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

ABSTRACT In 2013, Colombia implemented a tax reform that reduced payroll taxes by a total of 13.5 percentage points of wages. This paper evaluates 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. To account for the endogeneity of the treatment, we use an instrumental variables technique that exploits 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 approximately 213,000 jobs in existing pre-reform firms. In the long run, these effects will increase to more than 600,000 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.

JEL Codes: E62, H25, J21, J3

Keywords: Fiscal policy, payroll taxes, formal employment, formal wages

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 has high levels of both payroll taxes and informality. The country's informality rates are among the highest in the region: the informality rate peaked at 54 percent for its main twenty-three cities in May 2009, which means that more than half their employees had an informal job. The informality rate for small cities was even higher, reaching 64 percent in 2010. At the same time, nonwage labor costs (that is, payroll taxes assumed by

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both the employee and the employer) represented more than 60 percent of the wage rate before 2012.[1]

Based on these facts, in 2013 Colombia implemented a reform of the tax code that substantially reduced payroll taxes. The main purpose of this tax reform was to promote the creation of formal jobs. The reduction of payroll taxes was expected to boost formal employment because it would reduce the costs 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 ten times the minimum wage and working in firms with at least two employees.

This paper adds evidence to the literature on the effects of nonwage costs, which provides mixed empirical results, by evaluating the effects of the 2012 Colombian tax reform on formal employment and the average wage paid by firms.[2] Using formal workers' administrative records, we specify and estimate equations for firms' labor demand and wages between January 2009 and December 2014. To take into account the heterogeneity of these effects for different types of firms, all the equations for five different samples were estimated according to the size of the firms before the implementation of the reform. We present the estimation results using the whole sample. To corroborate our findings, we estimate regressions aggregating the variables by combinations of municipality and economic sector, while also dividing the estimation sample according to the size of the firms.

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

1. Hernández (2012); Moller (2012).

2. Some of the international evidence for the United States and Latin American countries finds that payroll taxes increase labor costs and reduce wages (Gruber, 1994, 1997; MacIsaac and Rama, 1997; Edwards and Cox-Edwards, 2002; Marrufo, 2001; Heckman and Pagés, 2004; Mondino and Montoya, 2004; Kugler and Kugler, 2009; Cruces, Galiani, and Kidyba, 2010; Scherer, 2015), reduce employment (Kaestner, 1996; Heckman and Pagés, 2004; Kugler and Kugler, 2009; Scherer, 2015), and increase unemployment (Heckman and Pagés, 2004); while other evidence shows minor or no effects on employment (Gruber, 1994, 1997; Cruces, Galiani, and Kidyba, 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 Pagés, 2004).

of dealing with the endogeneity of our variables of interest. We perform a series of robustness checks, and the impacts obtained from different specifications and methodologies are broadly similar to the results of our preferred specifications.

The next section of this paper describes the 2012 tax code reform in detail. We then explore the literature related to the connection between payroll taxes and labor market outcomes. We subsequently outline our sources of information, empirical strategy, and methodology. After presenting our empirical results, we provide some robustness checks. In the last section, we draw conclusions and offer general policy implications.

The 2012 Colombian Tax Reform

Developing countries have made significant efforts to reduce the size of their informal labor market, given that it is usually characterized by the low productivity of informal firms, little or no protection for workers, and avoidance of the rule of law.[3] 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. Under the former definition, informal workers are not officially covered by the social security system. Under the latter, workers are considered informal if they work for a small firm (usually five employees or less) or are self-employed nonprofessionals.[4] 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.[5] Nevertheless, since 2009, the year in which the 2008 financial crisis had the greatest impact on the Colombian economy, both the unemployment rate and informality

3. See Medina, Núñez, and Tamayo (2013), Cárdenas and Mejía (2007), and López (2010) for evidence on the Colombian labor market.

4. Workers are officially considered informal in Colombia if they are employed in a nongovernmental firm of five or fewer employees or if they are self-employed with no college degree.

5. It has the second-highest estimated long-run unemployment rate out of nineteen Latin American and Caribbean countries (Ball, Roux, and Hofstetter, 2013), and it has one of the most informal economies in the region (Perry and others, 2007).

have declined substantially. The national unemployment rate decreased by more than three percentage points (see figure 1), and the informality rate in the twenty-three main cities declined by more than four percentage points (see figure 2). During the same period, the economy experienced an important boost in wage employment: the proportion of wage workers to the total working-age population of the country increased by almost five percentage points (see figure 3).

Figure 4 shows the total number of formal workers by type of firm and by firm size, based on the administrative records of employees contributing to the Colombian social security system.[6] Under our definition of formality, the number of formal workers has increased substantially since October 2008, the month the Integrated Record of Contributions to Social Security (PILA), our main source of information, began to be collected. The figure illustrates the total number of employees by firm size: almost 70 percent of the people employed work at firms with more than a hundred employees. Firms with more than 500 employees represent almost 50 percent of total formal employment, while firms with two to five employees represent a very small share of formal employment.

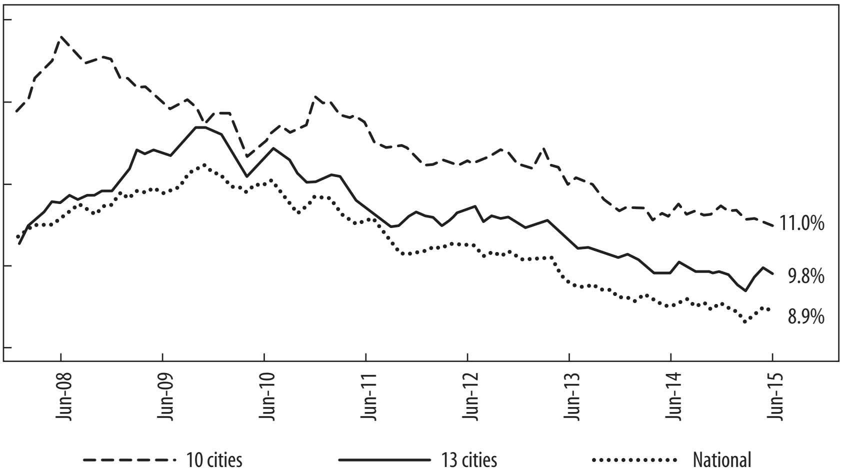
The implementation of the tax reform encompasses two periods: May 2013 to December 2013; and January 2014 onward. During the first period, eligible firms were exempted from paying five percentage points of their wages. In the second period, the reform was fully implemented, resulting in a 13.5-percentage-point reduction in payroll taxes for workers earning less than ten times the minimum wage and working for private, not-for-profit firms with at least two employees.[7] 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.

The decrease in the informality rate and the increase in the number of wage earners (figures 2 and 3) 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

6. The administrative records containing the information on employees contributing to the Colombian social security system are maintained by the Ministry of Health and Social Protection in its *Planilla Integrada de Liquidación de Aportes* (PILA).

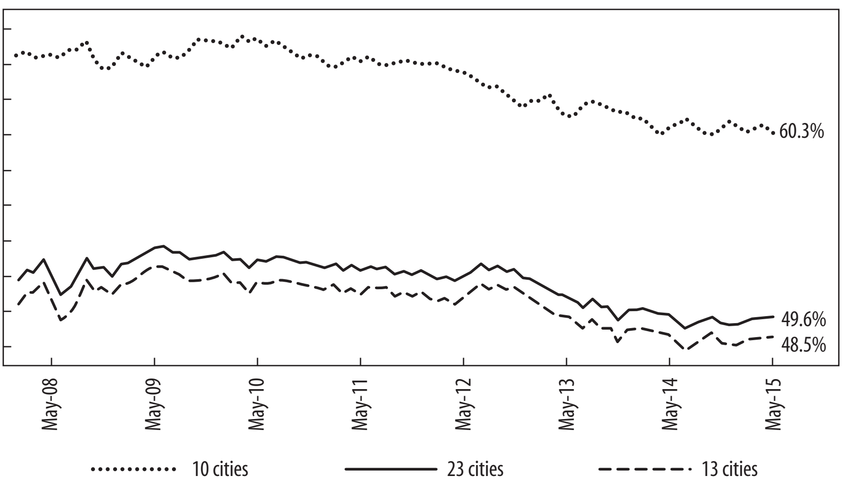
7. Act 1607 of 2012 and regulatory decree 0862 of 2013.

