause a reduction in the cost that firms faced for their workers. More specifically, the new tax code reduced payroll taxes on wages by 13.5 percentage points for workers earning up to 10 times the minimum wage and working in firms with at least two employees.

This paper adds evidence to the literature on the effects of non-wage costs, which has provided mixed empirical results, by evaluating the effects of the 2012 Colombian tax reform on formal employment and the average wage a firm pays.[1] Using formal workers'

[1] Part of the international evidence for the United States and Latin American countries find that payroll taxes increase labor costs and reduce wages (Gruber, 1994, 1997; MacIsaac and Rama, 1997; Edwards and Cox-Edwards, 2000; Marrufo, 2000; Heckman and Pages, 2004; Mondino and Montoya, 2004; Kugler and Kugler, 2009; Cruces et al., 2010; and Scherer, 2015), reduce employment (Kaestner, 1996; Heckman and Pages, 2004; Kugler and Kugler, 2009; Scherer, 2015), and increase unemployment (Heckman and Pages, 2004); while other evidence shows minor or no effects on employment (Gruber, 1994, 1997, and Cruces et al., 2010), or minor effects on wages (Kaestner, 1996), or indicates that results are contingent on whether workers value the mandatory benefits (Lora and Fajardo, 2016), or whether minimum wages are binding (Heckman and Pages, 2004).

administrative records, we specify and estimate equations for their firm's labor demand and wages between January 2009 and December 2014. In order to take into account the heterogeneity of these effects for different types of firms, all the equations for 5 different samples were estimated according to the size of the firms before the implementation of the reform, in our main results table we present the estimation results using the whole sample. As a way to corroborate our findings, we estimate regressions aggregating the variables by combinations of municipality and economic sector; in these regressions, we divide estimation sample according to the size firms as well.

We find a positive and significant increase in formal employment after the implementation of the reform; this effect is similar in estimations with aggregated data by the municipality and economic sector. We find a small positive effect of the reform on wages, but only for some sizes of firms; the overall effect in the short run is very small as well. Our findings are robust to a set of changes in the specification of our econometric models and alternative ways of dealing with the endogeneity of our variables of interest. We perform a series of robustness checks and, in a broader sense, the impacts we compute using our preferred specifications are similar to the ones obtained from different specifications and methodologies.

In the second section of this paper, the 2012 tax code reform is described in detail. In the third section, the literature related to the connection between payroll taxes and labor market outcomes is described. In the fourth section, our sources of information are described and, in section five, our empirical strategy and methodology. In the sixth section, our empirical results are presented. In section seventh, some robustness checks of our results are presented, and in the last section, we draw conclusions and offer general policy implications.

2. The 2012 Colombian tax reform and its contexts.

Developing countries have made significant efforts to try to reduce the size of their informal labor market given it is usually characterized by the low productivity of informal firms, poor or no protection for workers, and avoidance of the rule of law.[2] There are many definitions of informality, most of which boil down to two broad concepts: informality based on social security contributions, and informality based on characteristics of the firm. In the case of the later, a worker is informal if she works for a small firm (usually 5 employees or less) or she is a self-employed non-professional.[3] In the case of the former, the definition of informality is based on the fact that the worker is officially covered by the social security system. Given the nature of our data and the administrative records of the social security system, our definition of a formal job is based on enrollment in social security.

Colombia is characterized by high levels of unemployment and informality by the standards of the Latin-American region.[4] Nevertheless, since 2009, the year in which the 2008 financial crisis had the greatest impact on the Colombian economy, the unemployment rate and informality has declined substantially. The national unemployment rate decreased by more than 3 percentage points (see Graph 3), and the informality rate in the 23 main cities declined by more than 4 percentage points (see Graph 1). During the same period, the economy experienced an important boost in wage-employment: the proportion of wage-employed to the total working age population of the country increased by almost 5 percentage points (see Graph 2).

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[2] See Meghir, Narita and Robin (2015) for evidence of the higher productivity of the formal sector, and Medina, Núñez and Tamayo (2013), Cárdenas and Mejía (2007) and López (2010) for evidence for the Colombian labor market.

[3] A worker is officially informal in Colombia if she is employed in a non-governmental firm of five or fewer employees, or if she is a self-employed with no college degree.

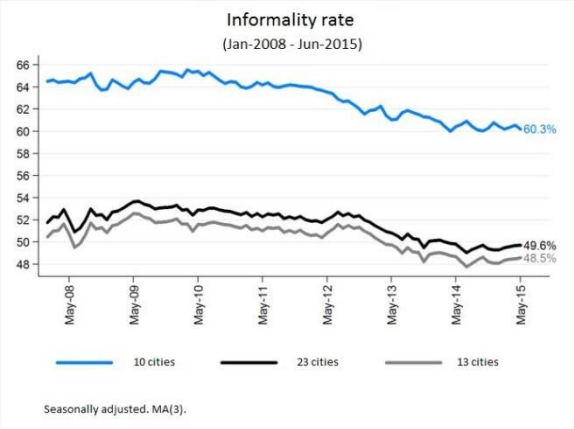
[4] It has the second highest estimated long run unemployment rate out of 19 countries in the Latin American and Caribbean region according to Ball et al. (2013), and it has one of the most informal economies in the region according to Perry et al. (2007).

Graph 4 shows the total number of formal workers by types of firms, and by firm size, based on the administrative records of employees contributing to the Colombian social security system.[5] Under our definition of formality, the number of formal workers has increased substantially since October 2008, the month the PILA (Integrated Record of Contributions to Social Security), our main source of information, began to be collected. The graph illustrates the total number of employees by firm size: almost 70% of the people employed work at firms with more than 100 employees. Firms with more than 500 employees represent almost 50% of total formal employment while firms with 2 to 5 employees represent a very small share of formal employment.

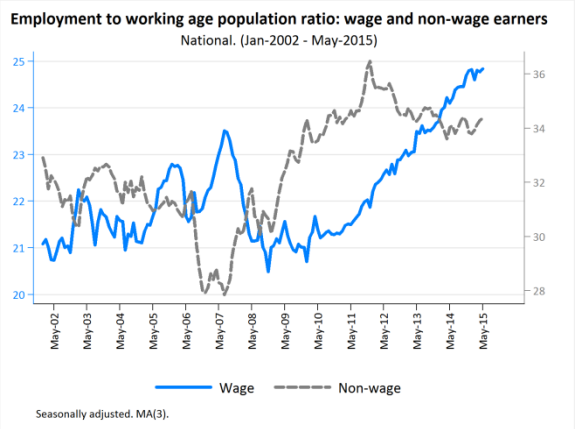
The implementation of the tax reform encompasses two periods: one from May 2013 to December 2013, which is represented by the shaded green area in Graph 4. During this period, eligible firms were exonerated from paying 5 percentage points of their wages. The second period started in January 2014, after the reform was fully implemented. This resulted in a 13.5 percentage point reduction in payroll taxes for workers earning less than 10 minimum wages and working for private, not-for-profit firms with at least 2 employees.[6] After the implementation of the tax reform, the total number of formal workers continued growing for all sizes of firms. Our objective in this research is to assess the existence and magnitude of a causal effect between the tax reform and the fluctuations in the average growth rate of formal workers in the post reform period.

[5] The administrative records containing the information on employees contributing to the Colombian social security system is called the *Planilla Integrada de Liquidación de Aportes* (PILA, by its acronym in Spanish).
[6] Act 1607 of 2012 and regulatory decree number 0862/2013.

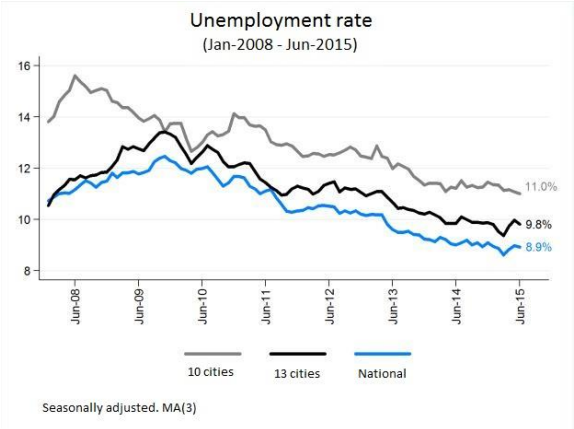
Graph 1



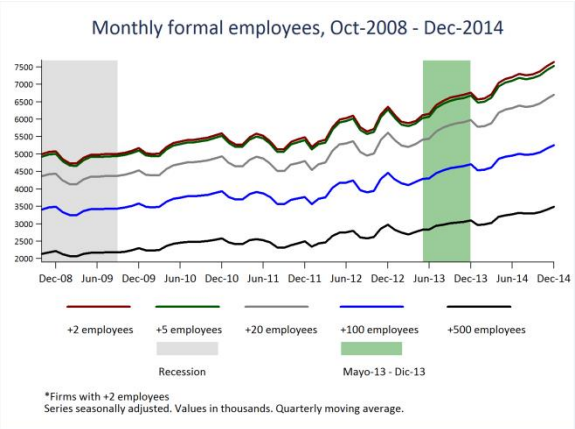
Graph 2



Graph 3



Graph 4



As Graph 1 and 2 show, the decrease in the informality rate and the increase in the number of wage-earners imply an improvement in labor market conditions in Colombia. Nevertheless, the levels of informality are still high, and the ratio of wage-earners to the total working age population is very low, even for a developing economy (25% at the national level). The large size of the informal sector has always been one of the top concerns regarding the Colombian labor market. There is a mainstream belief among labor economists which postulates that rigidities in the labor market and large non-wage costs are

a breeding ground for informality.[7] Before 2013, the Colombian labor market had one of the highest non-wage costs in the region.[8] Prior to the 2012 tax reform, the payroll taxes represented 60% of the average wage rate (Santa María et al., 2009; Hernández, 2012; Moller, 2012). The share of these non-wage extra-costs faced by the employer included social security contributions (health and pension), transportation subsidies, and payroll taxes.

Table 1: Pre-reform Non-wage costs.

	Contribution as % of wage rate
Pensions	16.0
Health care	12.5
Professional risks	2.0
Parafiscal	
Training (SENA)	2.0
In-kind childcare transfers (ICBF)	3.0
Compensation funds (*Cajas*)	4.0
Paid vacations	4.2
Severance pay	8.3
Mandatory bonuses	8.3
Total	60.3

Source: Hernández (2012)

Table 1 represents the baseline scenario of the payroll tax component before the changes in the tax code. Non-wage costs were 60.3% of the wage on average. The portion that the

[7] Bird and Smart (2012), Kugler and Kugler (2009), Sánchez, Duque and Ruíz (2009), Santa María, García and Mujica (2009), Peña (2013), etc.

[8] In 2012, Colombia was ranked 95 out of 183 countries in the Doing Business report (2012) based on an indicator that measures the number of payments per year, the time spent on payments, and the total tax rate faced by firms. By 2016 its rank was 136 out of 189 countries according to World Bank (2016).

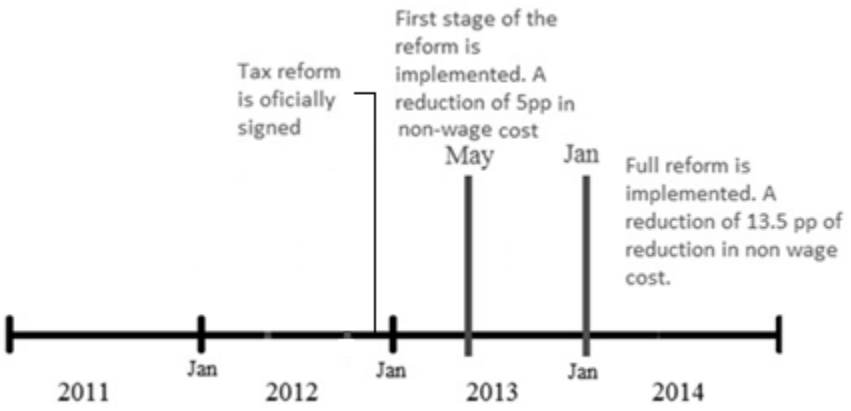
employer was obligated to pay by law was a total of 52.3% of the wage (subtracting 4% from employee contributions for pensions and health).[9] Under this scenario, the tax reform was proposed as a way to reduce labor costs and boost job creation, and especially, formal job creation. The changes to payroll taxes brought about by the 2012 tax reform eliminated the employer non-wage costs corresponding to contributions to health, job training programs (SENA), and childcare (ICBF), which were 8.5%, 2%, and 3% of the wages respectively. The elimination of these tax payments accounts for a total reduction of 13.5 percentage points in payroll taxes for workers earning up to 10 minimum wages, and who were not working in not-for-profit or public firms employing at least two people.

