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Article:

Bernal, Raquel, Meléndez, Marcela, Eslava, Marcela and Pinzón, Alvaro (2017) Switching from payroll taxes to corporate income taxes: firms' employment and wages after the 2012 Colombian tax reform. Economía, 18 (1). 41 - 74. ISSN 1529-7470

10.31389/eco.51

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Switching from Payroll Taxes to Corporate Income Taxes: Firms' Employment and Wages after the 2012 Colombian Tax Reform

ABSTRACT The 2012 Colombian tax reform reduced payroll taxes and employer contributions to health insurance by 13.5 percent, while also increasing corporate income taxes and leaving untouched the benefits to workers financed through these taxes. Shifting taxation from formal employment to other business activities is a policy recipe under heated discussion in Latin America. The reform offers an ideal laboratory for studying empirically the potential distortions against formal employment associated with payroll taxes in contrast to other taxes on firms. We analyze the impact of the reform on employment and wages using monthly firm-level data on all formal employment in nonpublic firms in the country and a difference-in-differences approach that takes advantage of the fact that a few sectors were exempt from the 2012 tax reform. We find a positive average effect of 4.3 percent on employment and 2.7 percent on average firm wages, for the average firm. The employment effect is found only for micro and small firms, whereas the bulk of the employment is concentrated in medium and large firms, which show no significant effect. According to these estimates, about 145,000 new jobs were created between January and May of 2015 by virtue of the reform. These results are generally supportive of efforts to reduce payroll taxes, though our findings on employment are less robust than those on wages, and large firms do not seem to have benefitted. The apparent lack of effect for medium and large employers is also a source of concern. We speculate that it may be due to these firms' being more sensitive to the increase in corporate taxation that financed the reduction in payroll taxes, but lack of access to the relevant data prevents us from offering solid evidence regarding this hypothesis.

JEL Codes: H25, H32

Keywords: Payroll taxes, tax reform, employment, Colombia

ACKNOWLEDGMENTS The authors thank Laura García, Anderson Ospino, and Valentina Martínez for very good research assistance at different stages of this project. They also thank Mariano Bosh and other members of the Labor Market and Social Security Unit of the Inter-American Development Bank (IDB) for their very useful feedback on previous drafts of this paper, as well as seminar participants at the Development Bank of Latin America (CAF) and the Universidad del Rosario and participants at the LACEA Labor Network Conference on Challenges for Labor Markets in Latin America (Santiago, November 2015) and the 2016 LACEA Annual Conference (Medellín, November 2016). Assistance from the Colombian Ministry of Finance and Ministry of Health in accessing the data and financial support for the project from the IDB are also gratefully acknowledged.

Social security systems aimed at covering workers against the risks of old-age poverty, sickness, work-related accidents, and unemployment are frequently financed via mandatory payroll contributions paid by both employers and employees, with employers usually responsible for the larger share of the contribution.[1] In much of Latin America, high payroll taxes have been pinpointed as one of the causes of high informality and high unemployment.[2]

In Colombia, payroll taxes have been used to finance not only health coverage, maternity leave provisions, and pensions, but also monetary subsidies and in-kind transfers for low-income workers.[3] Employers are also responsible for mandatory bonuses and annual severance payments. Put together, these costs imposed by regulation added more than 50 percent to a firm's wage bill by 2012. This rate had been increasing over the last two decades from an already high 40 percent in 1992. Costs attached to these regulations come on top of a mandatory minimum wage that exceeds the median income of workers in the country.

Extremely high payroll taxation in Colombia is a source of concern for analysts and policymakers, given its expected negative effects on employment and labor formality. Both unemployment and informality have, in fact, been very high over the past two decades. Consequently, the Colombian Congress approved a tax reform in December 2012 that reduced employer contributions by 13.5 percentage points for workers earning below ten minimum monthly wages. This group of workers represents the vast majority of the Colombian workforce (specifically, 98 percent of workers of private firms with at least two employees). In particular, the reform eliminated a 3 percent contribution to the National Family Welfare Agency (ICBF), a 2 percent contribution to the National Adult Training Agency (SENA), and 8.5 percent of the employers' contributions for workers' mandatory health insurance.

1. Supplemental materials for this paper can be found in the online appendix at http://economia.lacea.org/Suplemmentary%20Material%20Appendix%20Fall%202017.htm.

2. This feature is not exclusive to Latin America. The average combined (employee and employer) payroll tax rate in member countries of the Organization for Economic Cooperation and Development (OECD) was 22.6 percent in 2013, 6.7 percent higher than the United States' combined rate of 15.9 percent. France had the highest combined payroll tax burden of 38.5 percent, followed by Austria and Hungary, both with effective payroll tax rates of 36.6 percent.

3. Transfers are provided through public-private agencies in charge of education, recreation, health, subsidies for poor households, and other services for families. These agencies include the family compensation funds (*cajas de compensación familiar,* or CCF), the National Family Welfare Agency (ICBF), and the National Adult Training Agency (SENA).

One of the objectives of the reform was to stimulate the creation of formal employment. To compensate for lost income from payroll taxes, the reform also increased corporate income taxes by reducing some of the exemptions that firms were previously allowed to claim to reduce their taxable income. In particular, the corporate income tax rate fell by 8 percentage points, while a new 9 percent tax on firm profits, called CREE, was imposed. The tax base over which firms pay CREE, however, is larger than the base for the corporate income tax, because exemptions were eliminated. Thus, more than a reform that reduced the tax burden, this was a reform that shifted the burden from formal employment to corporate income. The amount of benefits received by workers was not affected by the reform.

This reform offers a unique opportunity to analyze the effectiveness of replacing payroll taxes with taxes that do not distort the incentives to hire workers relative to other inputs of production, but that are still levied on firms. With this motivation, we analyze the effects that the reform had on formal employment and wages, using detailed firm-level administrative data covering all formal employment in the country before and after the reform.

The focus on firms is natural, to the extent that it is firms' hiring and wage policies that are directly distorted by payroll taxes. At the same time, identifying the effects of this reform on firms is particularly challenging, since the reform did not focus on particular firms or sectors. We take advantage of the fact that not-for-profit firms, many of which are de facto for profit, were exempted from the components of the reform under analysis. Though we do not have information on firms' individual tax regimes, we do know the sector to which a firm belongs. Because firms in the education and training sector are with few exceptions registered as not-for-profit, we are able to use these firms to construct a control group. We then rely on a difference-in-differences identification strategy. We deal with concerns about the comparability of firms in education with those in other sectors through a series of robustness analyses.

Using these data and identification strategy also has the great advantage of allowing us to use firms' average wages as a potential outcome variable of the reform. Increases in wages have been previously identified as a crucial effect of reforms that reduce payroll taxes.[4] Examining this potential effect is not possible when one is analyzing data on individual workers and using the reform's eligibility threshold of ten monthly minimum wage as the basis for an identification strategy that focuses on workers around that threshold.

4. Kugler and Kugler (2009); Korkeamäki and Uusitalo (2009); Gruber (1997); World Bank (2009).

As we point out in our results, wages turn out to be an important adjustment mechanism for firms.

We find a positive effect of the 2012 tax reform on formal employment and wages in the short term. The average firm in sectors affected by the reform increased its formal employment by about 4.3 percent and its average wage by 2.7 percent in the first five months after the reform came into full effect, compared to the average firm in unaffected sectors. The implied elasticity of employment to labor costs is –0.3, which falls in the range of –0.06 to 4.8 percent that has been previously estimated.[5] The positive average employment effect is concentrated in micro and small firms, while all but the largest firms (200 employees or more) display increased wages. We speculate that the lack of employment effect for larger firms may be attributable to the concurrent increase in corporate income taxation associated with the 2012 tax reform, which likely affected these firms the most. Given the lack of data on firms' individual payment of income taxes, however, we can offer only suggestive evidence on this potential channel, by showing that firms in the sectors that were most affected by the increase in the corporate tax also introduced by the reform exhibited lower employment and average wage increases as a result of the reform.

We also find that more labor-intensive firms exhibited higher increases in formal employment and wages as a result of the reform, as one would expect. Moreover, across sectors we find that the positive employment effects of the reform that we identify are concentrated in a few sectors. They are stronger in service sectors, which are the most labor intensive and the most comparable with the control group. We are unable to identify a statistically significant increase in employment for manufacturing, agriculture, and mining.

The paper is organized as follows. The next section discusses existing evidence on the effect of payroll taxes on employment. We then explain the relevant features of the 2012 tax reform and set out our basic conceptual framework. After describing our identification strategy for the treatment effects of the tax reform and the data used in estimation, we present our estimation results. The final section concludes.

Payroll Taxes and Employment: Existing Evidence

Several studies estimate the response of employment and wages to payroll taxes in different contexts. Results suggest that the effects of payroll taxes are shared by incumbent and outsider workers: decreases in payroll taxes lead

5. Heckman and Pagés (2003).

to increases in wages and also to the creation of new jobs. Overall, previous evidence finds robust negative effects of payroll taxes on the wage margin and less robust but still negative effects on employment.

Kugler and Kugler exploit a large increase in payroll taxes following the social security reform in Colombia in 1993 to study the effect of payroll taxes on both employment and wages.[6] Using a balanced panel of 235 formal manufacturing plants over the period 1982–96 from the Annual Manufacturing Survey of Colombia, they estimate regressions in first differences, with and without sector and firm effects. They find that a 10 percent increase in payroll taxes reduced formal manufacturing wages between 1.4 and 2.3 percent and formal manufacturing employment between 4.0 and 5.0 percent. They find lower wage effects and greater negative effects on employment for production than for nonproduction workers.