FIGURE 1. Unemployment Rate, January 2008 to June 2015[a]



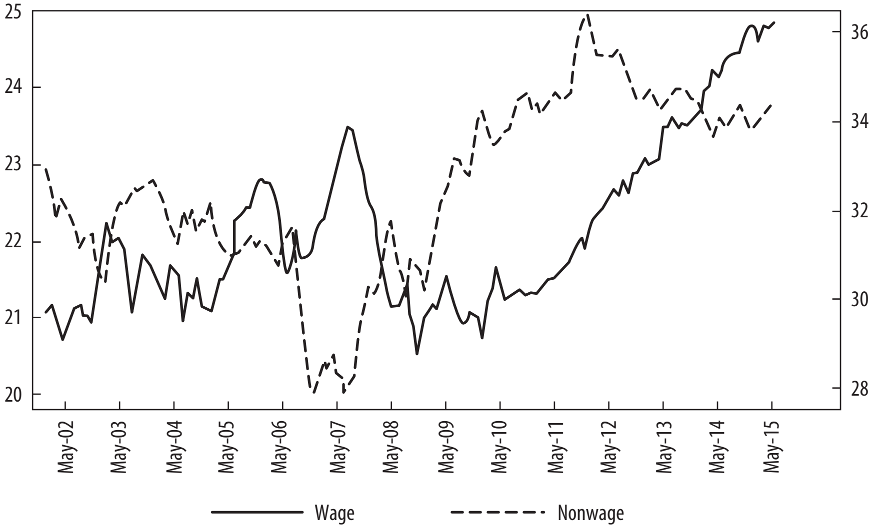
a. Seasonally adjusted; third-order moving average, or MA(3).

FIGURE 2. Informality Rate, January 2008 to June 2015[a]



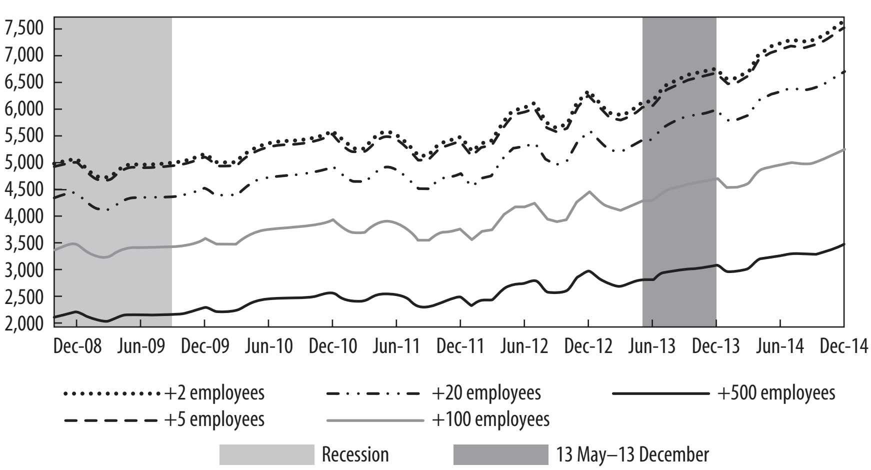
a. Seasonally adjusted; MA(3).

FIGURE 3. National Ratio of Employment to Working-Age Population: Wage and Nonwage Earners, January 2002 to May 2015[a]



a. Seasonally adjusted; MA(3).

FIGURE 4. Monthly Formal Employees, October 2008 to December 2014[a]



a. Includes only firms with two or more employees. Values are in thousands. Seasonally adjusted series; quarterly moving average.

TABLE 1. Prereform Nonwage Costs

Employers' contribution	*Percent of wage rate*
Pensions	16.0
Health care	12.5
Professional risks	2.0
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).

for a developing economy (25 percent at the national level). The large size of the informal sector has always been one of the top concerns in the Colombian labor market. There is a mainstream belief among labor economists that labor market rigidities and large nonwage costs create a breeding ground for informality.[8] Before 2013, the Colombian labor market had some of the highest nonwage costs in the region. Prior to the 2012 tax reform, payroll taxes represented 60 percent of the average wage rate.[9] The extra nonwage costs faced by employers included social security contributions (health and pension), transportation subsidies, and payroll taxes.

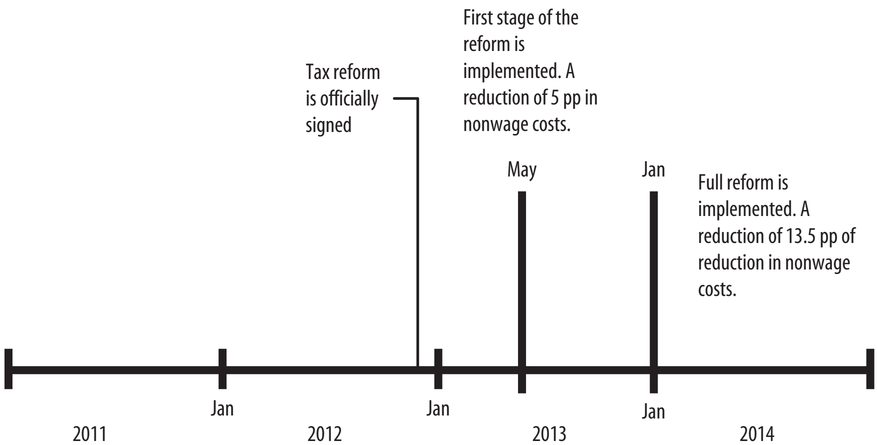
Table 1 represents the baseline scenario of the payroll tax component before the changes in the tax code. Nonwage costs were 60.3 percent of the wage, on average. The portion that the employer was obligated to pay by law totaled 52.3 percent of the wage (subtracting 4 percent from employee contributions for pensions and health insurance).[10] Under this scenario, the tax reform was proposed as a way to reduce labor costs and boost job creation, especially formal job creation. The changes to payroll taxes brought about by the 2012 tax reform eliminated the employer nonwage costs corresponding to contributions to health, job training programs (SENA), and childcare (ICBF), which

8. Bird and Smart (2012); Kugler and Kugler (2009); Sánchez, Duque, and Ruíz (2009); Santa María, García, and Mujica (2009); Peña (2013).

9. Santa María, Steiner, and Schutt (2010); Hernández (2012); Moller (2012).

10. The table does not include additional contributions such as the transportation subsidy for all employees earning up to two times the minimum wage (equivalent to about 11 percent of a minimum wage) or the interest on severance payments (equivalent to 12 percent of a monthly wage).

FIGURE 5. Timing of the 2012 Tax Code Reform



were 8.5 percent, 2.0 percent, and 3.0 percent of 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 ten times the minimum wage, 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 Congress in October 2012. The main objectives of this bill were to foster formal employment and enhance equity by making taxes more progressive and promoting the formalization of the labor market. The bill was approved in December 2012, but the reduction in payroll taxes was implemented in two stages. First, a five-percentage-point reduction in payroll taxes, corresponding to the SENA and ICBF contributions, was implemented in May 2013. Second, the employer's health contributions (8.5 percentage points) were eliminated in January 2014, for a total nonwage cost reduction of 13.5 percentage points of the wage rate.[11] These reductions only apply for employees whose wages are between one and ten times the minimum wage. Figure 5 summarizes the timing of the reform. The 2012 tax reform also introduced a new profit tax of 9 percent, known as the CREE tax, to replace the resources previously captured from wage taxes

11. Act 1607 of 2012 and Decree 0862 of 2013.

and contributions. At the same time, the Colombian income tax was reduced from 33 percent to 25 percent. In summary, the 2012 tax reform reduced taxes on wages and contributions by 13.5 percent, introduced a profit tax of 9 percent, and reduced the income tax by 8.0 percentage points. Government revenue declined as a result of the reform by about 0.2–0.5 percent of gross domestic product.[12]

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 nonwage 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 nonwage 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 sharply reduced payroll taxes for firms over a short period. This is important given that a firm's response can be asymmetric when it faces reductions or increases in nonwage costs.

Gruber assesses the effect of a 25-percentage-point reduction in payroll taxes in Chile that took place over a period of six years; he concludes that the incidence of this reduction took place entirely in wages and did not have any significant effect on employment.[13] Similarly, Gruber and Krueger, who consider the effect of disability insurance and maternity benefits, find no effects on employment, but rather a full wage shifting of employer contributions.[14] Some studies do find significant effects of payroll taxes on employment. Kaestner finds that an increase in the employer's cost of workers' compensation insurance significantly reduces employment for young adults and teenagers.[15] In addition, Kaestner finds that increases in insurance taxes reduce employment for teenagers.[16]

12. Fernández and Villar (2016).
13. Gruber (1994, 1997).
14. Gruber and Krueger (1991).
15. Kaestner (1996).
16. Hamermesh (2004) provides a survey of the findings on the effects of labor costs on labor demand in Latin American countries.

Among the studies that focus on Colombia, Kugler and Kugler examine the effect of a large increase in payroll taxes after a reform of the social security system in 1993.[17] They find negative and significant effects on employment and wages. Antón looks at the same question we are trying to answer in this study by examining the 2012 tax reform in Colombia to evaluate the effects of a fall in payroll taxes on employment and wages.[18] However, he uses a different methodology from the one in our study. Using a dynamic general equilibrium model, Antón finds that the reform would increase formal employment by 3.4 to 3.7 percent and formal wage rates by 4.9 percent.