To understand our identification strategy, it is important to describe the timing of the reform carefully. The bill was officially presented to the Congress in October 2012. The main objectives of this bill were to foster formal employment and enhance equity by making taxes more progressive and promote the formalization of the labor market. In December 2012, the bill was approved, but the reduction in the payroll taxes was implemented in two stages. The first reduction, consisting of a 5 percentage point reduction in payroll taxes corresponding to the SENA (2 p.p.) and ICBF (3 p.p.) contributions, was put into implementation in May 2013. In January 2014, on top of this first reduction, the employer's health contributions (8.5 p.p.) were eliminated as well for a total non-wage cost reduction of 13.5 p.p. of the wage rate starting that month.[10] These reductions only apply for employees whose wages are between one and ten times the minimum wage. Figure 1 summarizes the timing of the reform. The 2012 tax reform also introduced a new profit tax of 9%, known as CREE, to replace the resources previously captured from wage taxes and contributions. This new profit tax of 9% was introduced at the same time that a reduction in the Colombian income tax was implemented as well. In other words, the 2012 tax reform reduced the income tax from 33% to 25%. In summary, the 2012 tax reform reduced taxes on wages and contributions by 13.5%, introduced a profit tax of 9%, and reduced the

[9] Note that the table does not include additional contributions such as the transportation subsidy for all employees earning up to two minimum wages (equivalent to about 11% of a minimum wage), neither the interests on severance payments (equivalent to 12% of a monthly wage).
[10] Act 1607/2012 and Decree 0862/2013.

income tax by 8 percentage points. The government revenue declined as a result of the reform by about 0.2% to 0.5% of the GDP (Fernández and Villar, 2016).

Figure 1: Timing of the 2012 Tax Code Reform



3. Literature Review

The main purpose of this paper is to assess the effect of a reduction in payroll taxes on the formal employment of firms. The evidence for the existence of a causal effect of non-wage costs on employment is ambiguous in the literature; some papers find evidence supporting this hypothesis while others do not. In the literature on this topic, the variation in non-wage costs is usually the result of increases in payroll taxes. The main contribution of this paper is assessing the existence of a causal relationship in the context of an economic policy that reduced the payroll taxes for firms sharply and over a short period. This is important given that a firm's response can be asymmetric when facing reductions or rises in non-wage costs.

In Gruber (1997, 1994), the author assesses the effect of a 25 percentage point reduction in payroll taxes in Chile that took place over a period of 6 years and concludes that the incidence of this reduction took place entirely in wages and did not have any significant effect on employment. An additional example of studies that do not find effects on employment but rather a full wage shifting of employer contributions is Gruber and

Krueger (1991), which considers the effect of disability insurance and maternity benefits. Some studies do find significant effects of payroll taxes on employment. Kaestner (1996) finds that an increase in the employer's cost of workers' compensation insurance significantly reduces employment for young adults and teenagers. In addition, they find that increases in insurance taxes reduce employment for teenagers.[11]

Among the studies that focused on the Colombian case, Kugler and Kugler (2009) examine the effect of a large increase in payroll taxes that took place in Colombia after a reform of the social security system in 1993. They find negative and significant effects on employment and wages. In a recent study, Antón (2014) looks at the same question we are trying to answer in this study by examining the 2012 tax reform in Colombia in order to evaluate the effects of a fall in payroll taxes on employment and wages. However, the methodology of the paper is different from the one in this study. Using a dynamic general equilibrium model, this paper finds that the reform would increase formal employment between 3.4 to 3.7 percent, and formal wage rates would increase by 4.9 percent.

3.1 Theoretical Effects of the Reform

Broadly speaking, the Colombian tax reform modified the income tax along with the payroll tax; therefore, it is convenient to analyze a simple theoretical framework that considers the effects of both taxes on the labor market. Using Cobb-Douglas production and utility functions, Nickell (2004) shows that in the presence of those taxes and, in addition, a consumption tax, the real post-tax consumption wage is given by $w\tau$, with $\tau = (1 - t_1)(1 - t_2)/(1 + t_3)$, where t_1 is the payroll tax, t_2 is the income tax, and t_3 is the consumption tax. A key result is that employment decreases with τ, that is, with increases in either the payroll or income taxes, t_1 or t_2, or reductions in the consumption tax, t_3. The 2012 Colombian tax reform did not modify the consumption tax, but article 94 reduced the income tax from 33% to 25% while article 20 created the 8% income tax for equity (CREE

[11] Hamermesh (2004) provides a survey of the findings regarding the effects of labor costs in general on labor demand in Latin-American countries.

for its acronym in Spanish) that provisionally would be 9% for the years 2013 to 2015 (Article 23). Although CREE is somewhat different from the traditional income tax in terms of its taxable base and other characteristics, in practice, the government collected the same amount per percentage point of each of these taxes, which implies that, between their previous income tax and the CREE, the total income tax paid by firms saw a rough increase from 33% to 34% beginning in 2013. This is a roughly 3.3% relative increase, smaller than the 0.135/1.6 = 8.4% relative decrease in total wage costs implied by the reduction in payroll taxes, but still important.[12] The potential connection between the income and payroll taxes is likely to lead to biased estimates in the empirical work unless that potential source of endogeneity is addressed by the identification strategy.

Once we focus on the effects of payroll taxes and consider the approach used by Gruber (1997) with labor supply and demand of form $D = D\left[w\left(1+\tau_f\right)\right]$ and $S = S\left[w(1-a\tau_e) + qw\tau_f\right]$ respectively and with a simple production function of form $F(L) = L^{\alpha}$; where w stands form the pretax wage, τ_f stands for the payroll tax rate on firms and τ_e stands for the payroll tax rate on workers; the expressions for the effect of payroll taxes on wages and labor become[13]:

$$(dw/w)/d\tau_f = \frac{\eta_s q - \eta_d}{\left(1+\tau_f\right)\eta_d - \left(1 - a\tau_e + q\tau_f\right)\eta_s}$$

and

$$(dL/L)/d\tau_f = -\frac{\left[1 + \left(1+\tau_f\right)(dw/w)/d\tau_f\right]}{(1-\alpha)\left(1+\tau_f\right)}$$

[12] According to the figures reported by the National Tax and Customs Direction (DIAN by its acronym in Spanish), the government collected COP$41.4 billion from income tax, and COP$14.5 billion from CREE in 2015, that is, nearly COP$1.6 billion per percentage point taxed in each of these cases. The amount collected in payroll taxes channeled to health insurance was COP$1.19 billion in 2013 (in 2015 COP$) per percentage point contributed to health. Since workers earning more than 10 minimum wages continued to contribute the 13.5 p.p., the reduction in the amount of payroll taxes between 2013 and 2014 was only COP$6.77 billion (in 2015 COP$), or in other words, 4.2 times the increase in the income tax.

[13] See also Gruber and Krueger (1991) and Kugler and Kugler (2009).

where a is the rate of discount by which employees discount the benefits they have access to with their payroll tax payments, and q is how much they value the benefits they have access to with the payroll taxes paid by their employers ($a = 0$ and $q = 1$ indicate that benefits are valued at their tax cost). The expression for wages is always negative. In particular, when benefits are fully valued at their tax cost, labor supply is perfectly inelastic, or labor demand is perfectly elastic, in which cases it is equal to -1/(1+ τ_f). In that case, there is no effect of payroll taxes on labor.

In practice, labor demand is not perfectly elastic nor is labor supply perfectly inelastic. In addition, while contributions to pensions or health could be expected to be fully valued by employees, other contributions imposed in Colombia such as those for childcare (3 p.p.) or to the Family Compensation Fund (*Cajas de Compensación Familiar*, 4 p.p.) might be fully valued only by workers with children attending public childcare centers, who receive the monetary subsidy and frequently visit the *Cajas*' recreational centers.[14] Contributions to SENA (2 p.p.), the main public national institution that provides job training and technical and technological programs, would be valued by workers taking courses, which they do for a relatively short span of their working lives. The less the workers value the contributions, the lower the shifting from payroll taxes to wages, and the larger the shifting to employment.

It is important to bear in mind that there is broad evidence that, in Colombia, the minimum wage is bidding. Thus, it is unlikely that payroll taxes could be transferred to wages at the low end of the wage distribution, and rather that they should directly affect employment.[15]

4. Data

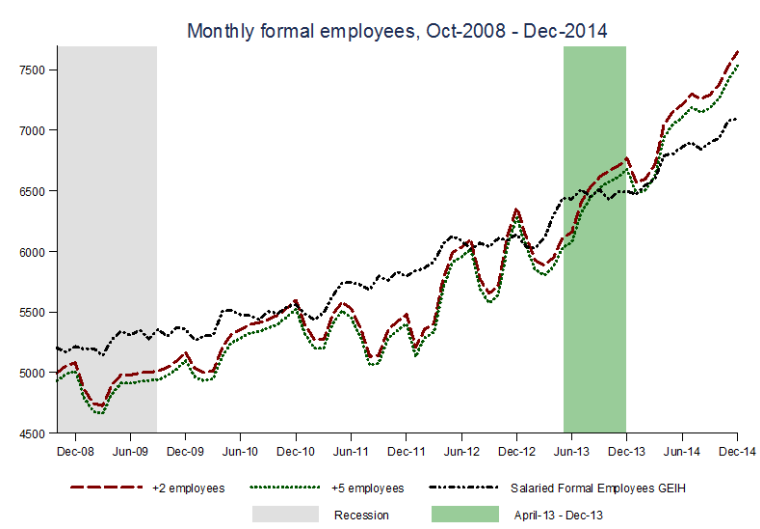
In this paper, we use firms' administrative records from the Colombian Ministry of Health and Social Protection, MHSP. Since 2008, Colombian firms have been required to report

[14] The monetary subsidy is a monthly transfer made by the *Cajas* to people depending on workers who earn no more than 4 minimum wages, work at least 96 hours per month, and earn jointly with his partner up to 6 minimum wages. The *Cajas* also offer other in kind subsidies through scholarships, books, drugs, etc.
[15] See Bell (1997), Arango and Pachón (2004), Maloney and Núñez (2004), Kugler and Kugler (2009) and Heckman and Pagés (2004), etc.

the social security payments for each of their workers. This system is known as the "Integrated Record of Contributions to Social Security" (PILA by its acronym in Spanish). When paying these mandatory contributions, employers must fill out a form for each of their employees. As a result, we can use information on firms and some basic demographic characteristics of the employees.

The PILA is a unique source of longitudinal monthly information about an employee, containing among other things, wages, contributions to pensions and health insurance, some basic demographic characteristics, and some basic characteristics of the firm. Using this information we construct a panel of formal employees working in all firms in Colombia. Again, employees are formal in the sense that they are reported to the PILA system, and their firms pay their payroll taxes. In the first months of PILA, there may be underreporting, at least in the first semester of 2008, because for some firms may take some months to comply with the obligation to report. In our applied work, we use a monthly panel of firms from July 2008 to December 2014. Since the implementation of the reform begun in May of 2013 and it was fully implemented by January 2014, the time frame of our data is adequate for the assessment of the policy.

Graph 5: PILA Employees versus Official Salaried Formal Workers



To summarize, PILA is a census of all formal firms and all formal workers employed by these formal firms in Colombia. Using the official definition of formality from the Administrative Department of National Statistics (DANE by its acronym in Spanish),

Graph 5 compares the total employment computed using PILA with the total formal-salaried employment. The latter is obtained from the official household survey used to report employment statistics in Colombia, the "*Gran Encuesta Integrada a Hogares*" (GEIH) collected by DANE. Measures of formal employment based on both the PILA and the GEIH should be relatively similar. Graph 5 shows that formal-salaried employment from these two sources is fairly comparable. Although the number of formal employees obtained from the PILA data is volatile, that should not affect our estimates, provided this difference is not related to the treatment intensity of the firms, which is what is expected.

5. Empirical Strategy

With the longitudinal information from the universe of all formal firms in Colombia, the effect of the reform on employment and wages is estimated using a linear regression strategy in a dynamic panel framework. In this paper, treatment consists of the reduction in payroll taxes due to the 2012 Tax Reform. The reduction in payroll taxes applies to all firms with at least two employees, working in the private for-profit sector, and to workers earning no more than 10 times the minimum wage (98% in our data), therefore, almost all firms are treated. Given this particular characteristic of the treatment, we exploit the intensity of the treatment to identify the effect of the tax reform. We use the size of the potential savings that benefit a firm as a result of the tax reform, as our measure of the intensity of the treatment. Potential savings refers to the additional monetary value that the firm would have paid in payroll taxes in a scenario without tax reform. Mathematically, this can be represented by the following expression:

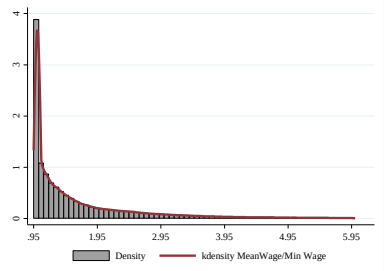
$$I_{j,t} = \sum_{\forall i \in j \ni w_{i,j,t} < 10mw} w_{i,j,t} * \delta_t,$$

where $w_{i,j,t}$ is the wage of employee *i* working for firm *j* at time *t*; and the summation includes all employees with wages lower than ten times the minimum wage, *mw*. Finally, δ_t is the percentage reduction in non-wage cost mandated by the reform.