Compared to the analysis in that paper, the study we develop here focuses on the distortionary effect of payroll taxes rather than confounding it with the effect of higher overall taxation, which should also affect firm size. Since the 2012 Colombian reform did not simply reduce payroll taxes, but rather replaced them with higher corporate income taxation, our analysis focuses on the relative effect of payroll taxes. Our methodological approach is also different, since we exploit the fact that a few sectors were not covered by the reform under evaluation, to implement a difference-in-differences identification strategy. Finally our current study covers all sectors and formal firms in the economy.

While we also find that wages and employment react negatively to increases in payroll taxes and contributions, our estimated employment elasticity is much smaller than Kugler and Kugler's estimate: about a quarter in size and statistically insignificant for manufacturing, which is the sector they analyzed. We attribute these differences to the fact that the 2012 reform did not reduce the overall tax burden on employers. Moreover, our findings suggest that manufacturing employment is less sensitive than employment in the service sectors, for which we do find a significant response of employment to the 2012 reform.

Other studies assess the impact of changes to payroll taxes and social security contributions on employment and wages using reforms in other countries as natural experiments. As in Kugler and Kugler's study, the reforms under analysis in general reduced payroll taxes or social security contributions while also affecting the overall tax burden.

For instance, Gruber explores the effect of a reduction in payroll taxation that took place in Chile in 1981, when the social security and disability

6. Kugler and Kugler (2009).

insurance programs were privatized and other changes were introduced to the system.[7] The average payroll tax rate for manufacturing firms fell from 30 to 8.5 percent. Using data from a census of manufacturing firms, and a first-difference approach, he finds that the effect of payroll taxation was fully on wages, with no effect on employment.

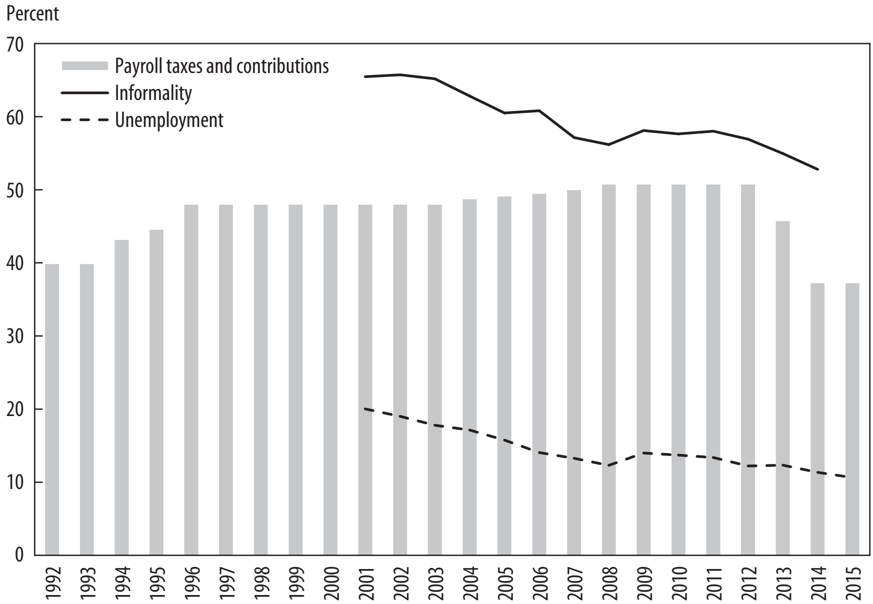
The World Bank report on the effect of labor costs on employment in Turkey also uses first-difference methodologies.[8] The authors find that employment in Turkey was indeed responsive to changes in labor costs, with an estimated elasticity of labor demand in the range of –0.4 to –0.6, comparable to findings for other middle-income and developed countries. Most of the employment adjustment in response to changes in labor costs occurred in less than eighteen months. A significant portion of the reduced tax was captured by workers in the form of higher wages.

Korkeamäki and Uusitalo evaluate the effect on employment and wages of an experiment that took place in northern Finland, where payroll contributions to the national pension system and the national health insurance were reduced by three to six percentage points over three years, from January 2003 to December 2005, but only for firms located in areas with high unemployment.[9] The authors use propensity score matching (PSM) techniques and data on employment and wages from the Finnish Tax Administration, data on firms and establishments from the Register of Enterprises and Establishments from Statistics Finland, and data on wages from two large employer organizations.[10] Their findings indicate that the decrease in payroll tax increased wage growth by 2 percent in the service sector in eligible regions. For manufacturing, their results are not robust and generally not significant, as is the case in our findings as well. While the authors speculate that this may be due to the small number of manufacturing firms in the treated regions, our findings suggest that manufacturing is indeed less responsive than services to reductions in labor costs.

Our study contributes to this literature by helping disentangle the distortion on employment caused by payroll taxes from the effect on firm size from overall taxation to the firm. We also contribute by implementing a difference-in-differences identification strategy in the context of a literature faced with

7. Gruber (1997).
8. World Bank (2009)
9. Korkeamäki and Uusitalo (2009).
10. Namely, the Confederation of Finnish Industry and Employers (TT) and the Employers' Confederation of Service Industries (PT),

FIGURE 1. Payroll Taxes and Health Contributions, the Unemployment Rate, and Informality[a]



Source: Authors' calculations.

a. Payroll taxes and contributions are calculated as a percent of wages, based on legislation. Informality is defined as the fraction of workers not contributing to pensions, based on household surveys. The unemployment rate is the official rate at the national level.

regulatory changes that do not lend themselves to well-identified evaluations, since they generally affect all of the business sector.

The 2012 Tax Reform in Colombia

In the 1990s and 2000s, Colombia exhibited double-digit unemployment rates and labor informality above 50 percent (figure 1). This came in a context of high and increasing payroll taxes and contributions. By 2012, mandatory payroll taxes and contributions added up to 49.9 percent, distributed as follows: 12.0 percent as employer contribution to the worker's pension; 8.0 percent as employer contribution for the worker's health coverage; 9.0 percent as payroll taxes (to finance SENA, ICBF, and CCF; see footnote 3); 8.4 percent as severance payments (to be deposited regularly to the worker's individual account); and 12.5 percent as vacation and legal bonuses (*primas*).

In December 2012, after a two-month discussion, Colombia's Congress approved a reform that had three main components.[11] First, it reduced payroll taxes by eliminating some of their components. In particular, it eliminated employers' contributions to SENA (the public training agency) and ICBF (the childhood services agency), previously set at 2 percent and 3 percent of firms' payrolls, respectively. The reform also eliminated employers' contributions to the health system, previously set at 8 percent of payroll. These payroll reductions applied only for workers with reported wages below ten minimum monthly wages. Over 98 percent of formal workers fall into this category, with very limited variability across firms, so this restriction is not useful for identification of the effects of interest.

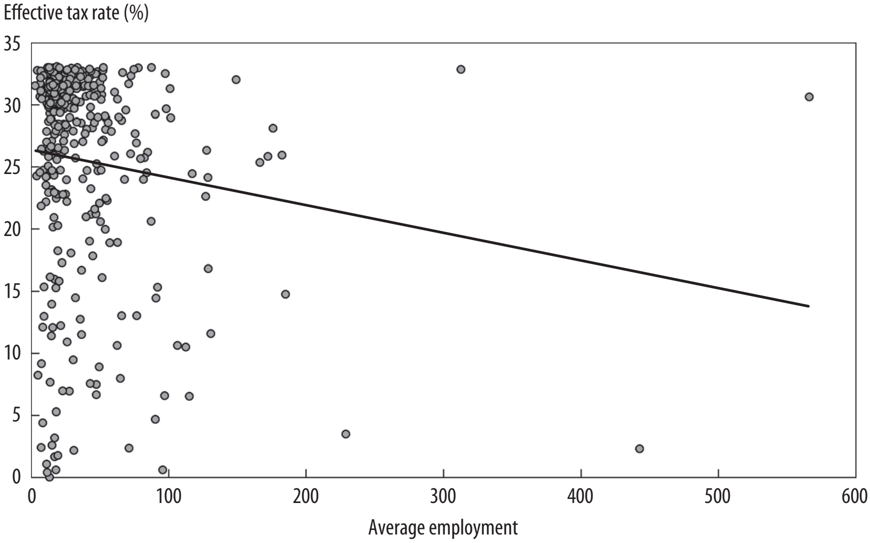
Second, the reform implemented a new corporate income tax (the CREE) of 9 percent of total profits, while reducing the existing corporate income tax from 33 percent to 25 percent.[12]

Third, the reform reduced exemptions to the corporate income tax. The Colombian tax legislation allows for a series of exemptions in the calculation of the regular corporate income tax base, as well as deductions from the tax for certain types of activities, such as printing and editorial activities; air and water transportation; hotels and other tourism activities; and environmental protection activities. In turn, several types of investments can be deducted from the tax, such as investments in research and development (R&D), agricultural activities, and activities related to environmental protection. Firms in tax-free zones also benefit from additional exemptions. As a result, there is wide dispersion in effective corporate income tax rates, both across and within sectors. Figure 2 shows the dispersion across four-digit-level sectors in 2012 (prior to the reform) in average effective tax rates versus average firm size. Effective tax rates varied from 0 to 33 percent, where 33 percent is the statutory rate. Moreover, there was a negative association between average effective tax rates and average firm size. Since the sectors that paid the lowest effective rates for corporate income taxes were hit hardest by the introduction of the CREE and the associated cut in the number of exemptions that could be claimed, this negative correlation suggests that larger firms were more vulnerable to the component of the reform that may have affected employment negatively. Unfortunately, we are unable to provide evidence that this is in fact the case, given that we do not have access to effective tax rates at the firm level.