Theoretical Effects of the Reform

Broadly speaking, the Colombian tax reform modified the income tax along with the payroll tax. It is therefore 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 shows that in the presence of those taxes plus 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.[19] 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 percent to 25 percent, while article 20 created the 8 percent income tax for equity (known as the CREE tax), which provisionally would be 9 percent in 2013, 2014, and 2015 (article 23). Although the CREE tax is somewhat different from the traditional income tax in terms of the 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 the previous income tax and the CREE, the total income tax paid by firms saw a rough increase from 33 percent to 34 percent beginning in 2013. This is about a 3.3 percent relative increase, which is smaller than the 0.135/1.6 = 8.4 percent relative decrease in total wage costs implied

17. Kugler and Kugler (2009).
18. Antón (2014).
19. Nickell (2004).

by the reduction in payroll taxes, but still significant.[20] 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, we use Gruber's approach with labor supply, $D = D[w(1 + \tau_f)]$; demand, $S = S[w(1 - a\tau_e) + qw\tau_f]$; and a simple production function, $F(L) = L^{\alpha}$; where w stands for the pretax wage, τ_f for the payroll tax rate on firms, and τ_e for the payroll tax rate on workers.[21] The expressions for the effect of payroll taxes on wages and labor then become:[22]

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

and

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

where a is the discount rate by which employees discount the benefits to which they have access through their payroll tax payments and q is how much they value the benefits to which they have access through 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, either labor supply is perfectly inelastic or labor demand is perfectly elastic, in which cases it is equal to $-1/(1 + \tau_f)$. In that case, there is no effect of payroll taxes on labor.

20. According to the National Tax and Customs Office (DIAN), the government collected 41.4 billion Colombian pesos from income tax and 14.5 billion pesos from CREE in 2015, that is, nearly 1.6 billion pesos per percentage point taxed in each of these cases. The amount collected in payroll taxes channeled to health insurance was 1.19 billion pesos in 2013 (in 2015 pesos) per percentage point contributed to health. Since workers earning more than ten times the minimum wage continued to contribute the 13.5 percentage points, the reduction in the amount of payroll taxes between 2013 and 2014 was only 6.77 billion pesos (in 2015 pesos), or 4.2 times the increase in the income tax.

21. Gruber (1997).

22. See also Gruber and Krueger (1991) and Kugler and Kugler (2009).

In practice, labor demand is not perfectly elastic and labor supply is not perfectly inelastic. In addition, while contributions to pensions or health insurance could be expected to be fully valued by employees, other contributions imposed in Colombia—such as those for childcare (three percentage points) or the family compensation fund (*cajas de compensación familiar,* four percentage points)—might be fully valued only by workers with children attending public childcare centers, who receive the monetary subsidy and frequently visit the family compensation funds' recreational centers.[23] Contributions to SENA (two percentage points), 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.

Finally, there is broad evidence that in Colombia, the minimum wage is binding. Thus, it is unlikely that payroll taxes could be transferred to wages at the low end of the wage distribution, but rather should directly affect employment.[24]

Data

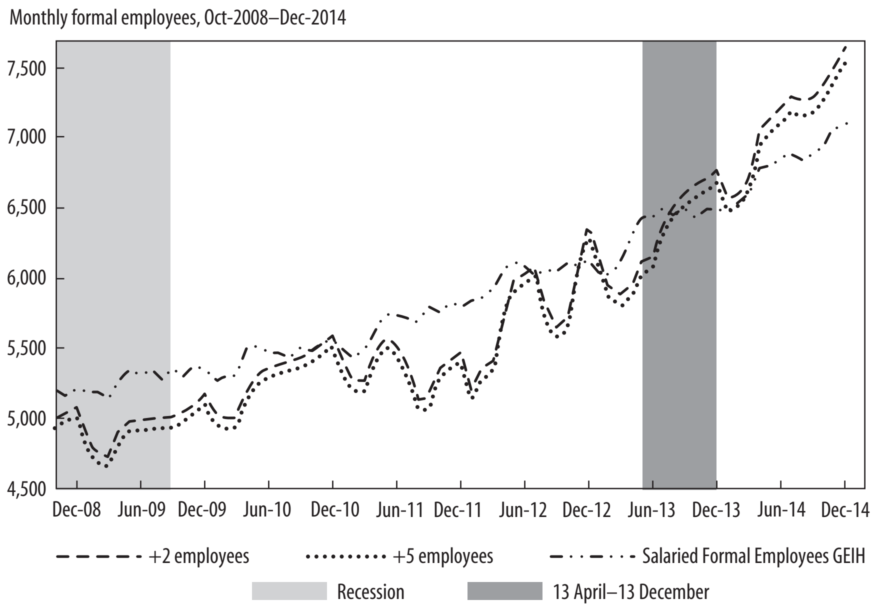
In this paper, we use firms' administrative records from the Colombian Ministry of Health and Social Protection (MHSP). Since 2008, Colombian firms have been required to report the social security payments for each of their workers. This system is known as the Integrated Record of Contributions to Social Security (PILA). 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, including wages, pension contributions, and health insurance payments, some basic demographic characteristics, and some basic characteristics

23. The monetary subsidy is a monthly transfer made by the family compensation funds to workers who earn no more than four times the minimum wage, work at least ninety-six hours a month, and earn jointly with their partners up to six times the minimum wage. The family compensation funds also offer other in-kind subsidies through scholarships, books, medications, and so forth.

24. See Bell (1997), Arango and Pachón (2004), Maloney and Núñez (2004), Kugler and Kugler (2009), and Heckman and Pagés (2004).

FIGURE 6. PILA Employees versus Official Salaried Formal Workers



of the firm. We use this information to 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 half of 2008, there may be underreporting, because it took a few months for some firms 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 began in May of 2013 and was fully completed by January 2014, the time frame of our data is adequate for assessing the policy.

To summarize, PILA is a census of all formal firms and all formal workers employed by these formal firms in Colombia. Figure 6 compares the total employment computed using PILA with total formal salaried employment using the official definition of formality from the Administrative Department of National Statistics (DANE). 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 the PILA and the GEIH should be relatively similar.

Figure 6 shows that formal salaried employment from these two sources is, in fact, 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.

Empirical Strategy

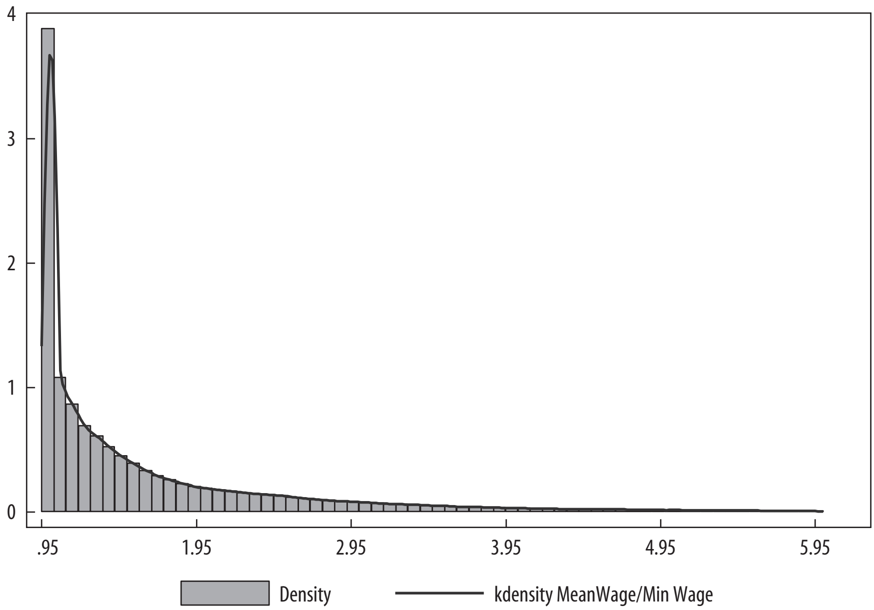
With the longitudinal information from the universe of all formal firms in Colombia, we estimate the effect of the reform on employment and wages using a linear regression strategy in a dynamic panel framework. In this paper, treatment consists of the reduction in payroll taxes due to the 2012 tax reform. 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 ten times the minimum wage (98 percent 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 for firms resulting from 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:

$$I_{j,t} = \sum\nolimits_{\forall i \in j, w_{i,j,t} < 10\text{MW}} w_{i,j,t} \circ \delta_t ,$$

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 (10MW). Finally, δ_t is the percentage reduction in nonwage costs mandated by the reform.