The reform affects homogeneously to virtually all firms, and all these firms benefit from the same reduction in the payroll tax tariff; nevertheless, the composition of the payroll is heterogeneous across firms, this guarantees that our measure of intensity treatment has enough variation. As Graph 6 shows, average wages, expressed as a proportion of the

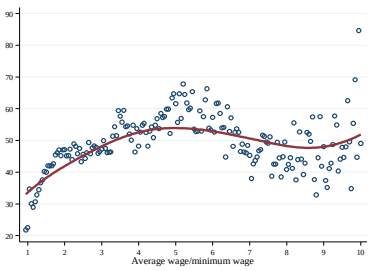
minimum wage, are concentrated near to the minimum wage, but the distribution of wages is moderately spread out. Graph 7 shows the average firm size by smaller bins of wages, the average firm´s employment increases for larger wages, this graph shows evidence on the important level of heterogeneity in firm size by different values of average wages. Graph 8 shows a histogram of the variable: share of employment with wages greater than 10 times the minimum wage. Around 85% of the firms have no employees with wages greater than ten times the minimum wage, the rest of the firms have a positive share of the payroll with these high wages; for instance, in more than 7% of the firms this share is greater than 5%, and in almost 5% of the firms this share is greater than 10%. Wages, firm´s total employment, and therefore the payroll cost, present important cross sectional variation; all this is evidence that firms vary considerably in their payroll composition; as a result of this, our intensity measure will have an important variation as well.

Graph 6: Wages distribution



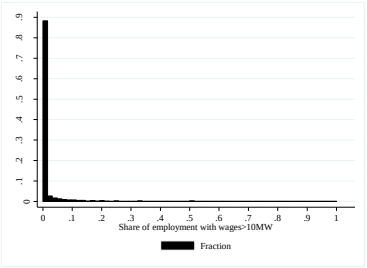
Notes: Density of average wages, expressed as multiples of the minimum wage.

Graph 7: Wages and employment



Notes: Average employment corresponding to small windows of average wages (Bandwith=0.01)

Graph 8: Payroll with wage>10MW



Notes: Distribution of the share of employment with wages greater than 10 times the minimum wage in the estimation sample.

The effect of the reform is assumed to be heterogeneous for some firm characteristics, and in particular, for their size based on their number of employees. Therefore, all our estimates are by samples of different firm sizes, based on the size the firms had at the baseline right before the approval of Act 1607 (December 2012). Five different sizes are considered: 2-5, 6-20, 21-100, 101-500, and more than 500 employees. In results tables, we present estimations with the entire sample as well.

Intensity of the treatment is an endogenous variable because it depends on wages, which are simultaneously determined with employment; in addition, its construction is done for all the employees earning less than 10 minimum wages, and therefore, it is highly correlated with the variable we want to explain, $e_{j,t}$. To circumvent the endogeneity problem, two different strategies are used: first, a modified version of the model that uses lagged wages

and employment is estimated to obtain the intensity of treatment, and second, an instrumental variable approach is implemented.

Let us first describe the modified version of the model, in which the treatment variable in period *t*, is denoted by $I_{j,t}^{-12}$, and is defined as:

$$I_{j,t}^{-12} = \sum_{\forall i \in j \ w_{i,j,t-12} < 10mw} w_{i,j,t-12} * \delta_t \qquad (1)$$

where, δ_t is the percentage reduction in non-wage cost generated by the reform, at time *t*, and $w_{i,j,t-12}$ is the wage of employee *i* working for firm *j* at time *t*-12. That is, to estimate the intensity of the treatment variable at *t*, we use the payroll tax percentage reduction at *t*, but the 12 month lagged wages ($w_{i,j,t-12}$), and the summation is on employees included in the restriction in $t-12$. Specifically, δ_t is equal to zero before May 1, 2013, it is equal to 0.05 between May 1st and December 31, 2013, and it is equal to 0.135 beginning January 1, 2014.

The regressions that are estimated can be represented using the following set of equations:

$$\begin{aligned}\ln(e_{j,t}) = {} & x_{j,t-12}'\beta_e + \alpha_e \cdot \ln(e_{j,t-12}) + \rho_{e1} ln\left(I_{j,t}^{-12}\right) + \rho_{e2} D_{2,j} \cdot ln\left(I_{j,t}^{-12}\right) \\ & + \sum_{s=0}^{2} \gamma_{es} \cdot D_{s,j} + \tau_{e1} \cdot time + \tau_{e2} \cdot time^2 + \pi_{ej}^{Y} + \pi_{ej}^{M} \\ & + \varepsilon_{ej} \qquad (2)\end{aligned}$$

$$\begin{aligned}\ln(w_{j,t}) = {} & x_{j,t-12}'\beta_w + \alpha_w \cdot \ln(w_{j,t-12}) + \rho_{w1} ln\left(I_{j,t}^{-12}\right) + \rho_{w2} D_{2,j} \cdot ln\left(I_{j,t}^{-12}\right) \\ & + \sum_{s=0}^{2} \gamma_{ws} \cdot D_{s,j} + \tau_{w1} \cdot time + \tau_{w2} \cdot time^2 + \pi_{wj}^{Y} + \pi_{wj}^{M} \\ & + \varepsilon_{wj} \qquad (3)\end{aligned}$$

Where $e_{j,t}$ is the number of employees in firm j and period t, $w_{j,t}$ stands for the average monthly wage of firm j and period t; $x_{j,t-12}$ is a vector of a firm's characteristics the year before; $e_{j,t-12}$ and $w_{j,t-12}$ are the firm's employment and average wage a year before respectively. In addition, $\pi_{.j}^{Y}$ and $\pi_{.j}^{M}$ are yearly and monthly fixed effects respectively. The regression includes three dummy variables: one dummy variable equal to one between January 1, 2009 and April 30, 2013, and equal to zero otherwise, D_0; another equal to one

between May 1st and December 31, 2013, and equal to zero otherwise, D_1; and a final dummy variable equal to one after January 1, 2014, and zero otherwise, D_2. Equations (2) and (3) allow for different impacts of the reform by the interaction between the intensity of treatment variable and the D_2 dummy variable. The effect of interest is given by $\rho_{.1} + \rho_{.2}$, which measures the elasticity of employment (or wages) to the intensity of treatment (change in payroll taxes) once the reform is fully implemented.

5.1 Instrumental Variable Approach

In addition to using the lagged treatment variable as in the modified model, the contemporaneous treatment, $I_{j,t} = \sum_{\forall i \in j \ni w_{i,j,t} < 10mw} w_{i,j,t} * \delta_t$ is also included and an instrumental variable approach is implemented in order to account for the endogeneity of $I_{j,t}$. We instrument our treatment variable using an instrument that exploits variation in the savings generated by the reform in firms that are similar to firm j in several characteristics. In particular, we exploit cross-sector variation in labor demand and wages (weighting the most similar firms more) to predict individual firms' labor demand and wages.[16]

More specifically, we construct a series of instruments that are weighted averages of savings generated by the reform in a group of firms that are similar to each firm in the estimation sample. To do this, a symmetric and row standardized proximity-matrix W is generated where each element of $W, \omega_{j,l}$, is a measure of the level of similarity of firm j with any other firm l in the sample. The Matrix W can be represented as:

$$W = \begin{bmatrix} 0 & \omega_{1,2} & \cdots & \omega_{1,N} \\ \omega_{2,1} & 0 & & \omega_{2,N} \\ \vdots & & \ddots & \vdots \\ \omega_{1,N} & \omega_{2,N} & \cdots & 0 \end{bmatrix}$$

$$Where, \omega_{j,l} = \frac{1}{\sqrt{\sum_{k=1}^{K} (c_{j,k} - c_{l,k})^2}} \qquad (6)$$

[16] This approach is similar in spirit to the one proposed by Bartik (1991) and followed by Blanchard and Katz (1992), Bound and Holzer (2000), Autor (2003), Notowidigdo (2010), Diamond (2010), Haltiwanger (2014), and Morales and Medina (2016). The methodology to construct the instruments resembles Morales (2015).

In previous equations, $c_{j,k}$ is the k characteristic of firm j, and $c_{l,k}$ is the k characteristic of firm l. The characteristics used to construct the instruments are: the size of the firm, its average wage, and its geographic longitude and latitude coordinates in kilometers. All these characteristics are standardized given that they are all measured by very different scales, and are estimated as averages from January 2012 to December 2012, which is the entire year before the tax reform was announced. This is done in order to guarantee the independence of the W matrix from the treatment variable.

The instrumental variable (E) used is the weighted average of the vector of all treatment intensities for each firm j in the sample using different lag orders for wages and employment for its construction ($I_{l,t}^{-L}$). Let us call this vector I_t^{-L}, which can be represented as:

$$E_t = W_l * I_t = \sum_{l=1}^{N} \omega_{j,l} I_{l,t}^{-L} \quad (7)$$

In order to guarantee the exogeneity of the instruments, the similar firms used to compute the weighted averages, belong to different economic sectors. In addition, lags and no current values of other firms' intensity of treatment are used to generate the instruments. Therefore, in such a case, for two firms j and l, $\omega_{j,l}$ is equal to zero if they belong to the same economic sector. Several instruments are generated using $I_{j,t}^{-6}, I_{j,t}^{-12}$, and $I_{j,t}^{2012}$ in equation (7). The variables $I_{j,t}^{-12}$and $I_{j,t}^{-6}$ represent potential savings due to the reform generated using the previous year and previous semester wage and employment respectively. Similarly, $I_{j,t}^{2012}$ represents potential savings due to the reform generated using the average wage and employment in 2012 when the tax reform had not yet been announced. We call these three instruments E_t^{-6}, E_t^{-12}, and E_t^{2012}.

Instruments similar to the ones we propose in this paper have been used in the literature before. In applied industrial organization literature, more specifically in the differentiated product demand estimation literature, studies usually use as instruments characteristics of other products as instruments for prices. The argument is that the degree of substitutability of a product will heavily influence its price (Berry, Levinsohn and Pakes, 1995). In this branch of the literature some studies have use the prices of the same products in other

markets (Nevo 2001; Hausman 1996) as instruments for prices, some other papers use characteristics of other neighborhoods as instruments for dwelling price (Bayer et al. 2007). On the second hand, in the social interactions literature, studies are usually interested in estimating the influence of a reference group's aggregate outcome on a particular entity's outcome. In order to do this, several studies have used as instruments characteristics of other entities outside of a particular entity's reference group. This practice is usually referred as using "excluded peers" information as instruments (De Giorgi et al., 2010; Bramoullé et al, 2009; Morales, 2015).

The construction of our exclusion restrictions is inspired in the literature mentioned in the previous paragraph. Our instruments $I_{j,t}^{-6}$, $I_{j,t}^{-12}$, and $I_{j,t}^{2012}$, are weighted averages of lagged payroll cost functions from other firms.[17] The existence of a strong correlation between the intensity of the treatment and the exclusion restrictions is expected because, as it is remarked in the social interaction literature, similar entities tend to behave similarly. Our assumption on the exogeneity of these exclusion restrictions is based on the fact we use firms from different economic sectors in order to construct our exclusion restrictions. We assume that the reference group, in a social interaction framework, consists of similar firms in the same economic sector; therefore, using firms in other economic sectors, is in a sense a generic way of using excluded peers information. In addition, we use lags instead of current values of the information from these similar firms.

In specification (2) and (3) there are 2 endogenous variables since the treatment intensity variable interacts with a dummy variable that is equal to one after the full implementation of the reform. In a case like this, the choice of instrument is complicated by the presence of the interaction. In order to properly identify coefficients ρ_1 and ρ_2 we follow a procedure based on Heckman and Vytlacil (1998). This is a two-step regression procedure, where, in the first step, $I_{j,t}$ is regressed on all exogenous variables including our three exclusion

[17] We run "2SLS regressions using instruments based on other characteristics of similar firms, but these instruments turn out to be weakly correlated with our endogenous variable. These instruments are easier to defend in terms of its exogenous character, nevertheless, in order to avoid a weak instrument problem we do not include them in our analysis.

restriction variables E_t^{-6}, E_t^{-12}, E_t^{2012}. From this regression, $\tilde{I}_{j,t}$, is obtained and, in a second stage, the instrumental variable regression is run using $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$ as instruments. This slight variation of the procedure presented in Heckman and Vytlacil (1998) is recommended in Wooldridge (2010) because it provides valid standard errors. The model estimated in the second stage is identified exactly because there are 2 instruments for two endogenous variables. Therefore, the relevance of our instruments can be tested using standard *F* tests in the first stage of the instrumental variable estimate, but no test can be run on the validity of our instruments in terms of over identification. In order to test our instruments for this type of validity, and check the robustness, over-identified 2SLS models of the equations (2) and (3) are estimated, but without the interaction term $D_{2,j} \cdot ln(I_{j,t})$. In these models, the same instruments, E_t^{-6}, E_t^{-12}, E_t^{2012} are used. The results of the over-identification tests and the treatment effects obtained from these models are presented in Tables 7 and 8 in the robustness checks section of the paper.

5.2 Estimate with aggregated data:

Our firm's estimates are complemented with estimates of wage and employment equations that use aggregated data by economic sectors in a given municipality. This is a way of corroborating our findings using the firm micro-data.[18] In particular, means of employment, the intensity of treatment, and covariates are computed for each economic sector in a given municipality. There are around 1100 municipalities in Colombia, and 10 economic sectors are used: Agriculture, Mining and Quarrying, Manufacturing, Construction, Energy and Utilities, Social Services, Transportation and Communications, Financial Services, Commerce, and Real estate. In the regressions with aggregated data we use an instrumental variable approach as well, the instrument we use are aggregations by municipalities and economic sectors of the instruments we compute by firms.