11. The reform also included other components affecting personal income taxes and the value added tax. The impact of those components is beyond the scope of this study.

12. The CREE rate was later reduced to 8 percent, but this change occurred beyond the time horizon of our estimation.

FIGURE 2. Effective Tax Rates versus Average Firm Employment across Sectors, 2012[a]



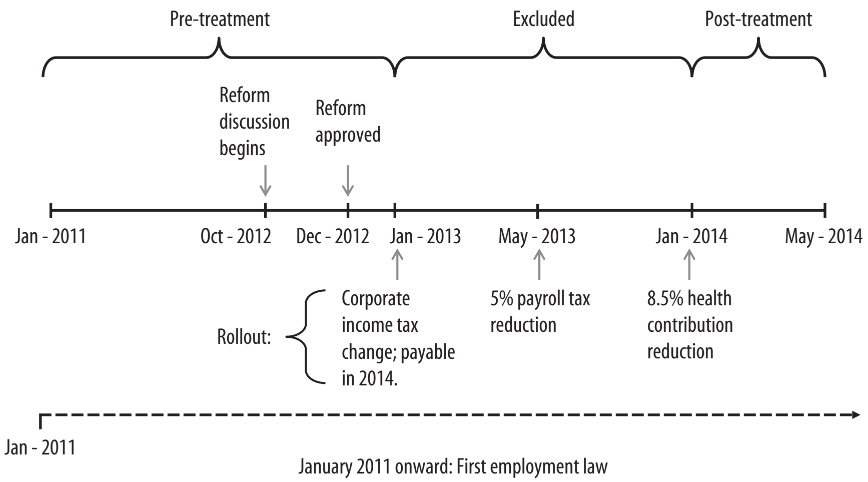
a. Effective tax rates are constructed from publicly available data aggregating tax records for fiscal year 2012, from the tax administration (DIAN). Average employment is calculated for July 2012, from PILA years 2011 to 2014. Only four-digit-level sectors included in our estimation are included in the figure. Constant: 26.391; coefficient: –0.0223; standard error: 0.088.

These different components of the 2012 reform came into effect at different points in time between January 2013 and January 2014 (figure 3). In particular, corporate income was subject to the newly introduced CREE from January 2013 onward. Although firms might have adjusted throughout 2013, the 2013 corporate tax was effectively paid in 2014. SENA and ICBF contributions were eliminated in May 2013. Health contributions were eliminated in January 2014.

The reform was thus fully in effect starting in January 2014, while 2013 was a transition period. In our baseline estimation, the post-reform period is set to start in January 2014, while the pre-reform period covers 2011 and 2012. 2013 is excluded from the estimation. Finally, the CREE was modified again by a reform discussed and passed in December 2014, which lowered the rate from 9 to 8 percent and introduced a new tax to corporate assets. Our data only cover the first five months of 2014.

As indicated in figure 3, the 2012 employment tax reform partially overlaps in time with a previous employment reform in effect between January 2011 and December 2014, the so-called First Employment Law, by which

FIGURE 3. Timeline of the Reform



new firms complying with particular age and employment characteristics, or those hiring new employees, were subject to temporary payroll tax and corporate income tax reductions. We choose to begin our period of estimation in January 2011, so that our pre- vs. post-reform comparison is not correlated with the introduction of the first employment reform.

Another public policy that overlaps in time with the tax reform is a stimulus plan introduced in April 2013 (*Plan de Impulso a la Productividad y el Empleo,* PIPE). One of the measures included in the PIPE was to move forward the start date of the 5 percent payroll tax reduction to 1 May 2013, rather than 1 July 2013 as established in the text of the tax reform initially approved by Congress. With regard to measures aimed at affecting firm choices and performance, the PIPE extended the duration of policies that were already in place and benefited mostly the manufacturing sector, such as zero import tariffs for imports of capital goods and inputs not produced locally. These policies were in effect as of our pre-reform period.[13]

The spirit of this set of reforms was to stimulate formal employment, while keeping tax revenue unchanged. The reduction in payroll taxes did not apply to employers that were not subject to corporate income taxes, because in their

13. Nevertheless, one of our robustness tests focuses on services, abstracting from sectors favored by these particular policies.

case the mechanism that should have compensated reduced payments of payroll taxes (that is, greater corporate income taxes via CREE) did not apply. This is the case, in particular, of not-for-profit organizations. Thus, firms under the not-for-profit regime constitute a potential control group. Though we cannot directly identify whether a particular employer is not-for-profit, the not-for-profit regime is known to be prevalent in the education sector. Tertiary education can only be not-for-profit under Colombian law, while training programs not leading to a degree, as well as primary and secondary education, are frequently provided by private institutions legally constituted as nongovernmental organizations. This comes in a context where the not-for-profit regime is widely abused: many services and even goods providers are registered as nongovernmental organizations to avoid corporate income taxes.[14]

Our use of the education sector as a counterfactual raises two concerns. First, to the extent that some of these institutions are in fact not-for-profit, the nature of their activity may not be directly comparable with that of the firms subject to the 2012 tax reform. Those firms would not be a good counterfactual, though it is not clear that their inclusion as controls would generate bias of a particular sign. We partially deal with this concern by showing the robustness of our estimated effect to concentrating on training and education institutions that we know are private, since public education is an area where the nature of the activity is hardly comparable to that of private businesses; our baseline specification does not focus solely on private institutions because for much of our control group we do not have information on whether they are private or public. Anecdotal evidence suggests that much of the private education sector does correspond to institutions registered as not-for-profit that are de facto for profit.[15]

A second concern relating to the use of education as a counterfactual goes in the opposite direction: some institutions in the education sector may actually be registered as for profit, pay taxes, and thus be subject to the reform

14. Public employers are also exempt from corporate income taxes and were excluded from the reduction in payroll taxes. We do not include this sector in the control group as part of our estimation strategy because public employers likely do not behave as profit maximizers and therefore do not respond to the basic logic that should lead to an increase in employment or wages as a result of a reduction in labor costs.

15. There are widely publicized cases of universities against which the Ministry of Education has taken action after finding that board members were actually owners of the institution and used its resources to pay for huge personal expenses. Anecdotal evidence suggests this is even more prevalent for small providers of training courses.

as much as other private firms. We also show the robustness of our result to excluding the education subsectors where this is most likely to occur. This source of concern, in any case, should lead to attenuation bias in our baseline estimates, where all of education is included as a control.

Conceptual Framework

Consider a profit-maximizing firm with Cobb-Douglas production function $Y_{it} = A_{it}K_{it}^{\alpha}L_{it}^{1-\alpha}$, where A is a technology shock, L is the payroll, and K the stock of capital. The firm faces a downward sloping (inverse) demand $P_{it} = D_{it}Y_{it}^{-\gamma}$, where D is a demand shock and $-1/\gamma$ is the elasticity of demand. The firm chooses its payroll (L) and capital (K) to solve

$$\max\left(1-\varphi_{it}\right)D_{it}\left(A_{it}K_{it}^{\alpha}L_{it}^{1-\alpha}\right)^{1-\gamma} - \left(1+\tau_{it}\right)L_{it} - rK_{it},$$

where φ_{it} is a corporate income tax rate, τ is a payroll tax, and r is the user cost of capital, both exogenous to the firm. The optimal payroll is given by

$$L_{it} = \left[\delta\left(1-\varphi_{it}\right)r^{-\alpha(1-\gamma)}\left(1+\tau_{it}\right)^{-\hat{\beta}}\mu_{it}\right]^{(1/\gamma)},$$

where $\hat{\beta} = 1-\alpha(1-\gamma) > 0, \delta = (1-\gamma)(1-\alpha)\left(\dfrac{\alpha}{(1-\alpha)}\right)^{\alpha(1-\gamma)}$ and $\mu_{it} = \dfrac{A_{it}^{1-\gamma}D_{it}}{r_t}$. Taking logs, and using $\ln(1+z) \approx z$ for small z, we can write

$$\ln\left(L_{it}\right) = \beta_0 - \varphi_{it} - \hat{\beta}\tau_{it} + \mu_i + \mu_t,$$

where we have decomposed the $\ln(\mu_{it})$ profitability shock into a firm fixed component and a time fixed component. The firm's optimal payroll decreases with both the tax rate on corporate income and payroll-specific taxes.

The 2012 Colombian tax reform would be predicted to have contradictory effects on the firm's optimal payroll. While the decrease in payroll taxes and contributions should push optimal payroll up, the increase in corporate income taxes should have the opposite effect. If the positive effect from the reduction in labor costs dominated, the consequent increase in the optimal payroll could be achieved via either an increase in employment or an increase in average wages. Our empirical strategy lets the data speak about which channel applies.