The reform affects virtually all firms homogeneously, and all these firms benefit from the same reduction in the payroll tax tariff. Nevertheless, the composition of the payroll is heterogeneous across firms, which guarantees that our measure of the intensity of treatment has enough variation. As figure 7 shows, average wages, expressed as a proportion of the minimum wage, are concentrated near the minimum wage, but the distribution of wages is moderately spread out. Figure 8 shows the average firm size by smaller bins of wages, where the average firm's employment increases for larger wages. This figure shows evidence on the important level of heterogeneity in firm size

FIGURE 7. Wage Distribution[a]

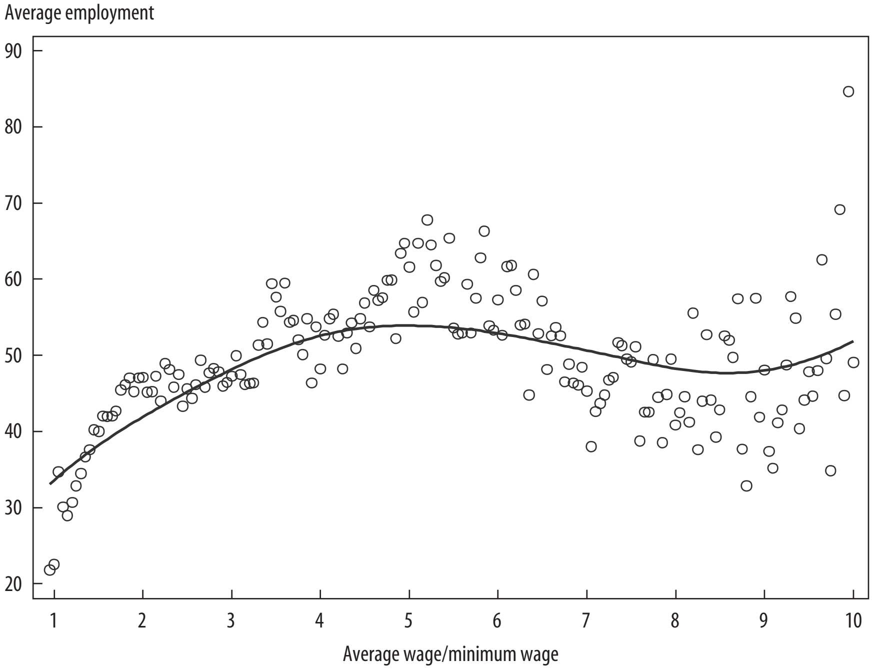


a. Density of average wages, expressed as multiples of the minimum wage.

by different values of average wages. Figure 9 shows a histogram of the variable: namely, the share of employment with wages greater than ten times the minimum wage. Around 85 percent of firms have no employees with wages greater than ten times the minimum wage, while the rest have a positive share of the payroll with these high wages. For instance, in more than 7 percent of the firms, this share is greater than 5 percent, and in almost 5 percent of the firms this share is greater than 10 percent. Wages, the firm's total employment, and thus the firm's payroll costs present substantial cross-sectional variation. All this is evidence that firms vary considerably in their payroll composition. Consequently, our intensity measure will also have substantial variation.

The effect of the reform is assumed to be heterogeneous for some firm characteristics, 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 the results tables, we present estimations with the entire sample, as well.

FIGURE 8. Wages and Employment[a]



a. Average employment corresponding to small windows of average wages (bandwidth = 0.01).

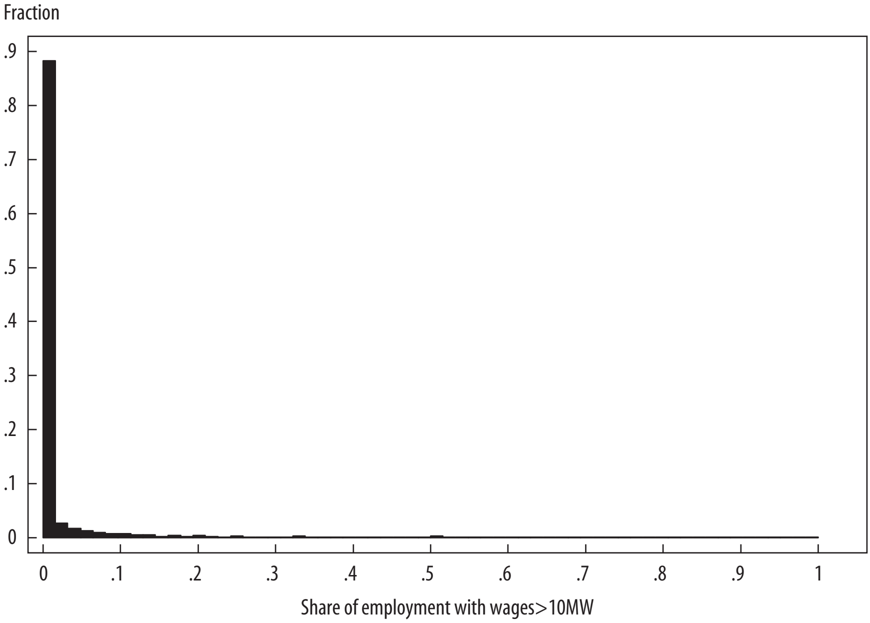
Intensity of the treatment is an endogenous variable because it depends on wages, which are simultaneously determined with employment. In addition, it is constructed for all the employees earning less than ten times the minimum wage, and it is therefore highly correlated with the variable we want to explain, $e_{j,t}$, We use two different strategies to circumvent the endogeneity problem: first, we estimate a modified version of the model that uses lagged wages and employment to obtain the intensity of treatment; second, we implement an instrumental variable approach.

Modified Model

In the modified version of the model, the treatment variable in period t is denoted by $I_{j,t}^{-12}$ and is defined as follows:

$$I_{j,t}^{-12} = \sum\nolimits_{\forall i \in j, w_{i,j,t} < 10\text{MW}} w_{i,j,t-12} \circ \delta_t, \quad (1)$$

FIGURE 9. Payrolls with Wages over Ten Times the Minimum Wage[a]



a. Distribution of the share of employment with wages greater than ten times the minimum wage in the estimation sample.

where δ_t is the percentage reduction in nonwage costs 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 twelve-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 1 May 2013, it is equal to 0.05 between 1 May and 31 December 2013, and it is equal to 0.135 beginning in January 2014.

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

$$(2) \qquad \ln\left(e_{j,t}\right) = \mathbf{x}'_{j,t-12}\beta_e + \alpha_e \circ \ln\left(e_{j,t-12}\right) + \rho_{e1} \ln\left(I_{j,t}^{-12}\right) + \rho_{e2} D_{2,j} \circ \ln\left(I_{j,t}^{-12}\right) + \sum_{s=0}^{2} \gamma_{es} \circ D_{s,j} + \tau_{e1} \circ \mathrm{TIME} + \tau_{e2} \circ \mathrm{TIME}^2 + \pi_{ej}^{Y} + \pi_{ej}^{M} + \varepsilon_{ej};$$

$$(3)\quad \ln\left(w_{j,t}\right) = \mathbf{x}'_{j,t-12}\beta_w + \alpha_w \circ \ln\left(w_{j,t-12}\right) + \rho_{w1}\ln\left(I^{-12}_{j,t}\right) + \rho_{w2}D_{2,j} \circ \ln\left(I^{-12}_{j,t}\right) + \sum_{s=0}^{2}\gamma_{ws} \circ D_{s,j} + \tau_{w1} \circ \text{TIME} + \tau_{w2} \circ \text{TIME}^2 + \pi^Y_{wj} + \pi^M_{wj} + \varepsilon_{wj};$$

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; $\mathbf{x}_{j,t-12}$ is a vector of a firm's characteristics the year before; and $e_{j,t-12}$ and $w_{j,t-12}$ are the firm's employment and average wage a year before, respectively. In addition, $\pi^Y_{.j}$ and $\pi^M_{.j}$ are yearly and monthly fixed effects, respectively. The regression includes three dummy variables: one dummy variable equal to one between 1 January 2009 and 30 April 2013 and zero otherwise, D_0; another equal to one between 1 May and 31 December 2013 and zero otherwise, D_1; and a final dummy variable equal to one after 1 January 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.

Instrumental Variable Approach

In addition to using the lagged treatment variable as in the modified model, we include the contemporaneous treatment, $I_{j,t} = \sum_{\forall i \in j, w_{i,j,t} < 10\text{MW}} w_{i,j,t} * \delta_t$, and implement an instrumental variable approach 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.[25]

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 $\mathbf{W}$ is generated where each element of $\mathbf{W}$, $\omega_{j,l}$,

25. 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 and Duggan (2003), Notowidigdo (2011), Diamond (2010), and Morales and Medina (2016). The methodology to construct the instruments resembles Morales (2015).

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 follows:

$$\text{(4)} \qquad \mathbf{W} = \begin{pmatrix} 0 & \omega_{1,2} & \cdots & \omega_{1,N} \\ \omega_{2,1} & 0 & & \omega_{2,N} \\ \vdots & & \ddots & \vdots \\ \omega_{1,N} & \omega_{2,N} & \cdots & 0 \end{pmatrix}$$

where $\omega_{j,l} = \dfrac{1}{\sqrt{\sum_{k=1}^{K} \left(c_{j,k} - c_{l,k}\right)^2}}$.