6. Summary Statistics and Results

[18] Estimation with aggregated data may be less sensitive to selection into estimation sample issues because any combination of municipality-sector is observed throughout the entire study period.

6.1 Summary Statistics

In Table 2 the summary statistics of a sample of more than 7'500,000 period-firm observations are presented. As the reader may remember, we are only considering firms with more than 2 employees, which are formal in the sense that they pay payroll taxes and contributions to their employees' social security. The average size of the firms on the panel is 52 employees. The average wage is COP$ 920,000 (around USD$300). In addition, 52% of the employees in these formal firms earn the minimum wage, 55% are between 25 and 44 years old, and 61% of them are males. The great majority of the firms in the sample are private firms (97%), and they belong mostly to the following economic sectors: trade, hotels, and food services (22%); real estate and leasing services (24%); community, social, and personal services (15%); and manufacturing (9%).

The intensity of treatment variables are the potential savings in labor costs that the reform implies for firms. The current intensity of treatment, $I_{j,t} = \sum_{\forall i \in j \ni w_{i,j,t} < 10mw} w_{i,j,t} * \delta_t$ is an average of COP$1.5 million per firm, but the average after the implementation of the reform is COP$5.7 million. This average amount of savings is not negligible at all. For example, taking into account the fact that the average wage per firm is 0.92 million pesos, the total current savings equal the monthly payment of more than six employees. The distribution of saving generated by the reform is highly spread out, the variance is almost COP$8 millions[19]. In addition, 95% of the firms in each month after the full implementation of the reform had payroll taxes savings smaller than COP$15 millions, while 75% of the firms had payroll taxes savings, as a result of the reform, smaller than COP$2.35 millions.

[19] For this calculation we exclude for the sample values grater to the 99 percentile of the distribution

Table 2: Summary Statistics by Firms

Variable	Obs	Mean	Std. Dev.
Employment	7534814	52.06	342.26
Real Average Wage	7527375	920042	742268
Private firm	7534814	0.97	0.18
Share of the payroll with wage <=1 MW (t-12)	7534814	0.52	0.39
Share of the payroll with 1 MW<wage <=2MW (t-12)	7534814	0.36	0.33
Share of the payroll with 3 MW<wage <=5MW (t-12)	7534814	0.06	0.12
Share of the payroll with 5 MW<wage <=10MW (t-12)	7534814	0.04	0.10
Share of payroll less than 25	7534814	0.20	0.26
Share of payroll between 25 and 44 (t-12)	7534814	0.55	0.24
Share of payroll between 45 and 59 (t-12)	7534814	0.22	0.19
Share of males in the payroll (t-12)	7532700	0.61	0.28
Mining	7532895	0.03	0.17
Manufacturing	7532895	0.11	0.32
Electricity, gas and water	7532895	0.00	0.06
Construction	7532895	0.09	0.29
Trade, hotels and food services	7532895	0.22	0.42
Transportation, warehousing and information	7532895	0.05	0.21
Finance services	7532895	0.05	0.22
Real estate, rental and leasing services	7532895	0.24	0.43
Community, social and personal services	7532895	0.15	0.35
$e_{t-12} * \overline{W}_{t-12} * \delta_t$	6623445	1506579	2180000
$e_t * \overline{W}_t * \delta_t$	7534814	1696867	2380000
$e_t * \overline{W}_t * \delta_t$ (Post-reform)	1718473	5669964	4230000
$e_{t-12} * \overline{W}_{t-12} * \delta_t$ (Post-reform)	2600129	4809446	4010000

Notes:

Monetary variables are expressed in current Colombian Pesos

6.2 Results

We estimate equations (2) and (3) with the complete firms´ sample; nevertheless, a different type of firm may be influenced differently by the reform; this may happen, among other reasons, because there is heterogeneity in the payroll composition of the firms. We test this hypothesis by estimating equations (2) and (3) using different samples, which are defined by the size of the firm. These firm sizes are constructed as a function of the average

firm´s employment in 2012 (the year before the tax reform began to be implemented). We find that elasticities of payroll savings to employment and wages are heterogeneous by firm type; therefore, in our applied work, we emphasize in the estimations by firm sizes. The sizes of the firms considered are between 2 and 5 employees, 6 and 20 employees, 21 and 100, 101 and 500, and finally firms with more than 500 employees. The regressions with aggregated data at the municipality-sector level are also presented; in this case, the means of all variables are computed by the municipality and economic sector using the same categorization as for the firm size in 2012. In addition, as our baseline model, we present estimates where the intensity of the treatment is contemporaneous ($I_{j,t} = \sum_{\forall i \in j \ni w_{i,j,t} < 10mw} w_{i,j,t} * \delta_t$), in which case our treatment variable is clearly endogenous for what was previously explained. The results of this specification are expected to be biased upwards.

From the estimation of regression equations (2) and (3), we obtain a significant and positive effect of the tax reform on employment both at the firm, and at the economic sector-municipality level. The evidence is mixed when it comes to average wages: for some firms the effect is positive and for others, it is negative. As the main purpose of this study is to assess the effects of the reform, its effects are summarized in Tables 3 to 6. In these tables, for each category of firm size, the effect of the intensity of treatment is translated into employment and average wage impacts generated by the reform. These computations are presented, for the estimations with firms, in Table 3 for employment and Table 5 for wages. Each of these tables summarizes the impact of the reform and contains three panels: the first and second panels present the OLS estimates of the contemporaneous and lagged treatment respectively, and the last panel presents the instrumental variable estimates of the effects that use the contemporaneous treatment. Tables 4 and 6 summarize the reform effects computed by municipality, economic sector and firm size on employment and average wages[20]. In all regressions we control for city fixed effects, month and year fixed

[20] In the Annexes (Tables 11 to 14), the full results of the 2SLS regressions are presented, and the OLS omitted for the sake of brevity; however, the intensity treatment effect coming from OLS models in Tables 3 to 6 is presented. In Tables 11 and 12, the IV estimates obtained from the contemporaneous treatment for

effect and a quadratic trend. In the estimates at the firm level, the fit of the regressions is quite good. In almost all regressions, the adjusted R^2 is higher than 55% in the employment regressions, and in the case of wages, the fit is even better with an adjusted R^2 greater than 90%. Similar fits are obtained for the regressions with aggregated data.

In the case of the employment regressions, for most of the firm size categories there is a positive and significant quadratic trend, and as could be expected, the one year lag for employment is important to explain current employment. In addition, the sixth order lag for mean wages has a negative impact on employment demand, conditional on the inclusion of the twelfth order lag, which has a positive effect. In regards to the control variables included in the firm's employment regressions, the one year lag for the share of the payroll that is minimum wage or less is negatively correlated with the level of employment. Employment is positively related to the share of those on the payroll who are under 44 years of age for firms with up to 100 employees. Finally, the share of males on the payroll has a positive correlation with employment for firms with up to 20 employees. However, in the case of larger firms, and in particular, for the very large firms (500+ employees), this correlation becomes negative.

In the average wage regressions for firms (Table 12), we find that the share of employees with 25 years or less on the payroll has a positive correlation with mean wages for all but the largest firms. The share of males is negatively related to mean wages for firms with up to 20 employees, but this relationship becomes positive for the largest firms. For all of the firm size categories, everything else being constant, there is a negative and significant quadratic trend, as lags of average wages correlate positively and significantly with current wages. The fit of the estimation regressions in the case of wages is even greater than in the employment regressions. In all cases, R^2 are above 90%.

Effects of the Reform on Employment

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firms are presented, and in Tables 13 and 14, the IV estimates of the contemporaneous treatment for the municipality-sector aggregates are presented.

In Table 3, the employment effects of the reform are presented for the whole sample and for different firm sizes that were identified from estimating equation (2) using firm data are presented. This specification includes an interaction term. Therefore, the short run effect of the full implementation of the tax reform is given by the sum of the coefficients of the log intensity treatment and the interaction of this variable with a dummy that is equal to one after December 2013 (full implementation period), $\rho_1 + \rho_2$ in equation (2).

The effect of the OLS estimated contemporaneous treatment is much higher than the effect of the OLS estimated lagged treatment. The elasticities obtained from the model with contemporaneous treatment are considerably greater than in the case of lagged treatment. In the case of the Instrumental Variable (IV) estimate, elasticities lie between the two previous estimates. The OLS estimate of equation (2), which uses the lagged intensity treatment, shows that a one percent reduction that a firm can obtain in the nonwage costs as a result of the tax reform increases a firm's employment of workers by 0.039%, 0.067%, 0.10%, 0.19%, and 0.17% for very small, small, medium, large, and very large firms respectively. The elasticity obtained from the estimation with the complete sample is 0.09%, close to the weighted average of the previous individual elasticities. These elasticities for the OLS model using contemporaneous intensity treatment are 0.16%, 0.31%, 0.39%, 0.47%, and 0.52% for very small, small, medium, big, and very big firms respectively. In this case, the elasticity obtained from the estimation with the complete sample is 0.34%. The contemporaneous intensity treatment is endogenous; therefore, the effects of the reform are expected to be over-estimated.[21] However, the effect of the reform can be underestimated using the lagged intensity treatment variable because the reform can cause savings in labor costs that are not collected if lagged wages and employment are used to construct the intensity of treatment variable. The elasticities computed from the 2SLS estimate lie between these two cases. They are 0.04%, 0.12%, 0.13%, 0.27%, and 0.32% for very small, small, medium, big, and very big firms respectively. As before, the elasticity obtained from

[21] In the case of regression with employment as dependent variables, for instance, OLS bias is expected to be positive because treatment intensity is a function of employment, which is the dependent variable; the error of equation (2) may be expressed as an increasing function of employment, and then, the correlation between this error and employment will be positive; therefore, the OLS bias would be positive.

the estimation with the complete sample, 0.16% is close to the weighted average of the previous individual elasticities.

Likewise, the employment gains based on the OLS models with the contemporaneous intensity of treatment are substantially higher than those based on the OLS models with lagged intensity of treatment. With the later, the estimate is a total of 129K jobs created in the short run as a result of the reform. This effect is substantially smaller than the one obtained with the former, 419K jobs created as a result of the reform. Our preferred estimate is the IV model. Remember that, in this case, we are following a procedure based on Heckman and Vytlacil (1998), where, as exclusion restrictions for a specific firm, weighted averages are used of the intensity of treatment in other firms that are similar to this specific firm in several dimensions. The employment effect of the reform with the IV model is 213k[22] new jobs created in the short run. The number of jobs resulting from the reform is computed from the elasticity identified in the regression times 13.5%, which is the potential savings on labor costs due to the reform. The lagged dependent variable allows us to obtain estimates of what the effect is in the long run. According to our IV estimates, by December 2015, 2016 and 2018 there could be 365K, 473K, and 534K new employees respectively, and in the long run, there could be 603K new employees as a result of the reform. Regressions in Table 3 do not control for the plausible endogeneity of the one year lag of the dependent variable; nevertheless, in a robustness check presented in section 7, in which this additional endogenous variable is controlled for, impacts of the reform in the short and long run comparable to the ones presented in Table 3 are found. These long term effects are similar to those found by Fernández and Villar (2016), and within the range of the effects found in a set of studies they cited which ranged between 145K and 700K new formal jobs.[23]

[22] The total aggregate impact is computed from the elasticities obtained from the regression for different firm's sizes.

[23] Bear in mind that the definition of formality considered by Fernández and Villar (2016) is different from ours and is based on the firm size (workers in firms with more than five employees), professionals, and technicians. The articles cited by these authors are Steiner and Forero (2016), Kugler and Kugler (2016), and Bernal, Eslava, and Meléndez (2016).

The effect of the reform is heterogeneous across firm size with higher elasticities for large firms. Regarding increase in employment, bigger firms contribute more to the new jobs generated by the reform. In the case of firms with more than 500 employees, this effect is estimated to be almost 130k jobs in the short run and 367K in the long run, which is more than 60% of the total estimated effect.

As Table 4 shows, the results of the estimates using aggregated sample by the municipality, economic sector and firm size are quite similar to the ones obtained using information by firm. Nevertheless, the effect on employment obtained from IV, which is our preferred specification, is a bit greater than the one estimated using microdata from firms. The overall number of jobs created due to the reform in the aggregated version of equation (2) is 225k jobs in the short run. As in the estimate based on microdata from firms, the effect of the reform is heterogeneous across firm size and bigger firms make a more important contribution to the new jobs generated by the reform, especially the ones with 100-500 and more than 500 employees. Together, these two firm sizes contribute more than 170K of the 231K estimated in equation (2) in the case of aggregated data.