Empirical Approach

Our baseline estimation uses a standard difference-in-differences regression, estimated on a monthly firm-level panel covering all formal private employers in the country. The data, which come from social security administrative records, are explained in detail in the following subsection. The basic regression can be written as follows:

$$Y_{ijt} = \beta_0 + \beta_1 D_i + \beta_2 T_t + \beta_3 \circ D_i \circ T_t + u_{ijt}, \tag{1}$$

where i is a sub-index for a firm, j indicates the four-digit sector to which the firm belongs, and t is a time period (month); Y_{ijt} is either the log of firm i's total number of workers in month t or the log of the firm's average wage in that period; D_i is an indicator equal to one if firm i is not part of the education or training sector (based on self-reported sector of activity in the baseline period) and zero otherwise; and T_t is a period dummy variable equal to one from January 2014 onward, as previously documented in figure 3.

We estimate equation 1 introducing firm and time fixed effects—where a period is a month-year combination, so that coefficients β_1 and β_2 end up subsumed into these effects. That is, we effectively estimate equation 2:

$$Y_{ijt} = \alpha_i + \alpha_t + \beta\left(D_i \circ T_t\right) + u_{ijt}. \tag{2}$$

Coefficient β captures the average treatment effect of the reform: the change in employment or wages between the pre- and post-reform periods experienced by the treatment group over and above any change that the control group may have experienced over the same period. Standard errors are clustered at the firm level.

To define our treatment dummy D_i as a function of the firm's economic sector, we use the sector to which the firm reported belonging in July 2012, before the reform started to be publicly discussed. By setting our baseline definition of the sector to July 2012, we deal with the concern that firms may start to adjust in response to the announcement of the reform prior to its approval. As a consequence of the choice to use July 2012 as our baseline period, our estimation includes only employers that actually reported information during that month. For that reason, employment creation/destruction is being estimated for incumbent firms only.

The difference-in-differences approach partially deals with the concern that the effects we estimate may be picking up the effect of other policies affecting formal employment, in particular the first employment law (figure 3). Moreover, because our estimation focuses on firms that were active in July 2012, it excludes new employers, who benefit the most from the first employment reform. (This does not make the sample balanced, however, as we still have firm exit from July 2012 onward.)

We estimate equation 2 for all firms, as well as separately for micro firms (fewer than ten employees), small firms (ten to forty-nine employees), and for medium and large firms (fifty employees or more). Firms are assigned to size categories according to their size in July 2012. The employment size thresholds are those established by Law 590 of 2000 to define firm size categories in Colombia.[16] We aggregate medium firms (fifty to 200 employees) and large firms (over 200 employees) because of small cell sizes if estimated separately. We therefore allow for heterogeneous effects for large versus medium firms in the regressions for the medium-large size category.

As discussed above, firms offering education services may not be an ideal control group because they follow a different logic than firms in other business sectors and because they are different from other sectors in terms of observed characteristics. To address this issue, we test our baseline results for robustness in a variety of ways. First, we restrict the control group to institutions offering courses and training *not leading to a formal degree* and the treatment group to firms in the service sectors comparable to these given their economic activity. There is some anecdotal evidence suggesting that, regardless of being classified as not-for-profit for tax purposes, institutions in the education sector that do not offer formal degrees operate just as any other business.

Second, because educational establishments that do not offer a degree are more likely than those that do to operate as for-profit for tax purposes, we also estimate our model in a different restricted sample that excludes institutions offering programs that do not lead to a degree. We would obviously prefer to remove only those that do pay taxes as for-profit institutions, but unfortunately we do not have access to this information, so we can only abstract from all institutions not offering formal degrees.

Third, we use propensity score matching in combination with our difference-in-differences approach to make sure that our results are not being driven by differences between the treatment and control groups in observed firm characteristics that change over time.

16. Law 590 of 2000 was later modified by Law 905 of 2004, but only the size categories in terms of assets, rather than employment, were adjusted.

These three robustness checks help us assess the extent to which the potential lack of comparability between our treated and control firms biases our baseline estimate. However, while the cleaner identification strategies offered by these robustness exercises might imply better internal validity, they come at a very high cost in terms of external validity, because some of these counterfactuals are quite restrictive and in some cases even ad hoc. Given that external validity is a high priority of this study, where we aim to estimate the number of formal jobs created by the reform and shed light on the potential benefits of future reforms to economywide payroll taxes and contributions, we decided to keep our baseline specification as in equation 2. This choice is further justified by the fact that estimates are quite robust across different definitions of the control group, especially regarding the effect on wages.

As a fourth and final robustness check, we run our baseline regressions on a balanced subsample. By the nature of the social security administrative records used in this study, firms enter and exit this record frequently. In addition, firms may enter and exit the market. Both market entry and exit and the decision to report to the social security system and pay contributions may respond to the reform. By reestimating our results on the balanced sample, we concentrate on changes in employment and wages for firms that neither enter nor exit the market and that also continuously pay social security contributions and payroll taxes every month throughout our estimation period.

After testing the robustness of our basic results to different sample definitions and refinements, we assess whether average treatment effects vary by firm characteristics. In particular, we explore whether effects were stronger for firms that were, arguably, more exposed to the different components of the 2012 tax reform. First, more labor-intensive firms could have responded more to the reduction in labor costs. Second, sectors that previously benefited from high corporate tax exemptions could have been more affected by the changes in corporate tax income rules associated with the 2012 tax reform. Finally, firms for which workers earning more than ten monthly minimum wages represent a larger fraction of their payrolls could have had a lower scope to take advantage of the reform.

Data: Baseline Estimation

Our main data source for this research is the social security administrative database, aggregated at the firm level. The data come from the *Planilla Integrada de Liquidación de Aportes* (PILA), the official registry and payment system of payroll taxes and social security contributions for formal employers and workers in Colombia. The PILA contains detailed information about all formal workers, whether employed or self-employed, including their reported wage, and employed workers' payroll taxes and contributions. We use information on the number of workers reported by the firm; the average wage across those workers; the fraction of the payroll corresponding to workers with reported wages below ten minimum monthly wages; and the sector of activity of the firm. We only observe formal employment because only formal workers are registered in the PILA, so we observe the creation of formal employment.

PILA data have monthly frequency, given that payroll taxes and social security contributions are paid at this frequency. For this study, we have access to data from January 2009 to May 2014, although, as stated, we only use information dating back to January 2011. Information after May 2014 is available, but we exclude it because a new change to corporate taxation on income and property was announced and approved by Congress at the end of 2014. Extending our estimation beyond 2014 would therefore make it even more challenging to disentangle the effect of the payroll tax reform. Thus, we consider here only short-term impacts of the 2012 tax reform.

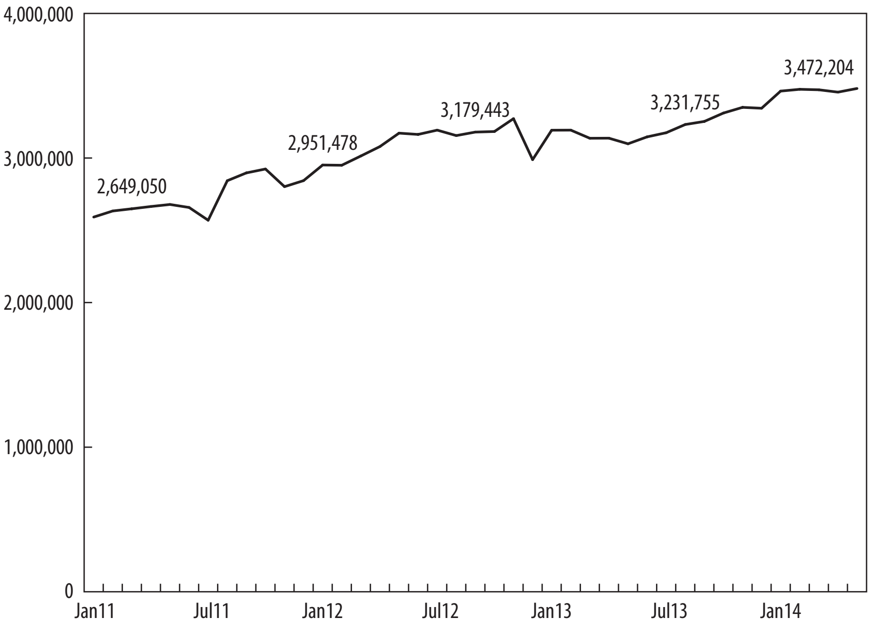
The PILA provides the four-digit code from the International Standard Industrial Classification (ISIC) for each firm's sector of economic activity and identifies each employer using its tax identification number. We use the sector code to define the treatment group and the control group.

We use PILA data aggregated at the employer level, for firms with more than one worker.[17] Single-employee employers were explicitly excluded from the reduction in payroll taxes in the reform. Because of our focus on firms, we exclude the self-employed and employers who are individuals (that is, they file under their personal tax identification number, rather than a separate business identification number) from the database.[18] To keep a focus on employers that should respond to entrepreneurial logic, we also exclude institutions with sector codes that correspond to public administration, multilateral agencies, unions, providers of outsourced labor, and hospitals. Finally,

17. The Ministry of Health granted us access to this data set under restrictive conditions that comply with the Colombian data-confidentiality regulation. In particular, all individual data were processed directly at the Ministry, and no individual-level data were made available to the research team.