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 geographical 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 guarantees 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 ($\mathbf{I}_{l,t}^{-L}$). Let us call this vector $\mathbf{I}_t^{-L}$, which can be represented as follows:

$$\text{(5)} \qquad E_t = \mathbf{W}_l \circ I_t = \sum_{l=1}^{N} \omega_{j,l} \mathbf{I}_{l,t}^{-L}.$$

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, 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 $\mathbf{I}_{j,t}^{-6}$, $\mathbf{I}_{j,t}^{-12}$, and $\mathbf{I}_{j,t}^{2012}$ in equation 7. The variables $\mathbf{I}_{j,t}^{-12}$ and $\mathbf{I}_{j,t}^{-6}$ represent potential savings due to the reform generated using the previous year and previous half-year wage and employment, respectively. Similarly, $\mathbf{I}_{j,t}^{2012}$ 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 are used in the literature. In the applied industrial organization literature, differentiated product demand estimations usually use characteristics of other products as instruments for prices. The argument is that the degree of substitutability of a product will heavily influence its price.[26] In this branch of the literature, some studies use the prices of the same products in other markets as instruments for prices, while others use characteristics of other neighborhoods as instruments for dwelling price.[27] 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. To do this, several studies use as instruments characteristics of other entities outside of a particular entity's reference group, that is, of excluded peers.[28]

The construction of our exclusion restrictions is inspired by the literature mentioned in the previous paragraph. Our instruments $\mathbf{I}_{j,t}^{-6}$, $\mathbf{I}_{j,t}^{-12}$, and $\mathbf{I}_{j,t}^{2012}$ are weighted averages of lagged payroll cost functions from other firms.[29] The existence of a strong correlation between the intensity of the treatment and the exclusion restrictions is expected because, as mentioned 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 that we use firms from different economic sectors 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 characteristics of excluded peers. In addition, we use lags instead of current values of the information from these similar firms.

In specifications 2 and 3, there are two endogenous variables since the treatment intensity variable interacts with a dummy variable that is equal to

26. Berry, Levinsohn, and Pakes (1995).

27. On other markets, see Nevo (2001) and Hausman (1996); on other neighborhoods, see Bayer, McMillan, and Rueben (2004).

28. De Giorgi, Pellizzari, and Redaelli (2010); Bramoullé, Djebbari, and Fortin (2009); Morales, (2015).

29. We run two-stage least squares (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 their exogenous character, but we do not include them in our analysis in order to avoid a weak-instrument problem.

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. To properly identify coefficients ρ_1 and ρ_2, we follow a two-step regression procedure, where, in the first step, $I_{j,t}$ is regressed on all exogenous variables, including our three exclusion restriction variables, E_t^{-6}, E_t^{-12}, and E_t^{2012}.[30] From this regression, we obtain $\tilde{I}_{j,t}$, and, in a second stage, run the instrumental variable regression using $\tilde{I}_{j,t}$ and $\tilde{I}_{j,t} * D_{2,j}$ as instruments.[31] The model estimated in the second stage is identified exactly because there are two 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 overidentification. To test our instruments for this type of validity and to check the robustness, we estimate overidentified two-stage least squares (2SLS) models of equations 2 and 3, 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}, and E_t^{2012}, are used. The results of the overidentification tests and the treatment effects obtained from these models are presented in the robustness checks section of the paper.

Estimate with Aggregated Data

Our firms' 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 microdata.[32] In particular, means of employment, the intensity of treatment, and covariates are computed for each economic sector in a given municipality. There are around 1,100 municipalities in Colombia, and we use ten economic sectors: 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, based on aggregations by municipalities and economic sectors of the instruments we compute by firms.

30. Heckman and Vytlacil (1998).

31. This slight variation of the procedure presented in Heckman and Vytlacil (1998) is recommended by Wooldridge (2010) because it provides valid standard errors.

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

TABLE 2. Summary Statistics by Firms[a]

Variable	*No. observations*	*Mean*	*Standard deviation*
Employment	7,534,814	52.06	342.26
Real average wage	7,527,375	920,042	742,268
Private firm	7,534,814	0.97	0.18
Share of the payroll with wage ≤ 1 MW (*t*–12)	7,534,814	0.52	0.39
Share of the payroll with 1 MW < wage ≤ 2MW (*t*–12)	7,534,814	0.36	0.33
Share of the payroll with 3 MW < wage ≤ 5MW (*t*–12)	7,534,814	0.06	0.12
Share of the payroll with 5 MW < wage ≤ 10MW (*t*–12)	7,534,814	0.04	0.10
Share of payroll age 25 years or under	7,534,814	0.20	0.26
Share of payroll age 25 to 44 years (*t*–12)	7,534,814	0.55	0.24
Share of payroll age 45 to 59 years (*t*–12)	7,534,814	0.22	0.19
Share of males in the payroll (*t*–12)	7,532,700	0.61	0.28
Mining	7,532,895	0.03	0.17
Manufacturing	7,532,895	0.11	0.32
Electricity, gas, and water	7,532,895	0.00	0.06
Construction	7,532,895	0.09	0.29
Trade, hotels, and food services	7,532,895	0.22	0.42
Transportation, warehousing, and information	7,532,895	0.05	0.21
Finance services	7,532,895	0.05	0.22
Real estate, rental, and leasing services	7,532,895	0.24	0.43
Community, social, and personal services	7,532,895	0.15	0.35
$e_{t-12}\overline{W}_{t-12}\delta_t$	6,623,445	1,506,579	2,180,000
$e_t\overline{W}_t\delta_t$	7,534,814	1,696,867	2,380,000
$e_t\overline{W}_t\delta_t$ (Post-reform)	1,718,473	5,669,964	4,230,000
$e_{t-12}\overline{W}_{t-12}\delta_t$ (Post-reform)	2,600,129	4,809,446	4,010,000

a. Monetary variables are expressed in current Colombian pesos (COP).

Summary Statistics and Results

Table 2 presents the summary statistics of a sample of more than 7,500,000 period-firm observations. As mentioned, we only consider firms with more than two 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 fifty-two employees. The average wage is 920,000 Colombian pesos (COP) (around U.S.$300). In addition, 52 percent of the employees in these formal firms earn the minimum wage, 55 percent are between twenty-five and forty-four years old, and 61 percent are males. The great majority of the firms in the sample are private firms (97 percent), and they belong mostly to the following economic sectors: trade, hotels, and food services (22 percent); real estate and leasing services (24 percent); community, social, and personal services (15 percent); and manufacturing (9 percent).

The intensity-of-treatment variables are the potential savings in labor costs that the reform implies for firms. The current intensity of treatment,

$I_{j,t} = \Sigma_{\forall i \in j, w_{i,j,t} < 10mw} w_{i,j,t} * \delta_t$, 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 equals the monthly payment of more than six employees. The distribution of saving generated by the reform is highly spread out, with a variance of almost COP$8 million.[33] In addition, 95 percent of the firms in each month after the full implementation of the reform had payroll tax savings smaller than COP$15 million, while 75 percent of the firms had payroll tax savings, as a result of the reform, smaller than COP$2.35 million.

Results

We estimate equations 2 and 3 with the complete sample of firms. However, a different type of firm may be influenced differently by the reform, due to 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 the estimations by firm size. The sizes of the firms considered are two to five employees, six to twenty employees, twenty-one to 100 employees, 101 to 500 employees, and over 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 in which the intensity of the treatment is contemporaneous ($\mathbf{I}_{j,t} = \Sigma_{\forall i \in j, w_{i,j,t} < 10MW} w_{i,j,t} * \delta_t$), in which case our treatment variable is clearly endogenous, as previously explained. The results of this specification are expected to be biased upward.

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 levels. The evidence is mixed when it comes to average wages: for some firms, the effect is positive; for others, it is negative. The effects are summarized in tables 3 to 6. In these tables, the effect

33. For this calculation, we exclude from the sample values greater than the ninety-ninth percentile of the distribution.

TABLE 3. Intensity-of-Treatment Effects on Employment, by Firm Size[a]

Specification and variable	*All firms*	*No. employees*				
		2–5	*5–20*	*20–100*	*100–500*	*Over 500*
OLS: Intensity of treatment = $e_t\bar{W}_t\delta_t$						
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*)$*D_2$	−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)	—	1,633	26,521	63,464	97,956	229,516
Total effect	419,091					
One-year lag coefficient	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	—	5,150	77,775	170,603	238,336	572,360
Total long-run effect	1,064,225					
OLS: Intensity of treatment = $e_{t-12}\bar{W}_{t-12}\delta_t$						
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*)$*D_2$	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	5,417	15,789	37,537	70,152
Total effect	129,328					

One-year lag coefficient	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	—	1,476	19,348	50,767	103,122	222,002
Total long-run effect	396,716					
IV: Intensity of treatment $= e_t\bar{W}_t\delta_t$						
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)$*D_2$	−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	9,460	20,278	53,996	129,762
Total effect	213,929					
One-year lag coefficient	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	—	1,491	31,961	62,779	139,526	367,597
Total long-run effect	603,354					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

a. 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}, and 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 errors are clustered by firm. Total effects were computed using only statistically significant elasticities in each regression by firm size.