Effects of the Reform on Wages

In Table 5, using firms' data, the average wage effects of the reform based on different firm sizes are presented. In some of the IV regressions, there is a positive and very small effect on average wages, with total elasticities below 1%. This is the case for the larger firms (100-500), medium sized firms (20-100) and the small firms (5-20) where tax reform has a slight positive effect on wages. In any case, the effect of the 2012 tax reform on average wages is small across all firm sizes in the short run. The total effect on wages resulting from the reform is computed from the elasticity identified in the regression times 13.5%, which is the potential savings on labor costs due to the reform. In general, the overall effect of the reform in the short run (weighting the effect of each firm size by the share of each category in total employment) is positive but small, an increase of 0.13% in average wages. In the long run, this effect rises; nevertheless, the total effect of the reform in the long run is still small (around 1%). In a robustness check presented in section 7, in which we control for plausible endogeneity of the one year lag of the dependent variable, we find impacts of the reform in the short and long run comparable to the ones presented in Table 5.

The results from the regressions using aggregated data by the municipality and economic sector are similar to the ones obtained from firm micro-data. For some of the firm sizes, there is no significant effect from the reform. However, in the case of small firms (2-5) and that of large (100-500) and very large firms (500+), the reform does have a positive effect on wages. In general, the overall effect of the entire reform, in the short run, is a rise of 0.5% in average wages. This is more than the effect computed with firm regression but still small.

Table 3: Intensity Treatment Effects by Firm on Employment

			OLS: Intensity of Treatment = $e_t * \bar{W}_t * \delta_t$			
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.347	0.164	0.315	0.403	0.445	0.554
	(0.011)	(0.008)	(0.019)	(0.011)	(0.030)	(0.032)
Log(I)*D2	-0.001	-0.013	0.013	0.006	0.043	0.012
	(0.009)	(0.003)	(0.012)	(0.012)	(0.028)	(0.033)
Elasticity	0.346	0.151	0.328	0.409	0.488	0.566
	(0.012)	(0.005)	(0.017)	(0.010)	(0.016)	(0.029)
Effect (13.5*Elasticity)	- -	1633	26521	63464	97956	229516
Total effect			419091			
1 year Lag coeff.	0.632	0.683	0.659	0.628	0.589	0.599
		(0.002)	(0.006)	(0.005)	(0.009)	(0.017)
Long Run Effect	- -	5150	77775	170603	238336	572360
Long Run Total Effect			1064225			
			OLS: Intensity of Treatment =		$e_{t-12} * \bar{W}_{t-12} * \delta_t$	
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	-0.005	-0.013	0.002	0.005	0.004	-0.099
	(0.004)	(0.000)	(0.005)	(0.009)	(0.017)	(0.036)
Log(I)*D2	0.094	0.052	0.065	0.096	0.183	0.272
	(0.006)	(0.001)	(0.007)	(0.011)	(0.022)	(0.054)
Elasticity	0.089	0.039	0.067	0.101	0.187	0.173
	(0.005)	(0.002)	(0.008)	(0.009)	(0.018)	(0.049)
Effect (13.5*Elasticity)	- -	432	5417	15789	37537	70152
Total effect			129328			
1 year Lag coeff.	0.687	0.707	0.720	0.689	0.636	0.684
		(0.002)	(0.005)	(0.004)	(0.009)	(0.019)
Long Run Effect	- -	1476	19348	50767	103122	222002
Long Run Total Effect			396716			
			IV: Intensity of Treatment =		$e_t * \bar{W}_t * \delta_t$	
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.176	-0.068	0.175	0.177	0.456	0.253
	(0.021)	(0.039)	(0.025)	(0.035)	(0.226)	(0.071)
Log(I)*D2	-0.021	0.108	-0.058	-0.046	-0.187	0.067
	(0.015)	(0.026)	(0.016)	(0.027)	(0.192)	(0.062)
Elasticity	0.155	0.040	0.117	0.131	0.269	0.320
	(0.008)	(0.013)	(0.011)	(0.012)	(0.037)	(0.033)
Effect (13.5*Elasticity)	- -	432	9460	20278	53996	129762
Total effect			213929			
1 year Lag coeff.	0.670	0.710	0.704	0.677	0.613	0.647
		(0.004)	(0.006)	(0.006)	(0.017)	(0.018)
Long Run Effect	- -	1491	31961	62779	139526	367597
Long Run Total Effect			603354			
Employment		80089	598947	1146600	1486889	3003746

Notes: Regressions with interactions are identified exactly with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given firm j, (E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012}) were constructed using firms in different economic sectors. The sample does not include public firms. The last row of the table indicates the total employment for each size of firm. Robust standard error is computed clustered by firm. Total effects were computed using only statistically significant elasticities in each regression by firm size.

Table 4: Intensity Treatment Effects by Municipality-Sector on Employment

	OLS: Intensity of Treatment= $e_t * \bar{W}_t * \delta_t$					
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.397	0.400	0.386	0.375	0.438	0.411
	(0.020)	(0.055)	(0.023)	(0.034)	(0.051)	(0.076)
Log(I)*D2	0.005	-0.060	-0.010	0.036	0.037	0.001
	(0.020)	(0.040)	(0.025)	(0.032)	(0.054)	(0.071)
Elasticity	0.402	0.340	0.376	0.411	0.475	0.412
	(0.016)	(0.068)	(0.020)	(0.015)	(0.030)	(0.052)
Effect (13.5*Elasticity)	- -	3676	30403	63619	95347	167068
Total effect			360113			
1 year Lag coeff.	0.586	0.615	0.591	0.620	0.567	0.539
		(0.021)	(0.011)	(0.009)	(0.019)	(0.031)
Long Run Effect	- -	9548	74334	167419	220200	362404
Long Run Total Effect			833906			
	OLS: Intensity of Treatment= $e_{t-12} * \bar{W}_{t-12} * \delta_t$					
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.053	0.068	0.090	0.072	0.000	-0.058
	(0.011)	(0.020)	(0.013)	(0.015)	(0.032)	(0.050)
Log(I)*D2	0.103	0.075	0.060	0.067	0.222	0.191
	(0.013)	(0.035)	(0.015)	(0.017)	(0.033)	(0.064)
Elasticity	0.156	0.143	0.150	0.139	0.222	0.133
	(0.010)	(0.039)	(0.013)	(0.013)	(0.027)	(0.042)
Effect (13.5*Elasticity)	- -	1546	12129	21516	44562	53932
Total effect			133685			
1 year Lag coeff.	0.626	0.645	0.631	0.658	0.608	0.587
		(0.022)	(0.011)	(0.009)	(0.019)	(0.033)
Long Run Effect	- -	4355	32869	62912	113679	130587
Long Run Total Effect				344402		
	IV: Intensity of Treatment= $e_t * \bar{W}_t * \delta_t$					
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.476	1.456	0.734	0.388	0.195	0.379
	(0.044)	(0.384)	(0.209)	(0.068)	(0.114)	(0.153)
Log(I)*D2	-0.141	-1.012	-0.523	-0.184	0.065	-0.084
	(0.034)	(0.342)	(0.179)	(0.049)	(0.089)	(0.122)
Elasticity	0.335	0.444	0.211	0.204	0.260	0.295
	(0.021)	(0.071)	(0.033)	(0.025)	(0.037)	(0.052)
Effect (13.5*Elasticity)	- -	4801	17061	31577	52190	119624
Total effect			225253			
1 year Lag coeff.	0.592	0.574	0.594	0.640	0.598	0.552
		(0.029)	(0.016)	(0.009)	(0.020)	(0.035)
Long Run Effect	- -	11269	42022	87715	129825	267018
Long Run Total Effect			537850			
Employment	6316272	80089	598947	1146600	1486889	3003746

Notes: Regressions with interactions are identified exactly with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given firm j, E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012}, were constructed using firms in different economic sectors. The sample does not include public firms. The last row of the table (employment) indicates the total employment for each size of firm. Robust standard error is computed clustered by municipality/sector. Total effects were computed using only statistically significant elasticities in each regression by firm size.

Table 5: Intensity Treatment Effects by Firm on Average Wages

	OLS: Intensity of Treatment= $e_t * \bar{W}_t * \delta_t$					
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.005	-0.014	0.029	0.015	0.008	0.005
	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)	(0.007)
Log(I)*D2	-0.001	0.002	0.004	0.000	-0.003	0.001
	(0.001)	(0.001)	(0.003)	(0.002)	(0.004)	(0.008)
Elasticity	0.004	-0.012	0.033	0.015	0.005	0.006
	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)	(0.007)
Effect (13.5*Elasticity)	- -	-0.162%	0.459%	0.203%	0.000%	0.000%
Total effect			0.078%			
1 year Lag coeff.	0.058	0.129	0.047	0.047	0.046	0.021
		(0.003)	(0.003)	(0.003)	(0.005)	(0.007)
Long Run Effect	- -	-0.186%	0.482%	0.212%	0.000%	0.000%
Long Run Total Effect			0.082%			
	OLS: Intensity of Treatment= $e_{t-12} * \bar{W}_{t-12} * \delta_t$					
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.000	-0.005	0.006	0.007	-0.011	-0.012
	(0.001)	(0.000)	(0.002)	(0.003)	(0.003)	(0.006)
Log(I)*D2	-0.002	-0.003	-0.002	-0.002	0.000	0.002
	(0.001)	(0.001)	(0.002)	(0.004)	(0.005)	(0.006)
Elasticity	-0.002	-0.008	0.004	0.005	-0.011	-0.010
	(0.001)	(0.001)	(0.002)	(0.002)	(0.005)	(0.006)
Effect (13.5*Elasticity)	- -	-0.054%	0.000%	-0.027%	-0.081%	0.000%
Total effect			-0.025%			
1 year Lag coeff.	0.059	0.133	0.044	0.045	0.047	0.024
		(0.003)	(0.003)	(0.003)	(0.005)	(0.008)
Long Run Effect	- -	-0.062%	0.000%	-0.028%	-0.085%	0.000%
Long Run Total Effect			-0.026%			
	IV: Intensity of Treatment= $e_t * \bar{W}_t * \delta_t$					
		2-5	5-20	20-100	100-500	500+
Log(I)	0.024	-0.014	0.047	0.013	0.212	0.011
	(0.007)	(0.013)	(0.009)	(0.012)	(0.093)	(0.013)
Log(I)*D2	-0.019	0.005	-0.032	-0.007	-0.180	-0.006
	(0.005)	(0.011)	(0.007)	(0.009)	(0.078)	(0.012)
Elasticity	0.005	-0.009	0.015	0.006	0.032	0.005
	(0.003)	(0.003)	(0.004)	(0.003)	(0.015)	(0.005)
Effect (13.5*Elasticity)	- -	-0.122%	0.203%	0.081%	0.432%	0.000%
Total effect			0.134%			
1 year Lag coeff.	0.920	0.973	0.915	0.917	0.859	0.935
		(0.004)	(0.005)	(0.006)	(0.025)	(0.020)
Long Run Effect	- -	-4.500%	2.382%	0.976%	3.064%	0.000%
Long Run Total Effect			1.067%			
Employment	6316272	80089	598947	1146600	1486889	3003746

Notes: Regressions with interactions are identified exactly with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given firm j E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012}, were constructed using firms in different economic sectors. The last row of the table indicates the total employment for each size of firm. Robust standard error is computed clustered by firm. Total effects were computed using only statistically significant elasticities in each regression by firm size.

Table 6: Intensity Treatment Effects by Municipality-Sector on Wages

OLS: Intensity of Treatment= $e_t * \bar{W}_t * \delta_t$						
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.062	0.081	0.074	0.064	0.051	0.029
	(0.004)	(0.020)	(0.005)	(0.006)	(0.011)	(0.011)
Log(I)*D2	-0.005	-0.029	-0.012	0.000	0.004	0.016
	(0.005)	(0.016)	(0.005)	(0.005)	(0.009)	(0.012)
Elasticity	0.057	0.052	0.062	0.064	0.055	0.045
	(0.006)	(0.025)	(0.005)	(0.005)	(0.007)	(0.016)
Effect (13.5*Elasticity)	- -	0.716%	0.837%	0.864%	0.743%	0.608%
Total effect			0.709%			
1 year Lag coeff.	0.032	0.084	0.022	0.009	0.026	0.020
		(0.013)	(0.004)	(0.005)	(0.009)	(0.014)
Long Run Effect	- -	0.781%	0.856%	0.872%	0.762%	0.620%
Long Run Total Effect			0.724%			
		OLS: Intensity of Treatment= $e_{t-12} * \bar{W}_{t-12} * \delta_t$				
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.031	0.033	0.042	0.027	0.029	0.011
	(0.003)	(0.014)	(0.005)	(0.005)	(0.007)	(0.013)
Log(I)*D2	-0.001	-0.004	-0.008	0.000	0.006	0.011
	(0.004)	(0.013)	(0.005)	(0.006)	(0.007)	(0.016)
Elasticity	0.030	0.029	0.034	0.027	0.035	0.022
	(0.004)	(0.015)	(0.004)	(0.005)	(0.006)	(0.015)
Effect (13.5*Elasticity)	- -	0.392%	0.459%	0.365%	0.473%	0.000%
Total effect			0.226%			
1 year Lag coeff.	0.026	0.075	0.015	0.003	0.019	0.019
		(0.013)	(0.004)	(0.004)	(0.009)	(0.014)
Long Run Effect	- -	0.423%	0.466%	0.366%	0.482%	0.000%
Long Run Total Effect			0.229%			
		IV: Intensity of Treatment= $e_t * \bar{W}_t * \delta_t$				
	All firms	2-5	5-20	20-100	100-500	500+
Log(I)	0.005	0.240	0.020	0.010	0.025	0.139
	(0.001)	(0.061)	(0.022)	(0.017)	(0.029)	(0.043)
Log(I)*D2	0.000	-0.185	-0.016	0.003	-0.001	-0.074
	(0.001)	(0.056)	(0.018)	(0.013)	(0.024)	(0.032)
Elasticity	0.005	0.055	0.004	0.013	0.024	0.065
	(0.001)	(0.014)	(0.006)	(0.008)	(0.010)	(0.018)
Effect (13.5*Elasticity)	- -	0.743%	0.000%	0.000%	0.324%	0.878%
Total effect			0.503%			
1 year Lag coeff.	0.761	0.800	0.757	0.814	0.709	0.727
		(0.025)	(0.020)	(0.020)	(0.043)	(0.046)
Long Run Effect	- -	3.713%	0.000%	0.000%	1.113%	3.214%
Long Run Total Effect			1.84%			
Employment	6316272	80089	598947	1146600	1486889	3003746

Notes: Regressions with interactions are identified exactly with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given observation j (E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012}) were constructed using firms in different economic sectors. The sample does not include public firms. The last row of the table (employment) indicates the total employment for each firm size. Robust standard error is computed clustered by municipality/sector. Total effects were computed using only statistically significant elasticities in each regression by firm size.