18. A different component of the 2012 reform applied to the self-employed, which may have led to an increase in formality by the self-employed: firms were made responsible for ensuring that consultants paid social security contributions on the payments made by the firm. This component is beyond the scope of our investigation, which focuses solely on employment and wages in firms with at least two employees.

FIGURE 4. Employment by Private Institutional Employers



Source: Authors' calculations, based on the final database described in table A1 in the online appendix.

we exclude firms with outliers in employment changes, in particular those that in two consecutive months change size category between medium-large (over fifty employees) and micro (ten employees or fewer), or between large (over 200 employees) and small (fewer than twenty employees).[19]

Figure 4 shows how employment by firms in our data set evolved over time in the estimation period.[20] Table 1 provides additional descriptive statistics. Beyond the variables included in estimation, it also reports employment and wages in levels in Colombian pesos. The wage reported corresponds to the average wage calculated by dividing the firm's reported payroll by its number of workers.

19. See table A1 and figure A5 in the online appendix.

20. There is a marked decrease right at the time of the reform. This is a feature common to Colombian official employment statistics at the end of 2012. The reason for this decrease is unknown, though one can speculate that firms may have decided to abstain from declaring some of their workers in the expectation that the reform may turn out to benefit, for instance, firms reporting "new" workers.

TABLE 1. Summary Statistics[a]
Mean value

	Treated firms		Control firms	
Variable	*Pre-reform*	*Post-reform*	*Pre-reform*	*Post-reform*
Log employment	2.08	2.18	2.67	2.74
	(1.39)	(1.42)	(1.55)	(1.61)
Log wage	13.66	13.73	13.69	13.75
	(0.58)	(0.57)	(0.46)	(0.49)
Employment (no. workers)	31	36	71	89
	(196)	(239)	(344)	(476)
Average wage (COP$)	1,038,956	1,114,923	992,090	1,059,297
	(993,803)	(1,059,550)	(601,527)	(644,266)
No. observations	2,015,658	445,134	71,936	15,512
No. firms	101,173	91,025	3,376	3,162
No. ISIC four-digit sectors	304	304	9	9

Source: PILA years 2011 to 2014.
a. Baseline is July 2012. Standard deviations are in parentheses.

At baseline, the average control firm is significantly larger in terms of employment than the average treated firm and pays slightly lower wages (though the mean log wage is, in fact, 2 percent larger in control compared to treatment). From baseline to follow-up, both groups display increasing employment and wages, with the average log increase being larger for the treatment group.

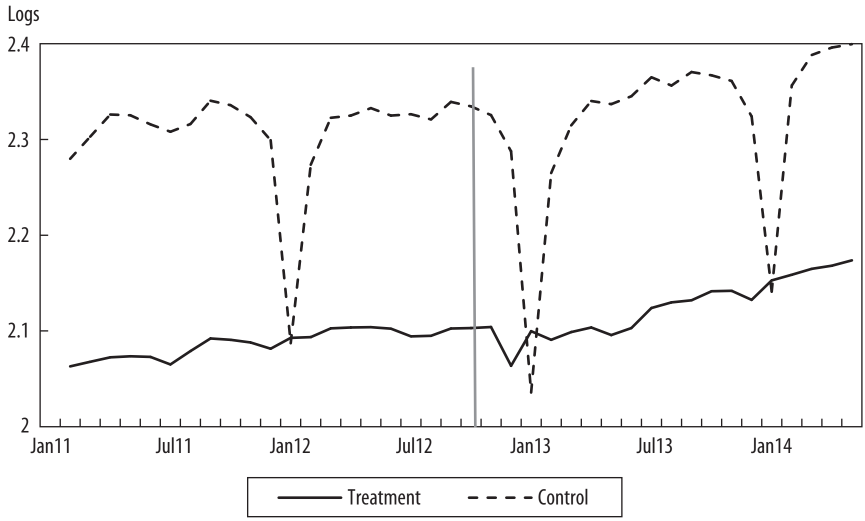
Figure 5 depicts average firm-level employment and average wage over time for both treatment and control groups, while figure 6 shows the difference between the two groups. Beyond a marked seasonality in the control group, due to an eleven-month hiring cycle that is common in education, there is no clear pre-reform difference in trends between the treatment and the control groups.

Estimation Results

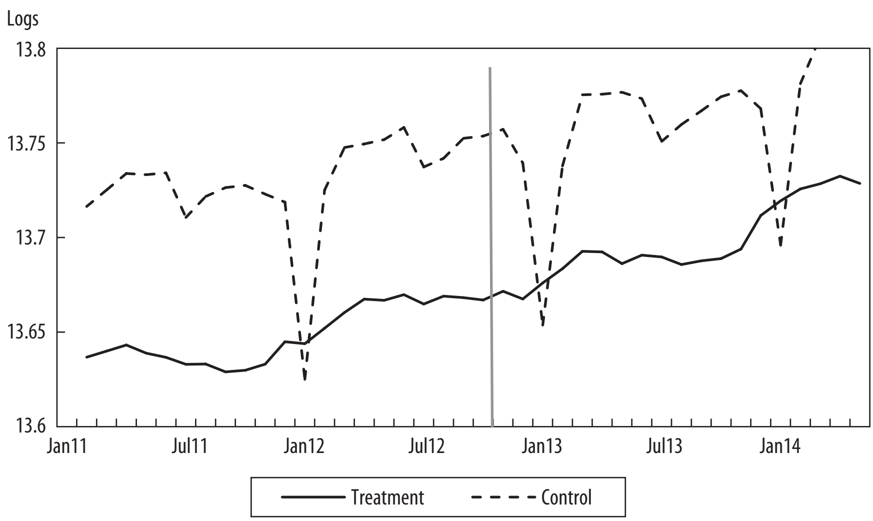
Our baseline regression is equation 2. Table 2 shows the results of this estimation. Standard errors are clustered at the firm level. On average, firms in sectors affected by the 2012 tax reform experienced increases in both employment and wages relative to firms in the education and training sectors, of 4.3 percent and 2.7 percent, respectively. The effect on employment is mainly driven by micro and small firms, which experienced respective increases of 3.7 percent and 3.1 percent relative to firms in the education sector within the respective

FIGURE 5. Average Employment and Wages[a]

A. Employment



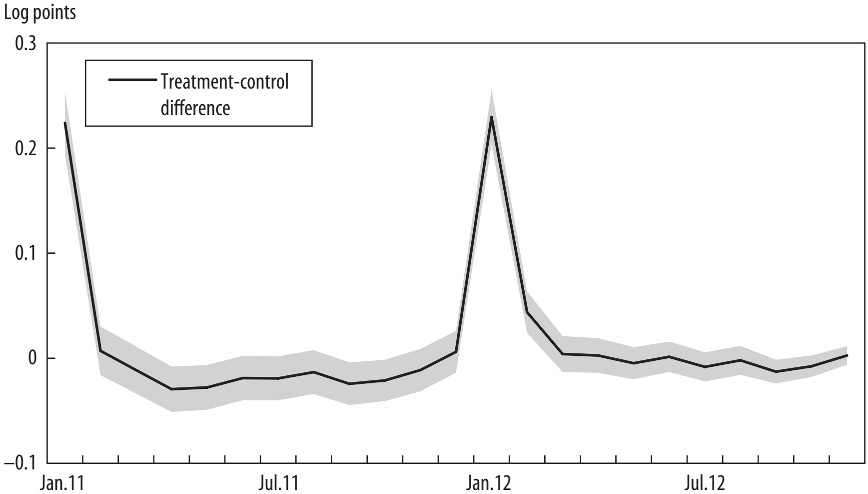
B. Wages



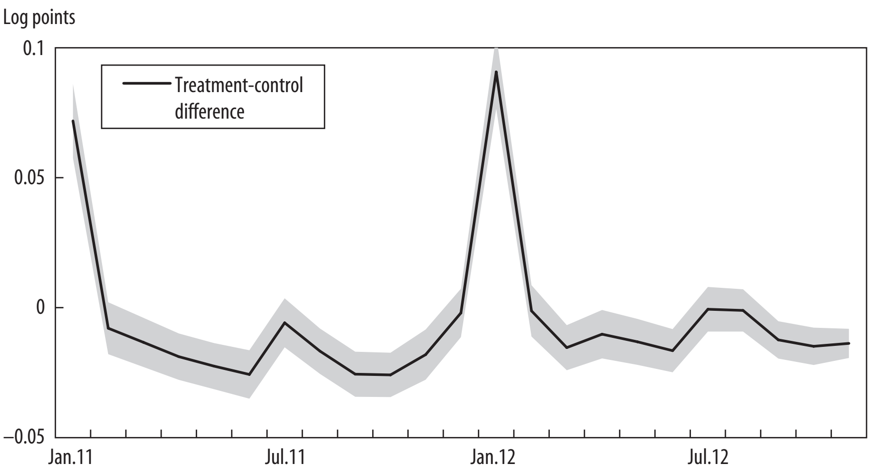
a. Authors' calculations from PILA years 2011 and 2014. The vertical line marks the time of the reform approval. Control: Education. Treatment: All other sectors. Employment: log number of workers. Wages: log wage for average worker at the firm.

FIGURE 6. Pre-treatment Trends[a]

A. Employment



B. Wages



a. The figure graphs the difference between the treatment and control groups in figure 5. The shaded area represents a 95 percent confidence band.