TABLE 4. Intensity-of-Treatment Effects on Employment, by Municipality and Economic Sector[a]

Specification and variable	*All firms*	*No. employees* 2–5	5–20	20–100	100–500	Over 500
OLS: Intensity of treatment $= e_t\bar{W}_t\delta_t$						
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)$*D_2$	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)	—	3,676	30,403	63,619	95,347	167,068
Total effect	360,113					
One-year lag coefficient	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	—	9,548	74,334	167,419	220,200	362,404
Total long-run effect	833,906					
OLS: Intensity of treatment $= e_{t-12}\bar{W}_{t-12}\delta_t$						
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)$*D_2$	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)	—	1,546	12,129	21,516	44,562	53,932
Total effect	133,685					

One-year lag coefficient	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	—	4,355	32,869	62,912	113,679	130,587
Total long-run effect	344,402					
IV: Intensity of treatment $= e_t \bar{W}_t \delta_t$						
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*)$*D_2$	−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)	—	4,801	17,061	31,577	52,190	119,624
Total effect	225,253					
One-year lag coefficient	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	—	11,269	42,022	87,715	129,825	267,018
Total long-run effect	537,850					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

a. 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}, and 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 errors are clustered by firm. Total effects were computed using only statistically significant elasticities in each regression by firm size.

TABLE 5. Intensity of Treatment Effects on Average Wages, by Firm Size[a]

Specification and variable	All firms	No. employees 2–5	5–20	20–100	100–500	Over 500
OLS: Intensity of treatment = $e_t \bar{W}_t \delta_t$						
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*)∗D_2	−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%					
One-year lag coefficient	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%
Total long-run effect	0.082%					
OLS: Intensity of treatment = $e_{t-12} \bar{W}_{t-12} \delta_t$						
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*)∗D_2	−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%					

One-year lag coefficient	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%
Total long-run effect	−0.026%					
IV: Intensity of treatment $= e_t \bar{W}_t \delta_t$						
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*)$*D_2$	−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%					
One-year lag coefficient	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%
Total long-run effect	1.067%					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

a. 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}, and 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 indicates the total employment for each size of firm. Robust standard errors are clustered by firm. Total effects were computed using only statistically significant elasticities in each regression by firm size.

TABLE 6. Intensity of Treatment Effects on Wages, by Municipality and Economic Sector[a]

		No. employees				
Specification and variable	*All firms*	*2–5*	*5–20*	*20–100*	*100–500*	*Over 500*
OLS: Intensity of treatment $= e_t\bar{W}_t\delta_t$						
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*)∗D_2	–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%					
One-year lag coefficient	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%
Total long-run effect	0.724%					
OLS: Intensity of treatment $= e_{t-12}\bar{W}_{t-12}\delta_t$						
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*)∗D_2	–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%					

One-year lag coefficient	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%
Total long-run effect	0.229%					
IV: Intensity of treatment $= e_t \bar{W}_t \delta_t$						
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)$*D_2$	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%					
One-year lag coefficient	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%
Total long-run effect	1.838%					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

a. 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}, and 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 indicates the total employment for each size of firm. Robust standard errors are clustered by firm. Total effects were computed using only statistically significant elasticities in each regression by firm size.

of the intensity of treatment is translated into employment and average wage impacts for each category of firm size. Tables 3 and 4 present the employment effects by firm size and by municipality and economic sector, respectively, while tables 5 and 6 present the respective results for wages. Each of these tables contains three panels: the first and second panels present the ordinary least squares (OLS) estimates of the contemporaneous and lagged treatment, respectively; the last panel presents the instrumental variable estimates of the effects that use the contemporaneous treatment. In all regressions we control for city fixed effects, month and year fixed effects, and a quadratic trend. In the firm-level estimates, the fit of the regressions is quite good. In almost all regressions, the adjusted *R* squared is higher than 55 percent in the employment regressions, and it is greater than 90 percent in the case of wages. Similar fits are obtained for the regressions with aggregated data.

In the case of the employment regressions, most of the firm size categories display a positive and significant quadratic trend, and the one-year lag for employment is important for explaining current employment, as expected. 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. With regard 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 forty-four years of age for firms with up to a hundred employees. Finally, the share of males on the payroll is positively correlated with employment in firms with up to twenty employees. However, this correlation becomes negative for larger firms, in particular very large firms (over 500 employees).

In the average wage regressions for firms, we find that the share of employees who have been on the payroll for twenty-five years or less 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 twenty 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* squared is above 90 percent.

EFFECTS OF THE REFORM ON EMPLOYMENT. Table 3 presents the employment effects of the reform for the whole sample and for the different firm sizes that were identified from estimating equation 2 using firm data. 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 of the intensity of treatment and the interaction of this variable with a dummy variable that is equal to one after December 2013 (full implementation period), that is, $\rho_1 + \rho_2$ 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 of treatment, shows that a one percent reduction in nonwage costs as a result of the tax reform increases a firm's employment of workers by 0.039 percent, 0.067 percent, 0.10 percent, 0.19 percent, and 0.17 percent for very small, small, medium, large, and very large firms, respectively. The elasticity obtained from the estimation with the complete sample is 0.09 percent, which is close to the weighted average of the individual elasticities. The elasticities for the OLS model using contemporaneous intensity of treatment are 0.16 percent, 0.31 percent, 0.39 percent, 0.47 percent, and 0.52 percent for very small, small, medium, large, and very large firms, respectively, while the elasticity obtained from the estimation with the complete sample is 0.34 percent. The contemporaneous intensity of treatment is endogenous; therefore, the effects of the reform are expected to be overestimated.[34] However, the effects of the reform can be underestimated using the lagged intensity-of-treatment variable because the reform could cause savings in labor costs that are not captured 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 percent, 0.12 percent, 0.13 percent, 0.27 percent, and 0.32 percent for very small, small, medium, large, and very large firms, respectively. As before, the elasticity obtained from the estimation with the complete sample, 0.16 percent, 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 variable are substantially higher than those based on the OLS models with lagged intensity of treatment: the former

34. For instance, in the case of regressions with employment as the dependent variable, OLS bias is expected to be positive because treatment intensity is a function of employment. The error term in equation 2 may be expressed as an increasing function of employment, so the correlation between this error and employment will be positive, and the OLS bias would thus be positive.

estimates a total of 419,000 jobs created in the short run as a result of the reform, versus 129,000 jobs for the latter. Our preferred estimate is the IV model. As described earlier, we are following a procedure based on Heckman and Vytlacil, in which the exclusion restrictions for a specific firm are weighted averages of the intensity of treatment in other firms that are similar to this specific firm along several dimensions.[35] The employment effect of the reform with the IV model is 213,000 new jobs created in the short run.[36] The number of jobs resulting from the reform is computed from the elasticity identified in the regression times 13.5 percent, which is the potential savings on labor costs due to the reform. The lagged dependent variable allows us to estimate the effect in the long run. According to our IV estimates, by December 2015, 2016, and 2018, there could be 365,000, 473,000, and 534,000 new employees, respectively, and in the long run, there could be 603,000 new employees as a result of the reform. The regressions in table 3 do not control for the plausible endogeneity of the one-year lag of the dependent variable. However, when we control for this additional endogenous variable in a robustness check (discussed below), the impacts of the reform in the short and long runs are comparable to the estimates in table 3. These long-term effects are similar to those found by Fernández and Villar and are within the range of effects found in studies they cite, which range between 145,000 and 700,000 new formal jobs.[37]

The effect of the reform is heterogeneous across firm sizes, with higher elasticities for large firms. Regarding increases in employment, bigger firms contribute more to the new jobs generated by the reform: firms with more than 500 employees are estimated to contribute almost 130,000 jobs in the short run and 367,000 in the long run, which is more than 60 percent of the total estimated effect.

As table 4 shows, the results of the estimates using the aggregated sample by municipality, economic sector, and firm size are quite similar to the results based only on firms. Nevertheless, the effect on employment obtained from the IV estimate, which is our preferred specification, is a bit greater than the IV results using microdata from firms. The overall number of jobs created

35. Heckman and Vytlacil (1998).

36. The total aggregate impact is computed from the elasticities obtained from the regression for different firm sizes.

37. Fernández and Villar (2016). Bear in mind that these authors use a different definition of formality based on the firm size (workers in firms with more than five employees) and including self-employed professionals and technicians. The articles cited by these authors include Kugler and Kugler (2015) and Bernal, Eslava, and Meléndez (2015).

due to the reform in the aggregated version of equation 2 is 225,000 jobs in the short run. As in the estimate based on microdata from firms, the effect of the reform is heterogeneous across firm sizes, and bigger firms account for the largest share of the new jobs generated by the reform. Together, firms with 100–500 employees and firms with over 500 employees contribute more than 170,000 of the 231,000 estimated in equation 2 in the case of aggregated data.