7. Robustness Checks

In order to check the robustness of our results, we estimated different specifications for our econometric models. These different approaches, on the one hand, allowed additional testing of the validity of our instruments, and on the other hand, offered an alternative for dealing with the endogeneity of the treatment intensity variable. Finally, a robustness check dealing with the plausible endogeneity of lags of the dependent variable in the regressions is presented.

7.1 Instrument validity

Over-identified models were estimated using the same specification in equations (2) and (3), but without the interaction term $D_{2,j} \cdot ln(I_{j,t})$, and exactly the same instruments were used in our preferred specification (E_t^{-6}, E_t^{-12}, E_t^{2012}). In these models without the interaction term, it is possible to test for over-identification restrictions using an over-identifying test[1]. This is done in order to have an idea of the validity of our instruments. Formally, over-identifying restrictions is a test of the independence of additional instruments with the regression error. Under the null hypothesis of the test, the instruments are valid, and we can have some confidence in the overall set of instruments used.

In Table 7 and 8, the estimated effects from the regressions of over-identified models for firms and municipality-economic sectors respectively are presented. In addition, using the same specification (without interactions), the results of exactly identified models are presented. In those cases, instrument E_t^{-6}, which is the strongest one, is used and the one that could potentially be the most endogenous of all three. In general, the effect computed from models without the interaction term $D_{2,j} \cdot ln(I_{j,t})$ is similar to the effects computed using our preferred specification.

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[1] The over-identifying restriction test is obtained as $N \cdot R_u^2$, where N and R_u come from an auxiliary regression of $\hat{U}_i$ on [**X** **Z**]. In this auxiliary regression, **X** represents the matrix of exogenous covariates and **Z** the matrix of instruments (Wooldridge, 2010). $N \cdot R_u^2$ is distributed χ^2 with degrees of freedom equal to the number of over-identifying restrictions. The null hypothesis of this test is the exogeneity of the instruments. Mathematically, H_0: $E(\mathbf{Z}'u) = 0$.

In the case of the regressions by firms, most of the estimates do not reject the over-identification hypothesis at the 95% confidence level. The over-identification hypothesis is rejected in only one regression, the wage regression for the second group. As shown in Table 7, the effects computed from the over-identified models with valid instruments (i.e. over-identified restriction hypothesis is not rejected) are similar to the effects computed from the exactly identified models using E_t^{-6} as the instrument. In the case of the robustness check regression by municipality sector, the over-identification hypothesis is rejected in some regressions (the 2-5 and the 5-20 firm size employment regressions), but again, the estimates are similar to the effects computed from exactly identified models using E_t^{-6} as the instrument.

In general the overall effect of the reform does not differ much across the different IV specifications regardless of whether the estimate is at the firm or the municipality-sector level or if the models are exactly or over identified. Considering the effects computed from all IV regressions, it may be said that the effect of the reform is not larger that 277k and not smaller than 188K new formal jobs in the short run, and between 600K and 657K in the long run, and that the effect on wages, in the short run, is in every case, substantially smaller than 1%.

Table 7: Instrument Robustness Checks by Firms

Effects on Employment by Firms					
Over Id models					
	2-5	5-20	20-100	100-500	500+
β	0.000	0.140	0.151	0.211	0.291
σ	(0.046)	(0.015)	(0.021)	(0.066)	(0.043)
F	326.667	14291.002	5622.278	209.579	905.161
Sargan Test (P-value)	0.785	0.087	0.258	0.066	0.117
Effect (13.5*Elasticity)	0	11320	23373	42354	118002
Total effect			195050		
1 year Lag coeff.	0.713	0.703	0.676	0.629	0.648
Long Run Effect	0	38115	72140	114162	335233
Long Run Total Effect			559650		
Employment	80089	598947	1146600	1486889	3003746
Exact Id models					
	2-5	5-20	20-100	100-500	500+
β	0.000	0.141	0.151	0.170	0.295
σ	(0.087)	(0.016)	(0.021)	(0.052)	(0.045)
F	393.6	40444.3	16535.2	228.3	2557.3
Effect (13.5*Elasticity)	0	11401	23373	34124	119624
Total effect			188523		
1 year Lag coeff.	0.712	0.703	0.676	0.636	0.648
Long Run Effect	0	38387	72140	93724	339841
Long Run Total Effect			544093		
Employment	80089	598947	1146600	1486889	3003746
Effects on Real Wages by Firms					
Over Id models					
	2-5	5-20	20-100	100-500	500+
β	0.000	0.037	0.000	0.117	0.000
σ	(0.022)	(0.008)	(0.007)	(0.054)	(0.008)
F	401.597	11844.756	5553.409	82.045	875.724
Sargan Test (P-value)	0.938	0.013	0.078	0.066	0.052
Effect (13.5*Elasticity)	0.000%	0.500%	0.000%	1.580%	0.000%
Total effect			0.419%		
1 year Lag coeff.	0.970	0.910	0.910	0.850	0.930
Long Run Effect	0.000%	5.550%	0.000%	10.530%	0.000%
Long Run Total Effect			3.005%		
Employment	80089	598947	1146600	1486889	3003746
Exact Id models					
	2-5	5-20	20-100	100-500	500+
β	0.000	0.030	0.000	0.112	0.000
σ	(0.045)	(0.006)	(0.007)	(0.051)	(0.008)
F	397.451	39771.115	16350.341	226.205	2469.556
Effect (13.5*Elasticity)	0.000%	0.405%	0.000%	1.512%	0.000%
Total effect			0.394%		
1 year Lag coeff.	0.967	0.915	0.916	0.852	0.935
Long Run Effect	0.000%	4.765%	0.000%	10.216%	0.000%
Long Run Total Effect			2.857%		
Employment	80089	598947	1146600	1486889	3003746

Notes: Standard errors in parenthesis.* p<0.10, ** p<0.05, *** p<0.01. In over-identified models, E_t^{-6}, E_t^{-12}, E_t^{2012} are used as the instrument, where for a given firm j (E_t^{-6}, E_t^{-12}, E_t^{2012}) were constructed using firms in different economic sectors. In exactly identified models E_t^{-6} is used as the instrument. The last row of the table (employment) indicates the total employment for each firm size. Robust standard error is computed clustered by firm.

Table 8: Instrument Robustness Checks by Municipality Sector

Effects on Employment by Municipalities					
Over Id models					
	2-5	5-20	20-100	100-500	500+
β	0.881	0.422	0.283	0.235	0.333
σ	(0.202)	(0.109)	(0.041)	(0.064)	(0.092)
F	2.848	4.739	13.403	41.874	10.628
Sargan Test (P-value)	0.000	0.002	0.146	0.138	0.513
Effect	9525	34122	43806	47172	135033
Total effect			269658		
1 year Lag coeff.	0.543	0.584	0.637	0.599	0.550
Long Run Effect	20843	82024	120677	117635	300074
Long Run Total Effect			641254		
Exact Id models					
	2-5	5-20	20-100	100-500	500+
β	1.057	0.434	0.286	0.244	0.340
σ	(0.286)	(0.109)	(0.042)	(0.069)	(0.093)
F	6.772	7.590	19.620	77.319	14.502
Effect	11428	35092	44270	48978	137872
Total effect			277641		
1 year Lag coeff.	0.519	0.582	0.637	0.598	0.549
Long Run Effect	23759	83953	121957	121836	305703
Long Run Total Effect			657208		
Employment	80089	598947	1146600	1486889	3003746
Effects on Real Wages by Municipalities					
Over Id models					
	2-5	5-20	20-100	100-500	500+
β	0.126	0.000	0.000	0.000	0.098
σ	(0.029)	(0.012)	(0.011)	(0.016)	(0.028)
F	22.610	89.544	80.032	46.085	25.336
Sargan Test (P-value)	0.000	0.170	0.101	0.002	0.885
Effect	1.701%	0.000%	0.000%	0.000%	1.323%
Total effect			0.651%		
1 year Lag coeff.	0.799	0.756	0.814	0.709	0.731
Long Run Effect	8.463%	0.000%	0.000%	0.000%	4.918%
Long Run Total Effect			2.446%		
Exact Id models					
	2-5	5-20	20-100	100-500	500+
β	-0.490	0.000	0.000	0.000	0.099
σ	(0.266)	(0.048)	(0.015)	(0.017)	(0.039)
F	5.760	7.065	19.590	84.496	14.937
Effect	-6.615%	0.000%	0.000%	0.000%	1.337%
Total effect			0.552%		
1 year Lag coeff.	0.914	0.762	0.813	0.710	0.731
Long Run Effect	-76.919%	0.000%	0.000%	0.000%	4.968%
Long Run Total Effect			1.387%		
Employment	80089	598947	1146600	1486889	3003746

Notes: Standard errors in parenthesis.* p<0.10, ** p<0.05, *** p<0.01. In over-identified models, E_t, E_t^{-12}, E_t^{2012} are used as the instrument, where for a given firm j (E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012}) were constructed using firms in different economic sectors. In exactly identified models, E_t is used as the instrument. The last row of the table (employment) indicates the total employment for each firm size. Robust standard error is computed clustered by municipality/sector.

7.2 Alternative ways of dealing with endogeneity.

Equations (2) and (3) were estimated using a firm's fixed effect panel estimate technique. This estimate makes it possible to eliminate permanent unobserved heterogeneity of firms from the error term of the equations, which may be the source of endogeneity bias. One limitation of firms' fixed effect estimate is that it cannot control for the existence of the firms' non-permanent unobserved heterogeneity, but it is still worth it to check for robustness.[2]

Table 9: Panel Firm Fixed Effect Regression Estimates

Effects on Employment by Firms					
	2-5	5-20	20-100	100-500	500+
Log(I)	0.006	0.003	0.001	0.000	0.002
Log(I)*D2	0.060	0.119	0.161	0.247	0.312
Elasticity	0.066	0.122	0.162	0.247	0.314
Effect (13.5*Elasticity)	714	9865	25076	49580	127329
Total effect			212564		
Employment	80089	598947	1146600	1486889	3003746
Effects on Real Wages by Firms					
	2-5	5-20	20-100	100-500	500+
Log(I)	-0.001	0.000	0.000	0.000	0.000
Log(I)*D2	-0.009	0.010	-0.001	-0.004	-0.003
Elasticity	-0.010	0.010	-0.001	-0.004	-0.003
Effect (13.5*Elasticity)	-0.135%	0.135%	-0.014%	-0.054%	-0.041%
Total effect			-0.023%		
1 year Lag coeff.	0.300	0.323	0.319	0.369	0.409
Long Run Effect	-0.193%	0.199%	-0.020%	-0.086%	-0.069%
Long Run Total Effect			-0.040%		
Employment	80089	598947	1146600	1486889	3003746

Notes: All significant effects are different from zero. The cases with no significant coefficients in the retrogressions are replaced by zeros The last row of the table (employment) indicates the total employment for each size of firm. Robust standard error is computed clustered by firm.

[2] Instrumental variable fixed effect regressions of equations (2) and (3) are presented as well. Unfortunately the properties of the set of instruments available are not as desirable as they are in the case of our preferred specification, and, therefore, the decision was made to not present them in this manuscript. Nevertheless, using the best instruments available, a short run impact was obtained of 174k formal jobs created by the Tax Reform, and of an economically negligible effect on wages.

The results of a firm's fixed effect estimate of equations (2) and (3) are presented in Table 9. From this estimate, the total number of jobs created due to the Tax Reform is almost 212k. This magnitude of formal job creation resulting from the Tax Reform is very similar to the one computed using our micro-data preferred specification. For average wages, the point estimates of the fixed effect estimate are, in some cases, different from our preferred specification. However, the main conclusion of these regressions is that the reform has had a negative, but extremely small effect on wages, which can be considered economically negligible. In the case of FE we do not interpret the long run effects because in the case that the one year lag of the dependent variable is endogenous in equation (2) and (3), the problem may be exacerbated by the typical fixed effect transformations. We deal with possible endogeneity of this variable in the next robustness check.