TABLE 2. Effects of the Reform on Firm Employment and Wages: Baseline Specification[a]

Explanatory variable	*All firms*[b] *(1)*	*Micro (2)*	*Small (3)*	*Medium and large (4)*	*Medium and large (5)*
		Outcome: Employment			
Treatment	0.0426***	0.0367**	0.0311**	−0.0267	−0.0289
	(0.00985)	(0.0174)	(0.0147)	(0.0199)	(0.0204)
Treatment∗(dummy)					0.00971
					(0.0168)
No. observations	2,548,240	1,481,014	788,061	279,165	279,165
Adjusted R squared	0.008	0.018	0.022	0.037	0.037
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
		Outcome: Wages			
Treatment	0.0269***	0.0218***	0.0367***	0.0128**	0.0162***
	(0.00361)	(0.00698)	(0.00518)	(0.00567)	(0.00583)
Treatment∗(dummy)					−0.0145***
					(0.00409)
No. observations	2,548,240	1,481,014	788,061	279,165	279,165
Adjusted R squared	0.022	0.019	0.030	0.026	0.026
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

a. The table reports the results of equation 2 estimated by OLS, with firm and period fixed effects. The dummy variable equals one if the firm is large and zero otherwise. Standard errors, clustered at the firm level, are in parentheses.

b. The overall effect in column 1 is not a weighted average of the effects for individual size classes (columns 2 through 5), as each of these individual effects is estimated in a separate regression, with the control group specific to the respective size class.

firm size category, while no significant change is observed for medium and large firms. Meanwhile, increases in wages as a result of the 2012 tax reform are found for all but large firms. Micro and small firms experienced average wage increases of 2.1 percent and 3.7 percent, respectively, while the increase for medium firms is around 1.6 percent.

Using the size-specific estimated effect and the total employment in each class, we calculate that our estimates imply an average monthly increase in employment of 29,000 jobs, or an estimated 145,000 jobs created over this initial five-month period after the implementation of the reform (an aggregate effect of about 1 percent monthly for the first five months of implementation).[21]

A simple back-of-the-envelope calculation of the potential fiscal effect of the reform suggests that the negative impact on fiscal revenue from reducing payroll tax rates was partially offset by increasing employment and wages. According to our baseline estimation results for the whole sample (column 1 in table 2), the payroll of the average firm increased by approximately 7 percent.[22] This increase compensates a good fraction of the 13.5 percent decrease in the payroll tax rate, for the average firm. However, this compensating effect is concentrated in micro and small firms (that is, the average firm is micro-small), which represent about 28 percent of employment and 22 percent of payroll. As a result, only about 11.4 percent of the forgone revenue from payroll taxation is recovered via an increase in employment and wages.[23] Of course, this partial recovery comes on top of the increase in corporate income taxes, which was designed to make the reform revenue-neutral.

We test the robustness of our results in different subsamples of firms, for both the control and treatment groups (see table 3). First, we present results for a subsample of sectors in control and treatment that we believe are closest in nature (column 1: Restricted sample I). This subsample includes treated firms in information technology consultancy, research and development services, cultural activities, and other personal services compared with control firms that we know are private: educational and training services not leading to a formal

21. At baseline (July 2012), total employment was distributed by firm size as follows: 241,856 (8 percent) in micro firms, 650,254 (20 percent) in small firms, and 2,300,969 (72 percent) in medium and large firms.

22. The 7 percent increase results from combining the increases in employment and wages: 1.07=1.0426*1.0269.

23. The 11.4 percent is the result of calculating (0.07/0.135)*0.22.

TABLE 3. Effects of the Reform on Firm Employment and Wages: Robustness[a]

Explanatory variable	*Restricted sample I (1)*	*Restricted sample II (2)*	*PSM (3)*	*Balanced sample (4)*
		Outcome: Log employment		
Treatment	0.0591**	0.0497***	0.0338***	0.0351***
	(0.0216)	(0.0107)	(0.0114)	(0.0116)
No. observations	304,654	2,532,171	443,579	1,429,232
Adjusted R squared	0.018	0.008	0.018	0.009
Firm fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
		Outcome: Log wage		
Treatment	0.0399***	0.0310***	0.0238***	0.0218***
	(0.0086)	(0.0039)	(0.0041)	(0.0038)
No. observations	304,654	2,532,171	443,579	1,429,232
Adjusted R squared	0.029	0.03	0.028	0.037
Firm fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

a. The table reports the results of equation 2 estimated by OLS, with firm and period fixed effects. Restricted sample I: The treatment group includes information technology consultancy, research and development services, cultural activities, and other personal services; the control group includes private providers of tertiary education and education not leading to a formal degree. Restricted sample II: The treatment group includes the full treatment sample; the control group excludes education and training not leading to a formal degree. PSM: Probability of treatment at baseline (July 2012) as a function of characteristics in July 2012; the estimation sample is restricted to the control group and the five nearest treated neighbors. Balanced sample: includes all firms reporting to PILA every period between July 2012 and May 2014. Standard errors are in parentheses.

degree and private providers of tertiary education.[24] Admittedly, this is an ad hoc selection of sectors based on our beliefs about which sectors are closest to education. Nevertheless, it sheds some light on whether or not our results derive from the particular differences between education and the rest of the economy. Second, we present results comparing the complete treatment group to control firms in education and training services leading to a formal degree (column 2: Restricted sample II). The exclusion of those not leading to formal degrees addresses the concern that many of these may actually declare a for-profit status and thus were affected by the reform. Third, we also show results of a matched difference-in-differences estimation in which the probability of treatment was estimated as a function of firm characteristics observed in July 2012 (column 3: PSM). We match the samples by using the five nearest neighbors in the treatment group for each firm in the control group. Finally, we

24. For primary and secondary education, we lack access to information on private/public ownership.

estimated the model with a balanced sample that excludes firms that exited the PILA system after July 2012 (column 4: Balanced sample).[25]

Our finding of positive and statistically significant effects on both employment and wages is, in general, robust in the specifications presented in table 3. In particular, restricting the sample to firms that are closer in terms of their economic activities and limiting control firms to those effectively unaffected by the reform yield slightly higher effects than those reported in our baseline specification using the complete sample (5.9 percent and 5.0 percent versus 4.7 percent in the case of employment, and 4.0 percent and 3.1 percent versus 2.7 percent in the case of average wages). The sample obtained using the PSM model, which matches firms based on July 2012 characteristics, and the balanced panel result in smaller but still positive and significant coefficients (both in economic and statistical terms) for both wages and employment.

Overall, we take our results to imply that the reform had a clearly positive effect on the wages of a firm's average worker, bound between 2.0 percent and 3.3 percent for the average firm and present for firms of all sizes. It also increased employment, in a range of 3.3 percent to 5.0 percent for the average firm, but this effect is concentrated in smaller firms. On aggregate, there was little employment creation (about 1.0 percent in the average post-reform month in our sample period). In addition to this important caveat, the employment effect is more difficult to identify with precision.[26]

25. See table A2 and figure A1 in the online appendix for participation models and balance tests. Firms frequently stop reporting to PILA for short periods of time. A firm's presence in the database after not having reported in the previous period, or its absence from the registry, should therefore not be understood as entry and exit from the market. When we examined pairwise entry and exit rates, defined as the fraction of firms that report to PILA in month t but not month $t-1$ or vice versa, we found that just prior to the approval of the reform, a large fraction of firms that were reporting to PILA stopped doing so, in both the treatment and control groups (see figure A4 in the online appendix). Immediately following the approval of the reform, there is marked entry into the database by firms that did not report in the previous month, even more marked in the treatment group than in the control group. Other than these two specific peaks, there is no clear difference pre- and post-reform or between treatment and control groups.

26. For example, the statistical significance of the estimated employment effect is lost specifically for small firms in some of the restricted samples. We also used an alternative specification of the propensity score by matching firms based on monthly characteristics during the whole year prior to the implementation of the 2012 tax reform. In this matched sample, the positive effect on wages is also positive and significant, but the effect on employment is not statistically significant and is close to zero in magnitude. This alternative specification, by matching control and treatment firms based on pre-treatment characteristics for several months before the reform, minimizes pre-treatment trend differences between treatment and control. However, it yields a de facto peculiar sample of treatment firms on an eleven-month hiring cycle similar to that of education, where the immediate effect of the reform (January 2014) is lost by construction. For this reason, we do not treat this as a preferred robustness test, but note that the effect on employment is not as robust as that on wages.

Finally, our estimated effects are robust to including outliers that we had cleaned from our baseline estimation because of extreme changes in employment. Despite marked changes in the numbers of observations, the estimated results remain similar in sign, significance, and magnitude.

Heterogeneous Effects by Exposure to Reform

In this section, we present heterogeneous effects by potential exposure to the payroll reduction component of the tax reform measured by the average labor-capital ratio in the sector and by potential exposure to the change in corporate income taxation as measured by the average effective tax rate in the sector. In particular, we expand specification 2 to allow the effect of the reform to vary with the degree of exposure in treated sectors to each of the reform's components, by including interactions of our treatment indicator with exposure measures. We measure exposure to the reduction of payroll taxes through sector-level labor-capital ratios at baseline (2012) and exposure to the increase in corporate income taxation with the (inverse of the) sector-level effective tax rate in 2012. Due to restrictions in data access, we had to proxy exposure at the sector level. We also estimated a version of the model in which labor-capital ratios are substituted by the share of each firm's payroll represented by workers earning less than ten minimum monthly wages, the only workers for which the reduction in payroll taxes applied.