EFFECTS OF THE REFORM ON WAGES. Table 5 presents the average wage effects of the reform based on different firm sizes, using firms' microdata. In some of the IV regressions, there is a positive and very small effect on average wages, with total elasticities below 1 percent. This is the case for large firms (100–500 employees), medium-sized firms (20–100 employees), and small firms (5–20 employees), where tax reform has a slight positive effect on wages. On aggregate, 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 percent, which is the potential savings on labor costs due to the reform. The overall effect of the reform in the short run (weighting the effect of each firm size by the category's share in total employment) is positive but small, with an increase of 0.13 percent in average wages. This effect rises in the long run, although it is still small (around 1 percent). When we control for plausible endogeneity of the one-year lag of the dependent variable, the short- and long-run impacts of the reform are comparable to the results presented in table 5 (see the next section).

The results from the regressions using aggregated data by municipality and economic sector are similar to the results based on firm microdata. For some of the firm sizes, the reform has no significant effect. However, the reform does have a positive effect on wages in small firms (two to five employees), and large firms (100–500 employees), and very large firms (over 500 employees). In general, the overall effect of the entire reform, in the short run, is a rise of 0.5 percent in average wages. This is larger than the effect computed with firm regressions, but it is still small.

Robustness Checks

To check the robustness of our results, we estimated different specifications of our econometric models. These different approaches allow additional testing of the validity of our instruments and offer an alternative for dealing with the endogeneity of the treatment-intensity variable. Finally, we present a robustness check dealing with the plausible endogeneity of lags of the dependent variable in the regressions.

Instrument Validity

Overidentified 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 overidentifying restrictions.[38] This provides an indication of the validity of our instruments. Formally, the overidentifying restrictions test is a test of the independence of additional instruments from 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.

Tables 7 through 10 present the estimated effects from the regressions of overidentified models for firms and for municipality-economic sectors, together with the results of exactly identified models using the same specification (without interactions). In the latter cases, the strongest instrument, E_t^{-6}, is used, as it could potentially be the most endogenous of the three. In general, the effects computed from models without the interaction term $D_{2,j} \cdot ln(I_{j,t})$ are similar to the effects computed using our preferred specification.

In the case of the firm-level regressions, most of the estimates do not reject the overidentification hypothesis at the 95 percent confidence level. The overidentification hypothesis is rejected in only one regression, namely, the wage regression for the second group. As shown in the tables, the effects computed from the overidentified models with valid instruments (that is, where the overidentified 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 municipality-sector regressions, the overidentification hypothesis is rejected in some regressions (the firms with two to five employees and five to twenty employees), 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

38. The overidentifying restriction test is obtained as $N \cdot Ru^2$, where N and Ru 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). $N \cdot Ru^2$ is distributed $\chi 2$ with degrees of freedom equal to the number of overidentifying restrictions. The null hypothesis of this test is the exogeneity of the instruments. Mathematically, $H0: E(\mathbf{Z}'u) = 0$.

TABLE 7. Instrument Robustness Checks: Effects on Employment, by Firm Size[a]

		No. employees				
Model and parameter	*All firms*	*2–5*	*5–20*	*20–100*	*100–500*	*Over 500*
Overidentified model						
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	14,291.002	5,622.278	209.579	905.161
Sargan test (*p* value)		0.785	0.037	0.258	0.027	0.053
Effect (13.5*Elasticity)		0	11,320	23,373	42,354	118,002
Total effect	195,050					
One-year lag coefficient		0.713	0.703	0.676	0.629	0.648
		(0.015)	(0.006)	(0.006)	(0.013)	(0.019)
Long-run effect		—	38,115	72,140	114,162	335,233
Total long-run effect	559,650					
Exactly identified model						
β		0.005	0.141	0.151	0.170	0.295
σ		(0.087)	(0.016)	(0.021)	(0.166)	(0.045)
F		393.6	40,444.3	16,535.2	228.3	2,557.3
Effect (13.5*Elasticity)		54	11,401	23,373	34,124	119,624
Total effect	188,577					
One-year lag coefficient		0.712	0.703	0.676	0.636	0.648
		(0.020)	(0.006)	(0.006)	(0.026)	(0.019)
Long-run effect		188	383,87	72,140	93,724	339,841
Total long-run effect	544,281					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.
a. In overidentified models, E_T^{-6}, E_T^{-12}, and 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 errors, clustered by firm, are in parentheses.

the municipality-sector level or whether the models are exactly identified or overidentified. Considering the effects computed from all IV regressions, we can say that the effect of the reform is not larger that 277,000 and not smaller than 188,000 new formal jobs in the short run and is between 600,000 and 657,000 in the long run, while the effect on wages, in the short run, is substantially smaller than 1 percent in every case.

Alternative Methods for Dealing with Endogeneity

We estimated equations 2 and 3 using a fixed-effect panel estimation technique, in order to eliminate permanent unobserved heterogeneity of firms from the error term of the equations, which may be the source of endogeneity

TABLE 8. Instrument Robustness Checks: Effects on Real Wages, by Firm Size[a]

Model and parameter	All firms	No. employees 2–5	5–20	20–100	100–500	Over 500
Overidentified model						
β		−0.011	0.037	0.009	0.117	0.008
σ		(0.022)	(0.008)	(0.007)	(0.054)	(0.008)
F		401.597	11,844.756	5,553.409	82.045	875.724
Sargan test (*p* value)		0.938	0.013	0.078	0.066	0.052
Effect (13.5∗Elasticity)		−0.149%	0.500%	0.122%	1.580%	0.108%
Total effect	0.491%					
One-year lag coefficient		0.970	0.910	0.910	0.850	0.930
		(0.012)	(0.005)	(0.006)	(0.029)	(0.020)
Long-run effect		−4.950%	5.550%	1.350%	10.530%	1.543%
Total long-run effect	3.921%					
Exactly identified model						
β		0.011	0.030	0.010	0.112	0.009
σ		(0.045)	(0.006)	(0.007)	(0.051)	(0.008)
F		397.451	39,771.115	16,350.341	226.205	2,469.556
Effect (13.5∗Elasticity)		0.149%	0.405%	0.135%	1.512%	0.122%
Total effect	0.479%					
One-year lag coefficient		0.967	0.915	0.916	0.852	0.935
		(0.016)	(0.005)	(0.006)	(0.028)	(0.020)
Long-run effect		4.500%	4.765%	1.607%	10.216%	1.869%
Total long-run effect	4.095%					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. In overidentified models, E_T^{-6}, E_T^{-12}, and 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 errors, clustered by firm, are in parentheses.

TABLE 9. Instrument Robustness Checks: Effects on Employment, by Municipality and Economic Sector[a]

		No. employees				
Model and parameter	*All firms*	*2–5*	*5–20*	*20–100*	*100–500*	*Over 500*
Overidentified model						
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 (13.5*Elasticity)		9,525	34,122	43,806	47,172	135,033
Total effect	269,658					
One-year lag coefficient		0.543	0.584	0.637	0.599	0.550
		(0.041)	(0.019)	(0.009)	(0.020)	(0.036)
Long-run effect		20,843	82,024	120,677	117,635	300,074
Total long-run effect	641,254					
Exactly identified model						
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 (13.5*Elasticity)		11,428	35,092	44,270	48,978	137,872
Total effect	277,641					
One-year lag coefficient		0.519	0.582	0.637	0.598	0.549
		(0.052)	(0.019)	(0.009)	(0.021)	(0.036)
Long-run effect		23,759	83,953	121,957	121,836	305,703
Total long-run effect	657,208					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.
a. In overidentified models, E_T^{-6}, E_T^{-12}, and 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 errors, clustered by firm, are in parentheses.

bias. One limitation of using firm fixed effects is that it cannot control for the existence of the firms' nonpermanent unobserved heterogeneity, but it is still worthwhile as a robustness check.[39]

The results of including firm fixed effects in the estimation of equations 2 and 3 are presented in table 11. From this estimate, the total number of jobs created due to the tax reform is almost 212,000. This magnitude of formal

39. We also performed instrumental-variable fixed-effect regressions of equations 2 and 3. Unfortunately, the properties of the set of available instruments are not as desirable as they are in the case of our preferred specification, so we decided not to present them in this manuscript. Nevertheless, using the best instruments available, we obtained a short-run impact of 174,000 formal jobs created by the tax reform and of an economically negligible effect on wages.