7.3 Additional endogeneity issues.

The two equations estimated in this paper (equations 2 and 3) are dynamic in the sense that they include a twelfth order lag of the dependent variable as a control variable. An additional concern that may arise from the estimate of dynamic models is the endogeneity of lagged dependent variables. In order to make sure the estimated impacts are not biased because of this issue, specifications of equations (2) and (3) are estimated where the one year lag of the dependent variable is treated as an endogenous variable as well. This increases the number of endogenous variables by an additional one. In order to estimate these models, controlling for this additional endogeneity problem, we follow Arellano Bond (1991). As additional instruments, we use high order lags (24 months or higher) of the dependent variable and its first difference, and high order lags of all exogenous covariates.

The results this additional robustness check are presented in Table 10. From this estimate, the total number of jobs created due to the Tax Reform in the short and long run is almost 174k and 668k, respectively. These magnitudes of the formal job creation resulting from the Tax Reform are greater in the long run, and smaller in the short run than the one computed using our preferred specification, but in a broader sense, the impact of the reform using both specifications is comparable. In the case of wages, using the specification

proposed in this robustness check, the estimated impact of the reform is still small in the short run as in the long run.

Table 10: Arellano and Bond Robustness Check

Effects on Employment by Firms					
	2-5	5-20	20-100	100-500	500+
Log(I)	0.222	0.244	0.200	-0.007	0.203
	(0.096)	(0.023)	(0.031)	(0.138)	(0.075)
Log(I)*D2	-0.167	-0.112	-0.094	0.182	0.073
	(0.073)	(0.014)	(0.020)	(0.106)	(0.062)
Elasticity	0.055	0.132	0.106	0.175	0.276
	(0.030)	(0.013)	(0.017)	(0.039)	(0.044)
Effect (13.5*Elasticity)	595	10673	16408	35128	111920
Total effect			174723		
1 year Lag coeff.	0.611	0.700	0.700	0.683	0.760
	(0.032)	(0.017)	(0.012)	(0.027)	(0.033)
Long Run Effect	1529	35577	54693	110813	466332
Long Run Total Effect			668944		
Employment	80089	598947	1146600	1486889	3003746
Effects on Real Wages by Firms					
	2-5	5-20	20-100	100-500	500+
Log(I)	-0.014	0.042	0.030	0.041	-0.002
	(0.068)	(0.048)	(0.037)	(0.041)	(0.038)
Log(I)*D2	0.024	-0.004	-0.047	0.003	0.012
	(0.075)	(0.049)	(0.037)	(0.043)	(0.042)
Elasticity	0.010	0.038	-0.017	0.044	0.010
	(0.033)	(0.035)	(0.025)	(0.024)	(0.030)
Effect (13.5*Elasticity)	0.000%	0.000%	0.000%	0.594%	0.000%
Total effect			0.140%		
1 year Lag coeff.	0.357	0.352	0.504	0.866	0.864
	(0.267)	(0.106)	(0.113)	(0.119)	(0.090)
Long Run Effect	0.000%	0.000%	0.000%	4.433%	0.000%
Long Run Total Effect			1.044%		
Employment	80089	598947	1146600	1486889	3003746

Notes: Standard errors in parenthesis.* p<0.10, ** p<0.05, *** p<0.01. The last row of the table (employment) indicates the total employment for each size of firm. Robust standard error is computed clustered by firm. Total effects were computed using only statistically significant elasticities in each regression by firm size. Robust standard error is computed clustered by firm.

7.4 Additional effects and treatments

Equations (1) and (2) are estimated using a different set of dependent variables, mainly average age and the share of the payroll receiving less than 1.5 minimum wages. These regressions are estimated using Instrumental Variable methodology, with same the

instruments used for the main employment and wage outcomes. This evidence deserves its own comments.[3]

On the one hand, for most of the firm sizes, the tax reform was found to reduce the firm´s average employee age. The overall effect is a reduction of almost 2% of the average age of the employees on the payroll in the short run as a result of the tax reform. This is evidence that the reform induced firms to hire young workers to fill vacant, newly created jobs; nevertheless, it is important to have in mind that Colombia has implemented a previous policy (Law 1429 of 2010), which is focused on the creation of formal jobs for young workers[4]. On the other hand, as a result of the reform, the share of workers in medium and large firms with wages less than or equal to 1.5 minimum wages increased significantly. Therefore, on average, new employees in these firms got an entry level wage close to the minimum wage. The overall effect is an increment of 0.138% in the share of employees with wages lower than 1.5 times the minimum wage, which is statistically significant, but very small.

8. Conclusions

[3] In an additional set of regressions, equations (1) and (2) including a variable that describes the percentage of workers with earnings lower than the minimum wage are estimated during the three month time span before firm employment was observed. This is an approach adopted by Neumark, Cunninham and Siga (2006) to capture the effect of the minimum wage on employment. This regression is run in an instrumental variable setting where the endogenous variables are $ln(I_{j,t})$ and $D_{2,j} \cdot ln(I_{j,t})$, and the same instruments as in our preferred specification are used (Table 3). The minimum wage is found to have a negative impact on employment. The overall elasticities are -2.3%. The effects of the 2012 reform on formal employment are essentially the same as those presented in Table 3; therefore, we are confident that the effect of the tax reform computed is ceteris paribus any effect that the minimum wage had on employment during those same years.

[4] During the time of implementation the 2012 Tax Reform, Colombia had been implemented another policy intervention with the same purpose of reducing labor market informality, this is better known as the "fist employment law" (law 1429 of 2010). In broad terms, this law reduces contributions to SENA, ICBF and CAJAS (4 percentage points paid by employers), in a lapse of no longer than 6 years, for young workers, workers with no experience in the labor market and with wages smaller than 1.5 times the minimum wage, and some other small groups. This intervention has been in place several years before the implementation of the 2012 tax reform. Since our variable of intensity of treatment capture the change in payroll savings after the implementation of the 2012 reform, plausibly, the effect we capture is clean of any effect from law 1429. If something, the effect we estimate from the tax reform would be a lower bound because some of the benefits of 1429 are replaced by the 2012 reform, which for some firms may have a discouraging effect in terms of job creation within the set of firms most likely intensive in employees earning less than 10 minimum wages, negatively affecting the effect on our treatment group.

The Colombian labor market has been characterized by high levels of unemployment and informality by the standards of the Latin-American region. In addition, Colombia is one the countries with highest levels of non-wage costs in the American region. Before this tax code reform, non-wage labor costs accounted for more than 60 percent of the wage rate (Hernández, 2012; Moller, 2012). To foster the creation of new, formal jobs, Colombia implemented a tax reform that reduced payroll taxes by 13.5 percentage points between 2013 and 2014. In this paper, labor demand and average wage equations are estimated to assess the effects of the reform on labor and wages. In our regressions, the total amount of payroll savings that are generated as a result of the policy change is used as our treatment variable. In all cases, our regression was estimated using micro data by firms and aggregated data by the interaction of municipality and economic sector.

A positive effect of the 2012 tax reform on firm employment is estimated. Our estimates reveal that the effect of the reform is positive for all firm sizes, but it is greater for larger firms, especially those having more than 100 employees, which accounts for more than 80% of the total effect. Using our preferred regression model (Instrumental Variables), it is clear that between 213k and 225k formal jobs were created in the short run as a result of the reform, and between 540K and 603K in the long run, depending on whether we use micro or aggregated data respectively.

For some of the firm sizes, there is a small but positive effect on average wages with elasticities substantially smaller than 1%. In general, the effect of the 2012 Tax Code Reform is small on average wages across all firm sizes. Our findings identify the effect of the entire reform, in the short run, as an increment of 0.12% and 0.42% in average wages for regression with micro and aggregated data respectively. In regressions that control for the endogeneity of the one year lag of the dependent variable, which is better suited for computing long term effects, small effects in the short and long run are found as well. Therefore, the conclusion is that the economic impact of the reform can be considered rather small.

Based on the figures for total taxes collected in the health sector, between 2013 and 2014, contributions to health went from 12.5 p.p. to about 8.5 p.p. of wages. Since 1 p.p. represents about COP$1 billion per year in contributions, total savings by firms because of

the reform would be expected to amount to about COP$6.35 billion a year. This, divided by 567K new employees, would be about COP$11.2 million a year per employee, which is roughly the cost of paying an average worker his base wage in one year. Thus, given the fact that nonwage costs were reduced from 1.6 to 1.465 times the base wage, employers would be investing about 40% of the total cost of employing the new employees generated by the reform, while their new savings would cover the remaining 60%.

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Annexes.

Table 11: IV Employment Regression Results by Firms

Variable	Firms 2-5	Firms 5-20	Firms 20-100	Firms 100-500	Firms 500+
	Log employment	Log employment	Log employment	Log employment	Log employment
Log(Savings t)	-0.068*	0.175***	0.177***	0.456**	0.253***
	(0.039)	(0.025)	(0.035)	(0.226)	(0.071)
Log(Savings t-12)*D2	0.108***	-0.058***	-0.046*	-0.187	0.067
	(0.026)	(0.016)	(0.027)	(0.192)	(0.062)
D0 (2012m12-2013m4)	-0.042***	-0.035***	-0.040***	-0.047***	-0.036
	(0.003)	(0.003)	(0.006)	(0.013)	(0.027)
D1 (2013m5-2013m12)	0.975**	-2.227***	-2.592***	-7.327**	-4.562***
	(0.470)	(0.315)	(0.499)	(3.545)	(1.233)
D2 (2014m1-2014m12)	-0.326*	-1.584***	-2.094***	-4.716***	-6.100***
	(0.175)	(0.152)	(0.177)	(0.615)	(0.623)
Trend	-0.052***	0.017***	0.031***	0.045**	0.082**
	(0.004)	(0.006)	(0.010)	(0.019)	(0.038)
$Trend^2$	0.000***	-0.000***	-0.000***	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log Employment (t-12)	0.710***	0.704***	0.677***	0.613***	0.647***
	(0.004)	(0.006)	(0.006)	(0.017)	(0.018)
Log Mean Wage (t-12)	0.345***	0.196***	0.331***	0.298***	0.319***
	(0.011)	(0.015)	(0.025)	(0.066)	(0.119)
Log Mean Wage (t-6)	-0.456***	-0.223***	-0.348***	-0.493***	-0.391***
	(0.008)	(0.013)	(0.021)	(0.054)	(0.100)
Share of the payroll with wage <=1 MW (t-12)	-0.222***	-0.196***	-0.103*	-0.509***	-0.376
	(0.045)	(0.049)	(0.055)	(0.183)	(0.290)
Share of the payroll with 1 MW<wage <=2MW (t-12)	-0.212***	-0.189***	-0.083	-0.414**	-0.201
	(0.043)	(0.048)	(0.052)	(0.183)	(0.260)
Share of the payroll with 3 MW<wage <=5MW (t-12)	-0.225***	-0.168***	-0.093*	-0.287**	-0.479**
	(0.050)	(0.048)	(0.052)	(0.141)	(0.187)
Share of the payroll with 5 MW<wage <=10MW (t-12)	-0.111**	-0.153***	-0.062	-0.241	0.068
	(0.050)	(0.051)	(0.065)	(0.173)	(0.292)
Share of payroll less than 25 (t-12)	0.216***	0.160***	0.083*	-0.117	-0.556
	(0.008)	(0.017)	(0.049)	(0.127)	(0.528)
Share of payroll between 25 and 44 (t-12)	0.151***	0.149***	0.115**	-0.099	-1.117**
	(0.007)	(0.016)	(0.047)	(0.121)	(0.499)
Share of payroll between 45 and 59 (t-12)	0.083***	0.079***	0.015	-0.211	-1.076*
	(0.008)	(0.017)	(0.052)	(0.132)	(0.587)
Share of males in the payroll (t-12)	0.012***	0.006	-0.013	0.017	-0.071
	(0.002)	(0.005)	(0.009)	(0.021)	(0.047)
City Fixed Effects	yes	yes	yes	yes	yes
Month Fixed Effects	yes	yes	yes	yes	yes
Sector Fixed Effects	yes	yes	yes	yes	yes
Constant	19.040***	-4.357**	-8.996***	-9.907*	-21.542*
	(1.097)	(1.772)	(2.985)	(5.691)	(11.797)
Observations	120044	574681	334938	103964	25425
Adjusted R-squared	0.613	0.607	0.605	0.614	0.660
F-statistic	780.98	7571.31	19205.39	10145.66	416.45

Notes:

Regressions with interactions are exactly identified, with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given firm j, E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012}, were constructed using firms in different economic sectors. The sample does not include public firms. The last row of the table (employment) indicates the total employment for each firm size. All regressions use a random sample of 35% of the formal firm population in Colombia.