Before presenting results for these specifications, we briefly comment on conceptual bases for them, as well as on the measurement of exposure indicators.

—Labor-capital exposure. The elasticity of employment and wages to payroll tax reductions depends on how labor intensive the technology is: employers that use more labor-intensive technologies are expected to increase their payroll more as a result of reduced payroll taxes. Since the PILA does not have information on technology, we bring in information on capital stocks for a subset of firms for which this information is available in a publicly available administrative data set: namely, the *Supersociedades* data set.[27] For these firms, we construct labor-capital ratios using the capital stock reported to *Supersociedades* in 2012 and the employment recorded in the PILA in July 2012. We then average across these firms within each four-digit-level sector

27. The *Supersociedades* database contains official financial statements of all firms registered as partnerships and some other firms. It effectively covers all medium and large firms, and an important fraction of small firms (about 50 percent of all formal small firms). In our data set, 20 percent of firms present in PILA in 2012 are also found in the *Supersociedades* data.

(based on sector codes reported in July 2012) to obtain a sector-level average labor-capital ratio. We apply this ratio to all firms in the sector, as a measure of pre-reform exposure to the payroll tax reduction. Our use of sector-level—rather than firm-level—labor-capital ratios not only reflects the fact that we lack information on capital for most of the sample, but is also natural to the extent that we are attempting here to measure a characteristic of the technology that is generally common to firms producing the same goods. It also helps us address concerns about the endogeneity of the labor-capital choice. However, our sectoral labor-capital measure is noisy both because it corresponds to a selected sample of firms in each sector and because capital comes from financial statements subject to underreporting incentives and to accounting practices for reporting book values of fixed assets that may differ from the economic concept of productive capital.

—Effective tax exposure. The change in corporate income taxes corresponds to a reduction in applicable exemptions. Thus, firms that previously enjoyed those exemptions, or equivalently those with lower effective income tax rates, suffered the largest increase in taxation. We therefore measure effective tax exposure, TE, for sector *j* as follows:

$$\text{(3)} \qquad TE_{j,2012} = 1 - \frac{\tau^{EFF}_{j,2012}}{\tau^{NOM}_{j,2012}} = 1 - \frac{\tau^{EFF}_{j,2012}}{0.33},$$

where $\tau^{EFF}_{j,2012}$ is the effective tax rate in 2012 and 0.33 is the nominal corporate income tax rate pre-reform. We used sector-level data from the Colombian tax authority (DIAN) to calculate the average income tax rates effectively paid by firms in each sector in 2012. The data, which are publicly available through the DIAN website, report total corporate income taxes paid and total income declared before tax exemptions and deductions, by firms in each sector. We calculate the effective tax rate as the ratio between the former and the latter. The expected increase in taxation due to the elimination of exemptions in the 2012 reform is stronger when TE is closer to one (that is, the firm claimed more exemptions prior to 2012) and weaker when TE is closer to zero. Effective tax rates for selected sectors were presented in figure 2. Because some firms in a given sector take greater advantage than others of certain exemptions and reductions (for example, firms in tax-free zones or firms making deductible investments), we would have ideally measured tax exposure at the firm level. Unfortunately, researchers are not granted access to firm-level tax data due to confidentiality regulations.

We estimated the heterogeneous effects for the sample of our baseline specification (table 2). We use flexible specifications where heterogeneous effects are allowed to be nonlinear and find some curvature in heterogeneous effects. Results are presented in table 4 and summarized in figures 7 to 9 for different levels of labor-capital ratios and effective taxation rates. In table 4, all exposure measures have been de-meaned so that the coefficient on the treatment variable alone can be interpreted as the effect on the average labor-capital ratio or average effective taxation.

Results for average exposure levels are consistent with those in table 2. As observed in figures 7 and 9, the effect on both employment and wages is generally increasing with L/K, even after we control for tax exposure. However, we have very little power to identify differences in estimated effects between different levels of exposure, as reflected in the fact that these differences are generally not statistically significant. Something similar can be stated regarding heterogeneous effects by tax exposure (figures 8 and 9): the positive effect of the reform is broadly decreasing in the degree of tax exposure, as expected, but the differences between levels of exposure are not statistically significant. Interestingly, the slope of the effect with respect to tax exposure is less marked (in terms of both magnitude and statistical significance) within size classes than pooling firms of all sizes.[28] Together with the negative pre-reform association between effective tax rates and average firm size at the sector level (reported in figure 2), this is broadly consistent with our argument that the lack of a positive effect of the reform on employment for larger firms may reflect their vulnerability to the elimination of tax exemptions also associated with the 2012 reform.

Because the results in this section are not estimated with the desired precision level, we take them as only suggestive of heterogeneous effects by degree of exposure in the expected directions. We speculate that our lack of estimation power is due to the fact that we do not have information on tax exposure for individual firms and have information on capital only for a few selected firms.

Alternative, but weaker, heterogeneous effects models are generally supportive of the findings just reported.[29] First, we substituted the labor-capital ratios by the share of payroll represented by workers earning less than ten minimum monthly wages, which are the only workers for whom payroll taxes were reduced. A major problem with this approach is that over 98 percent of

28. See figure A3 in the online appendix.
29. See figures A2 and A3 and tables A3 and A4 in the online appendix.

TABLE 4. Heterogeneous Effects by Pre-Reform Exposure Levels[a]

	Log employment			Log wages		
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Treatment	0.0427***	0.0422***	0.0423***	0.0269***	0.0267***	0.0267***
	(0.00985)	(0.00985)	(0.00985)	(0.00361)	(0.00361)	(0.00361)
ln(L/K) * Treatment	0.0309***		0.0269***	0.0153***		0.0126***
	(0.00780)		(0.00800)	(0.00301)		(0.00310)
$\ln(L/K)^2$ * Treatment	−0.00423***		−0.00375***	−0.00167***		−0.00135***
	(0.00123)		(0.00125)	(0.000465)		(0.000473)
Tax exposure * Treatment		0.0258	0.0227		0.0451***	0.0416***
		(0.0352)	(0.0352)		(0.0142)	(0.0142)
Tax exposure^2 * Treatment		−0.0831**	−0.0656		−0.0878***	−0.0749***
		(0.0418)	(0.0418)		(0.0169)	(0.0170)
Summary statistic						
No. observations	2,548,240	2,548,240	2,548,240	2,548,240	2,548,240	2,548,240
Adjusted *R* squared	0.008	0.008	0.008	0.022	0.022	0.022
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

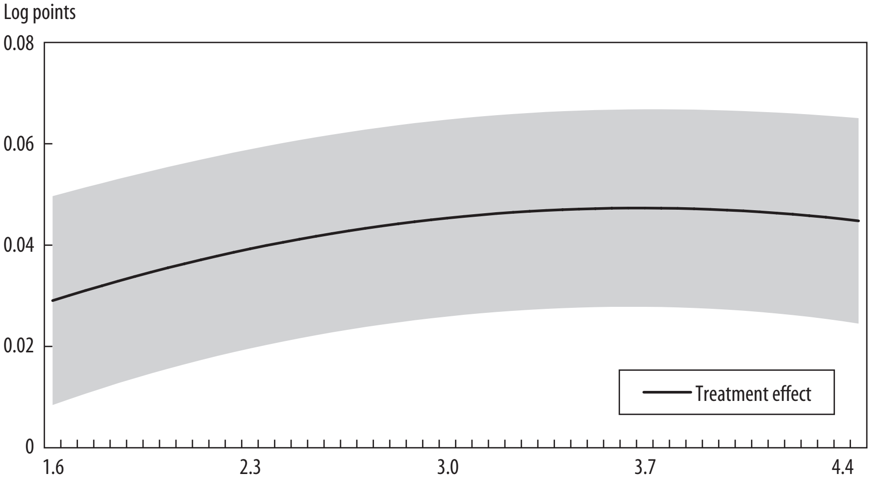
** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

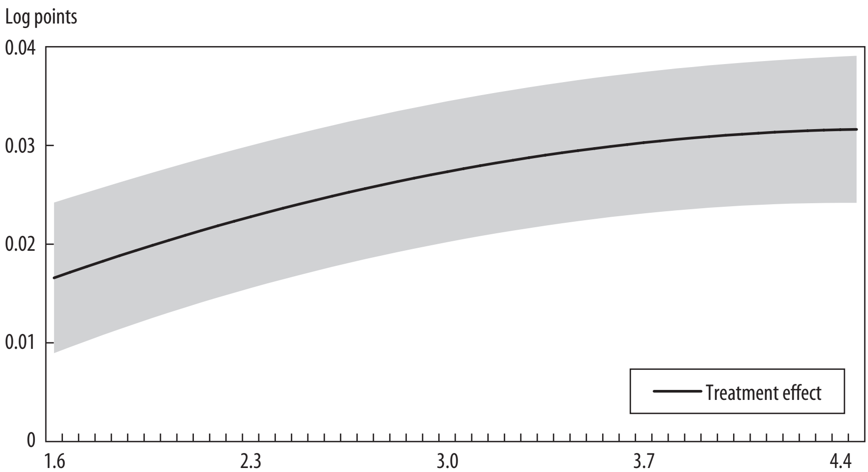
a. The table reports the results of equation 2, expanded with interactions, estimated by OLS with firm and year fixed effects (L/K) and tax exposure at the four-digit sector level (L/K) and tax exposure have ben de-meaned, so that the coefficient on Treatment is the effect at average exposure. Tax exposure is calculated as in equation 3. Standard errors, clustered at the firm level, are in parentheses.