TABLE 10. Instrument Robustness Checks: Effects on Real Wages, by Municipality and Economic Sector[a]

Model and parameter	All firms	No. employees 2–5	5–20	20–100	100–500	Over 500
Overidentified model						
β		0.126	0.012	0.012	0.025	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 (13.5∗Elasticity)		1.701%	0.162%	0.162%	0.338%	1.323%
Total effect	0.775%					
One-year lag coefficient		0.799	0.756	0.814	0.709	0.731
		(0.026)	(0.020)	(0.020)	(0.044)	(0.045)
Long-run effect		8.463%	0.664%	0.871%	1.160%	4.918%
Total long-run effect	2.940%					
Exactly identified model						
β		−0.490	−0.017	0.020	0.017	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 (13.5∗Elasticity)		−6.615%	−0.230%	0.270%	0.230%	1.337%
Total effect	0.633%					
One-year lag coefficient		0.914	0.762	0.813	0.710	0.731
		(0.061)	(0.022)	(0.020)	(0.044)	(0.046)
Long-run effect		−76.919%	−0.964%	1.444%	0.791%	4.968%
Total long-run effect	1.744%					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.
a. In overidentified models, E_T^{-6}, E_T^{-12}, and 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 errors, clustered by firm, are in parentheses.

TABLE 11. Panel Regression Estimates with Firm Fixed Effects[a]

Dependent and explanatory variables	*All firms*	*No. employees*				
		2–5	*5–20*	*20–100*	*100–500*	*Over 500*
Dependent variable: Employment						
Log(*I*)		0.006	0.003	0.001	0.000	0.002
		(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Log(*I*)∗D_2		0.060	0.119	0.161	0.247	0.312
		(0.003)	(0.001)	(0.002)	(0.003)	(0.007)
Elasticity		0.066	0.122	0.162	0.247	0.314
		(0.003)	(0.001)	(0.002)	(0.003)	(0.007)
Effect (13.5∗Elasticity)		714	9,865	25,076	49,580	127,329
Total effect	212,564					
One-year lag coefficient		0.330	0.287	0.347	0.307	0.272
		(0.007)	(0.001)	(0.002)	(0.003)	(0.006)
Long-run effect		1,065	13,835	38,401	71,544	174,902
Total long-run effect	299,749					
Dependent variable: Real wages						
Log(*I*)		−0.001	0.000	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(*I*)∗D_2		−0.009	0.010	−0.001	−0.004	−0.003
		(0.002)	(0.000)	(0.001)	(0.001)	(0.001)
Elasticity		−0.010	0.010	−0.001	−0.004	−0.003
		(0.002)	(0.000)	(0.000)	(0.000)	(0.001)
Effect (13.5∗Elasticity)		−0.135%	0.135%	−0.014%	−0.054%	−0.041%
Total effect	−0.023%					
One-year lag coefficient		0.300	0.323	0.319	0.369	0.409
		(0.010)	(0.002)	(0.002)	(0.004)	(0.010)
Long-run effect		−0.193%	0.199%	−0.020%	−0.086%	−0.069%
Total long-run effect	−0.040%					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

a. 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 errors, clustered by firm, are in parentheses.

job creation is very similar to the level computed using our microdata 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. We do not use fixed effects to interpret the long-run impact 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-effects transformations. We deal with possible endogeneity of this variable in the next robustness check.

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 estimation of dynamic models is the endogeneity of lagged dependent variables. To make sure that the estimated impacts are not biased by this issue, we estimated specifications of equations 2 and 3 in which the one-year lag of the dependent variable is treated as an endogenous variable as well. This increases the number of endogenous variables by one. To estimate these models, we follow Arellano and Bond in controlling for this additional endogeneity problem.[40] As additional instruments, we use high-order lags (twenty-four months or higher) of the dependent variable and its first difference, as well as high-order lags of all exogenous covariates.

The results of this additional robustness check are presented in table 12. From this estimate, the total number of jobs created due to the tax reform in the short and long runs is almost 174,000 and 668,000, 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 under our preferred specification, but in a broader sense, the impact of the reform is comparable using both specifications. 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 and in the long run.

40. Arellano and Bond (1991).

TABLE 12. Arellano-Bond Robustness Check[a]

Dependent and explanatory variables	*All firms*	*No. employees* 2–5	5–20	20–100	100–500	Over 500
Dependent variable: Employment						
Log(*I*)		0.222	0.244	0.200	−0.007	0.203
		(0.096)	(0.023)	(0.031)	(0.138)	(0.075)
Log(*I*)∗D_2		−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	10,673	16,408	35,128	111,920
Total effect	174,723					
One-year lag coefficient		0.611	0.700	0.700	0.683	0.760
		(0.032)	(0.017)	(0.012)	(0.027)	(0.033)
Long-run effect		1,529	35,577	54,693	110,813	466,332
Total long-run effect	668,944					
Dependent variable: Real wages						
Log(*I*)		−0.014	0.042	0.030	0.041	−0.002
		(0.068)	(0.048)	(0.037)	(0.041)	(0.038)
Log(*I*)∗D_2		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%					
One-year lag coefficient		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%
Total long-run effect	1.044%					
Employment	6,316,272	80,089	598,947	1,146,600	1,486,889	3,003,746

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.
a. Total effects were computed using only statistically significant elasticities in each regression by firm size. The last row of the table (employment) indicates the total employment for each firm size. Robust standard errors, clustered by firm, are in parentheses.

Additional Effects and Treatments

We estimated equations 1 and 2 using a different set of dependent variables, mainly average age and the share of the payroll receiving less than 1.5 times the minimum wage. These regressions are estimated using instrumental variables, with the same instruments used for the main employment and wage outcomes. This evidence deserves its own comments.[41]

First, 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 percent of the average age of the employees on the payroll in the short run as a result of the tax reform. The reform thus induced firms to hire young workers to fill vacant, newly created jobs. This trend, however, also reflects a previous policy implemented in Colombia (Law 1429 of 2010), which is focused on the creation of formal jobs for young workers.[42] Second, the share of workers in medium-sized and large firms with wages less than or equal to 1.5 times the minimum wage increased significantly as a result of the reform. 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 percent in the share

41. In an additional set of regressions, we estimated equations 1 and 2 including a variable that describes the percentage of workers with earnings below the minimum wage during the three-month period before firm employment was observed. Neumark, Cunninham, and Siga (2006) use this approach to capture the effect of the minimum wage on employment. This regression is run in an instrumental-variable setting where the endogenous variables are $\ln(\mathbf{I}_{j,t})$ and $D_{2,j} \cdot \ln(\mathbf{I}_{j,t})$, 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 percent. 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 computed effect of the tax reform is unaffected by any effect that the minimum wage had on employment during those same years.

42. Prior to implementing the 2012 tax reform, Colombia had enacted what is now commonly known as the fist employment law (law 1429 of 2010), with the same purpose of reducing labor market informality. In broad terms, this law reduced contributions to SENA, ICBF, and the family compensation funds (four percentage points paid by employers), over a period of no more than six years, for young workers, workers with no experience in the labor market, and workers with wages below 1.5 times the minimum wage, and some other small groups. This intervention was in place for almost three years before the implementation of the 2012 tax reform. Since our treatment-intensity variable captures the change in payroll savings after the implementation of the 2012 reform, the effect we capture is plausibly clean of any effect from Law 1429. If anything, the effect we estimate from the tax reform would be a lower bound: because some of the benefits of 1429 were replaced by the 2012 reform, the later reform may have had a discouraging effect in terms of job creation within the set of firms with a larger share of employees earning less than ten times the minimum wage, resulting in a negative effect on our treatment group.

of employees with wages lower than 1.5 times the minimum wage, which is statistically significant, but very small.

Conclusions

The Colombian labor market is characterized by high levels of unemployment and informality by the standards of the Latin American region. Colombia also has some of the highest nonwage costs in the region. Before the 2012 tax code reform, nonwage labor costs accounted for more than 60 percent of the wage rate.[43] To foster the creation of 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 the regressions, our treatment variable is the total amount of payroll savings generated as a result of the policy change. In all cases, our regressions were estimated using microdata broken down by firm size and aggregated data by municipality and economic sector.

We find a positive effect of the 2012 tax reform on firm employment. Our estimates reveal that the effect of the reform is positive for all firm sizes, but it is greatest for larger firms: firms with more than 100 employees account for more than 80 percent of the total effect. According to our preferred regression model (instrumental variables), the reform resulted in the creation of between 213,000 and 225,000 formal jobs in the short run and between 540,000 and 603,000 in the long run, depending on whether we use microdata 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 percent. In general, the average effect of the 2012 tax reform is small across all firm sizes. Our findings identify the effect of the entire reform, in the short run, as an increment of 0.12 percent and 0.42 percent in average wages for regressions with microdata and aggregated data, respectively. Small effects in the short and long runs are also found 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. We therefore conclude that the economic impact of the reform was rather small.

43. Hernández (2012); Moller (2012).

Based on the figures for total taxes collected in the health sector between 2013 and 2014, contributions to health went from 12.5 percentage points of wages to about 8.5 percentage points. Since one percentage point represents about COP$1 billion per year in contributions, total savings by firms would amount to about COP$6.35 billion a year. This, divided by 567,000 new employees, would be about COP$11.2 million a year per employee, which is roughly the cost of paying an average worker's base wage for one year. Given that nonwage costs were reduced from 1.600 to 1.465 times the base wage, employers would be investing about 40 percent of the total cost of employing the new employees generated by the reform, while their new savings would cover the remaining 60 percent.

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