Table 12: IV Average Wage Regression Results by Firms

Variable	Firms 2-5	Firms 5-20	Firms 20-100	Firms 100-500	Firms 500+
	Log wage	Log wage	Log wage	Log wage	Log wage
Log(Savings t)	-0.014	0.047***	0.013	0.212**	0.011
	(0.013)	(0.009)	(0.012)	(0.093)	(0.013)
Log(Savings t-12)*D2	0.005	-0.032***	-0.007	-0.180**	-0.006
	(0.011)	(0.007)	(0.009)	(0.078)	(0.012)
D0 (2012m12-2013m4)	-0.002	0.004***	0.000	0.014***	-0.001
	(0.001)	(0.001)	(0.002)	(0.004)	(0.007)
D1 (2013m5-2013m12)	0.145	-0.604***	-0.190	-3.314**	-0.197
	(0.162)	(0.116)	(0.164)	(1.454)	(0.234)
D2 (2014m1-2014m12)	0.094**	-0.215***	-0.097*	-0.519**	-0.132
	(0.039)	(0.050)	(0.051)	(0.251)	(0.097)
Trend	-0.036***	-0.029***	-0.020***	-0.014**	-0.048***
	(0.002)	(0.003)	(0.004)	(0.006)	(0.012)
$Trend^2$	0.000***	0.000***	0.000***	0.000**	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log Employment (t-12)	0.126***	0.046***	0.046***	0.050***	0.021***
	(0.003)	(0.003)	(0.003)	(0.006)	(0.007)
Log Mean Wage (t-12)	0.973***	0.915***	0.917***	0.859***	0.935***
	(0.004)	(0.005)	(0.006)	(0.025)	(0.020)
Log Mean Employment (t-6)	-0.124***	-0.046***	-0.040***	-0.056***	-0.016***
	(0.002)	(0.003)	(0.003)	(0.012)	(0.006)
Share of the payroll with wage <=1 MW (t-12)	0.138***	-0.061***	-0.024	-0.129**	0.003
	(0.015)	(0.022)	(0.025)	(0.060)	(0.081)
Share of the payroll with 1 MW<wage <=2MW (t-12)	0.155***	-0.036*	-0.002	-0.117*	-0.006
	(0.015)	(0.021)	(0.025)	(0.060)	(0.082)
Share of the payroll with 3 MW<wage <=5MW (t-12)	0.134***	0.007	0.059**	0.029	0.040
	(0.017)	(0.021)	(0.028)	(0.050)	(0.071)
Share of the payroll with 5 MW<wage <=10MW (t-12)	0.078***	-0.030	0.040	0.019	0.085
	(0.019)	(0.024)	(0.039)	(0.073)	(0.178)
Share of payroll less than 25 (t-12)	0.053***	0.055***	0.018	0.088	-0.051
	(0.002)	(0.008)	(0.016)	(0.066)	(0.084)
Share of payroll between 25 and 44 (t-12)	-0.000	0.035***	-0.004	0.034	-0.094
	(0.003)	(0.007)	(0.015)	(0.065)	(0.081)
Share of payroll between 45 and 59 (t-12)	-0.001	0.026***	-0.042**	0.012	-0.047
	(0.002)	(0.008)	(0.018)	(0.071)	(0.096)
Share of males in the payroll (t-12)	-0.018***	-0.011***	-0.003	0.010	0.015
	(0.004)	(0.002)	(0.003)	(0.006)	(0.010)
City Fixed Effects	yes	yes	yes	yes	yes
Month Fixed Effects	yes	yes	yes	yes	yes
Sector Fixed Effects	yes	yes	yes	yes	yes
Constant	11.482***	10.068***	7.261***	6.419***	15.764***
	(0.635)	(0.862)	(1.101)	(1.711)	(3.778)
Observations	120041	574665	334925	107404	25426
Adjusted R-squared	0.910	0.900	0.919	0.922	0.945
F-statistic	1372.21	15813.91	45841.42	15772.85	569.41
Standard errors in parenthesis. * p<0.10 ** p<0.05 *** p<0.01					

Notes:

Regressions with interactions are exactly identified, with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given firm j, E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012}, were constructed using firms in different economic sectors. The sample does not include public firms. The last row of the table (employment) indicates the total employment for each firm size. All regressions use a random sample of 35% of the formal firm population in Colombia.

Table 13: IV Employment Regression Results by Municipality-Sector

Variable	Firms 2-5 Log employment	Firms 5-20 Log employment	Firms 20-100 Log employment	Firms 100-500 Log employment	Firms 500+ Log employment
Log(Savings t)	1.456***	0.734***	0.388***	0.195*	0.379**
	(0.384)	(0.209)	(0.068)	(0.114)	(0.153)
Log(Savings t-12)*D2	-1.012***	-0.523***	-0.184***	0.065	-0.084
	(0.342)	(0.179)	(0.049)	(0.089)	(0.122)
D0 (2012m12-2013m4)	-0.055***	-0.027***	-0.022***	-0.036**	-0.033
	(0.012)	(0.007)	(0.008)	(0.015)	(0.027)
D1 (2013m5-2013m12)	-17.593***	-9.331***	-5.515***	-3.196*	-6.753**
	(4.671)	(2.673)	(0.961)	(1.795)	(2.655)
D2 (2014m1-2014m12)	-5.663***	-2.831***	-3.121***	-4.514***	-5.598***
	(0.938)	(0.448)	(0.379)	(0.624)	(0.975)
Trend	-0.007	-0.006	0.027**	0.054**	0.084
	(0.019)	(0.012)	(0.013)	(0.023)	(0.054)
$Trend^2$	0.000	0.000	-0.000*	-0.000**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log Employment (t-12)	0.574***	0.594***	0.640***	0.598***	0.552***
	(0.029)	(0.016)	(0.009)	(0.020)	(0.035)
Log Mean Wage (t-12)	0.150**	0.073***	0.034	0.082*	0.255**
	(0.058)	(0.025)	(0.033)	(0.047)	(0.103)
Log Mean Wage (t-6)	-0.365***	-0.129***	-0.064**	-0.104***	-0.028
	(0.030)	(0.025)	(0.025)	(0.039)	(0.078)
Private (t-12)	0.053**	-0.014	0.082***	-0.010	-0.232***
	(0.026)	(0.010)	(0.011)	(0.028)	(0.078)
Share of the payroll with wage <=1 MW (t-12)	-0.931*	-0.044	-0.188**	-0.072	0.665***
	(0.524)	(0.095)	(0.085)	(0.163)	(0.176)
Share of the payroll with 1 MW<wage <=2MW (t-12)	-0.998*	-0.050	-0.210**	-0.075	0.610***
	(0.514)	(0.095)	(0.086)	(0.152)	(0.146)
Share of the payroll with 3 MW<wage <=5MW (t-12)	-1.104**	-0.156	-0.170*	-0.170	0.176
	(0.483)	(0.097)	(0.097)	(0.185)	(0.227)
Share of the payroll with 5 MW<wage <=10MW (t-12)	-1.148**	0.014	-0.330**	-0.061	0.365
	(0.474)	(0.108)	(0.144)	(0.225)	(0.318)
Share of payroll less than 25 (t-12)	0.061	0.123***	-0.036	0.193	0.061
	(0.050)	(0.036)	(0.065)	(0.237)	(0.405)
Share of payroll between 25 and 44 (t-12)	0.012	0.103***	0.049	0.074	-0.340
	(0.045)	(0.031)	(0.058)	(0.218)	(0.373)
Share of payroll between 45 and 59 (t-12)	0.026	0.088**	0.095	0.057	-0.060
	(0.057)	(0.035)	(0.063)	(0.239)	(0.455)
Share of males in the payroll (t-12)	-0.006	0.037***	-0.006	-0.088**	-0.231*
	(0.015)	(0.011)	(0.018)	(0.040)	(0.131)
City Fixed Effects	yes	yes	yes	yes	yes
Month Fixed Effects	yes	yes	yes	yes	yes
Sector Fixed Effects	yes	yes	yes	yes	yes
Constant	7.743	3.498	-7.176*	-15.538**	-26.662
	(5.864)	(3.512)	(4.048)	(7.082)	(16.905)
Observations	61497	140795	111309	43149	16514
Adjusted R-squared	0.619	0.540	0.613	0.591	0.585
F-statistic	36.260	767.580	109.620	111.840	106.960

Standard errors in parenthesis. * p<0.10 ** p<0.05 *** p<0.01

Notes:

Regressions with interactions are exactly identified, with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given observation j are an average of E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012} by municipality and economic sector. The last row of the table (employment) indicates the total employment for each firm size. Robust standard error is computed clustered by municipality. All regressions use a random sample of 35% of the formal firm population in Colombia.

Table 14: IV Average Wage Regression Results by Municipality-Sector

Variable	Firms 2-5	Firms 5-20	Firms 20-100	Firms 100-500	Firms 500+
	Log wage	Log wage	Log wage	Log wage	Log wage
Log(Savings t)	0.240***	0.020	0.010	0.025	0.139***
	(0.061)	(0.022)	(0.017)	(0.029)	(0.043)
Log(Savings t-12)*D2	-0.185***	-0.016	0.003	-0.001	-0.074**
	(0.056)	(0.018)	(0.013)	(0.024)	(0.032)
D0 (2012m12-2013m4)	-0.013*	-0.001	0.013***	0.009	0.030***
	(0.007)	(0.003)	(0.003)	(0.006)	(0.010)
D1 (2013m5-2013m12)	-2.949***	-0.261	-0.125	-0.378	-2.355***
	(0.740)	(0.277)	(0.242)	(0.453)	(0.750)
D2 (2014m1-2014m12)	-0.772***	-0.081	-0.179	-0.395**	-1.165***
	(0.190)	(0.085)	(0.121)	(0.166)	(0.339)
Trend	-0.053***	-0.041***	-0.027***	-0.024**	-0.033**
	(0.010)	(0.005)	(0.005)	(0.010)	(0.016)
$Trend^2$	0.000***	0.000***	0.000***	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log Employment (t-12)	0.086***	0.021***	0.008	0.025***	0.019
	(0.013)	(0.004)	(0.005)	(0.009)	(0.014)
Log Mean Wage (t-12)	0.800***	0.757***	0.814***	0.709***	0.727***
	(0.025)	(0.020)	(0.020)	(0.043)	(0.046)
Log Employment (t-6)	-0.142***	-0.011**	-0.001	-0.018**	-0.015
	(0.015)	(0.005)	(0.005)	(0.009)	(0.010)
Private (t-12)	-0.054***	-0.052***	-0.026***	-0.001	-0.065***
	(0.012)	(0.005)	(0.006)	(0.013)	(0.022)
Share of the payroll with wage <=1 MW (t-12)	-0.177	-0.221**	-0.209***	-0.107	-0.088
	(0.124)	(0.100)	(0.064)	(0.094)	(0.062)
Share of the payroll with 1 MW<wage <=2MW (t-12)	-0.185	-0.175*	-0.187***	-0.015	-0.083*
	(0.120)	(0.099)	(0.064)	(0.080)	(0.042)
Share of the payroll with 3 MW<wage <=5MW (t-12)	-0.181	-0.073	-0.153**	0.314***	0.142*
	(0.110)	(0.104)	(0.068)	(0.086)	(0.077)
Share of the payroll with 5 MW<wage <=10MW (t-12)	-0.178	-0.013	-0.187**	0.237**	0.045
	(0.110)	(0.115)	(0.094)	(0.119)	(0.139)
Share of payroll less than 25 (t-12)	0.021	0.030	-0.032	-0.066	-0.285**
	(0.041)	(0.018)	(0.029)	(0.079)	(0.124)
Share of payroll between 25 and 44 (t-12)	0.019	0.011	-0.040	-0.036	-0.096
	(0.040)	(0.017)	(0.027)	(0.068)	(0.103)
Share of payroll between 45 and 59 (t-12)	0.023	-0.003	-0.040	-0.045	-0.143
	(0.041)	(0.019)	(0.031)	(0.079)	(0.107)
Share of males in the payroll (t-12)	0.003	0.007	0.005	0.006	0.038
	(0.009)	(0.005)	(0.009)	(0.015)	(0.036)
City Fixed Effects	yes	yes	yes	yes	yes
Month Fixed Effects	yes	yes	yes	yes	yes
Sector Fixed Effects	yes	yes	yes	yes	yes
Constant	19.562***	15.980***	11.227***	11.527***	14.379***
	(2.929)	(1.438)	(1.603)	(3.103)	(4.692)
Observations	61497	140795	111305	43148	16789
Adjusted R-squared	0.831	0.822	0.822	0.857	0.851
F-statistic	0.000	0.000	0.000	0.000	0.000
Standard errors in parenthesis. * p<0.10 ** p<0.05 *** p<0.01					

Notes:

Regressions with interactions are exactly identified with instruments $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$, where $\tilde{I}_{j,t}$ is the linear projection of $I_{j,t}$ in terms of all exogenous variables and exclusion restrictions E_t^{-6}, E_t^{-12}, E_t^{2012}. For this regression, exclusion restrictions for a given observation *j* are an average of E_t^{-6}, E_{jt}^{-12}, E_{jt}^{2012} by municipality and economic sector. The last row of the table (employment) indicates the total employment for each firm size. Robust standard error is computed clustered by municipality. All regressions use a random sample of 35% of the formal firm population in Colombia.