FIGURE 7. Heterogeneous Effects by ln(L/K)[a]

A. Employment

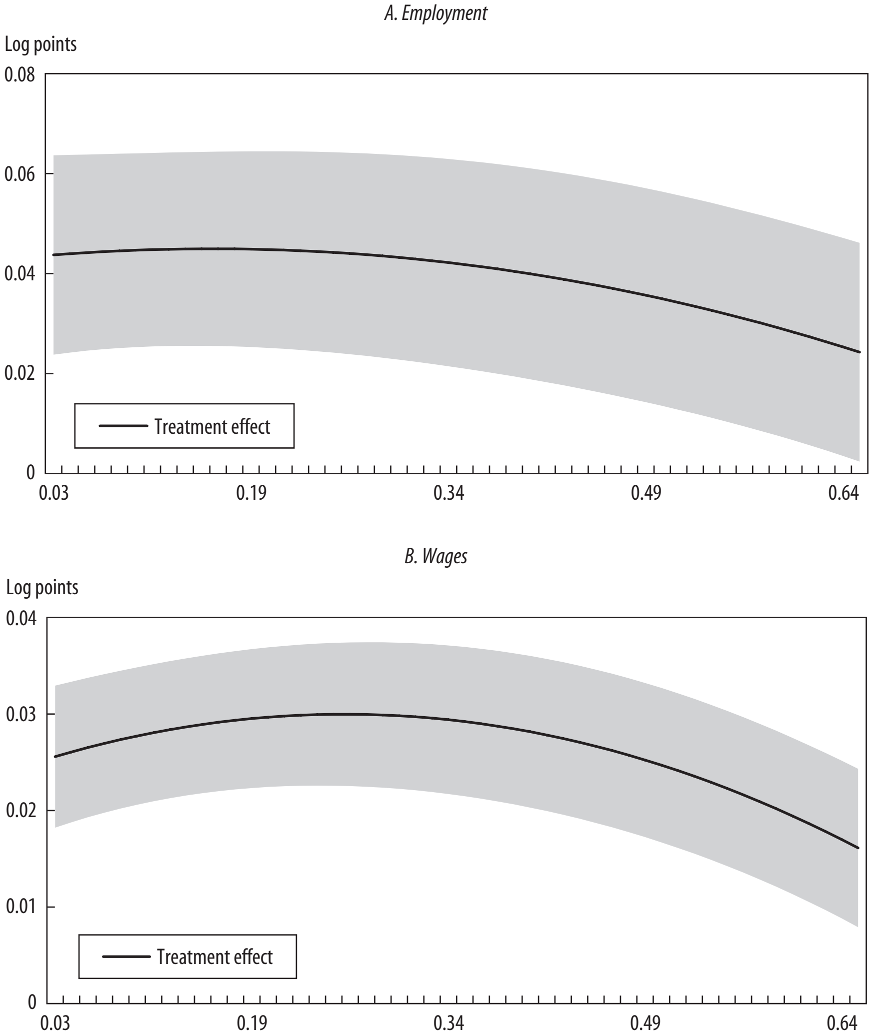


B. Wages



a. The figure presents the estimated average treatment effects for different levels of L/K, estimated in table 4 (columns 1 and 4). The shaded area represents a 95 percent confidence band.

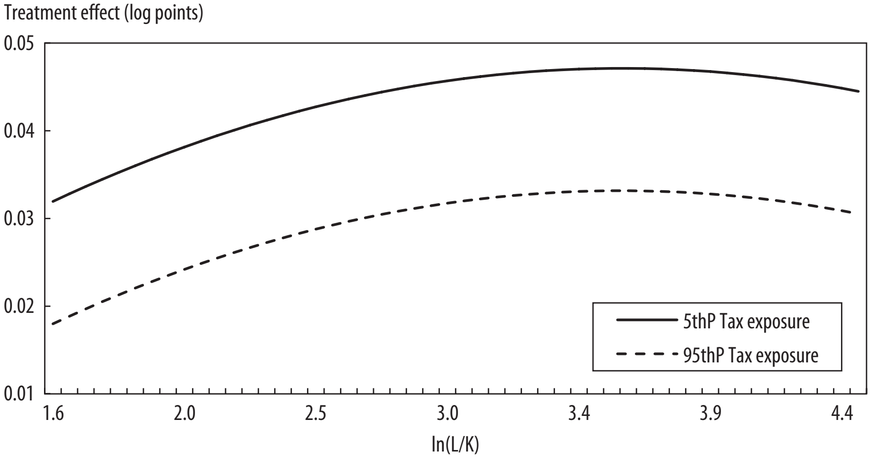
FIGURE 8. Heterogeneous Effects by Tax Exposure[a]



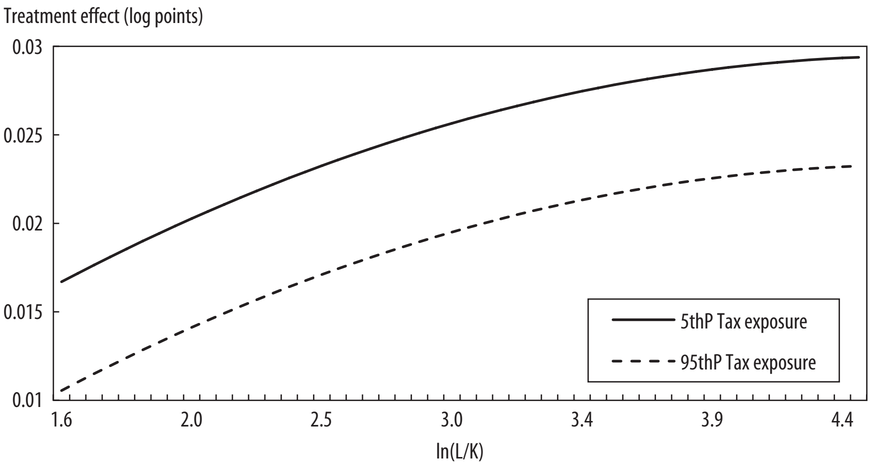
a. The figure presents the estimated average treatment effects for different levels of tax exposure, estimated in table 4 (columns 2 and 5). The shaded area represents a 95 percent confidence band.

FIGURE 9. Heterogeneous Effects by ln(L/K) and Tax Exposure[a]

A. Employment



B. Wages



a. The figure presents estimated average treatment effects for different levels of L/K at different levels of tax exposure, estimated in table 4 (columns 3 and 6).

workers and close to 90 percent of the payroll in our data set fall into this category, with little variation across firms, which places a big question mark on this dimension as a source of heterogeneity in effects at the level of the firm. In any case, running this exercise on our sample returns a wage effect that is strongly increasing in the payroll fraction for exposed workers, with statistically significant differences between low and high levels of exposure, and no heterogeneous effect on employment.

Second, we re-estimated our model using two-digit sectors of the ISIC classification (revision 3), as an alternative way of approaching the question of whether more labor-intensive sectors responded more to the decrease in payroll taxes. Results are generally consistent with a positive answer to this question. We find that the positive effect of the reform on employment was concentrated on service sectors, including construction, wholesale and retail trade, transportation, hotels and restaurants, information technology and telecommunications (ITT), financial services, real estate, and professional services. For manufacturing and agriculture, we estimate much smaller coefficients of about 1 percent, which are statistically insignificant in both cases. For mining, we find a large negative effect of the reform. Mining in Colombia is dominated by the oil industry, a highly capital-intensive sector. Of course, this sector-by-sector approach is problematic to the extent that there is wide variability in the degree to which alternative treatment sectors are comparable to the education sector, our control group.

Conclusions

The 2012 Colombian tax reform offers an ideal laboratory to study the potential distortions against employment caused by payroll taxes. Rather than just reducing taxes, the reform was designed to continue raising approximately the same fiscal revenue from firms, but in a way that is less biased against employment. Consequently, and unlike other reforms to payroll taxes, the effect of the distortion on employment is not confounded with the effect of a general decrease in the taxation faced by the firm. Moreover, shifting taxation from employment to other business activities is a policy recipe under discussion throughout Latin America and likely in other parts of the world.

To take advantage of these unique features of the 2012 Colombian tax reform, we analyze the reform's impact on employment and wages. Our findings suggest that employment in micro and small firms is highly sensitive to employment-biased taxes, while medium and large firms seem to react mainly to overall taxation. In our data, firm-level employment did not seem to increase in the category of medium to large firms, whereas micro and small firms increased employment by over 3 percent. This implies that businesses employing at least two workers created 145,000 new formal jobs in the first five months after the reform. At the same time, wages increased in firms of all size categories, at a magnitude of close to 2.7 percent for the average firm.

While these findings are supportive of additional efforts to reduce payroll taxes, they also raise concerns about the sensitivity of employment in Colombia to the overall taxation faced by firms. This high sensitivity to taxes is probably not independent of the fact that businesses in Colombia face an extremely high overall tax rate (about 70 percent).[30] In the current context of reduced tax revenue due to falling oil prices, these findings suggest extreme caution against further increases in corporate taxes.

30. World Bank (2017).

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