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

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Abstract

The Colombian 2012 tax reform reduced payroll taxes and employer contributions to health insurance by 13.5%, 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. In this context, the reform offers an ideal laboratory to study empirically the potential distortions against formal employment associated with payroll taxes *in contrast to other taxes to firms*. Using monthly firm-level data on all formal employment in the country, and a difference-in-difference approach that takes advantage of the fact

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that a few sectors were exempt from the 2012 tax reform, we analyze the impact of the reform on employment and wages. We find a positive average effect of 4.3% on employment and of 2.7% on average firm wage, for the average firm. The employment effect is identified only for micro and small firms, while we do not find a significant employment effect for medium and large firms, where the bulk of the employment is concentrated. According to these estimates, between January and May of 2015 about 145K new jobs were created by virtue of the reform. Though our findings on employment are less robust than those on wages, they are generally supportive of efforts to reduce payroll taxes, which are still high in the country. Since the apparent lack of effect for medium and large employers may be due to these firms being more sensitive to the increase in corporate taxation that financed the reduction in payroll taxes, our results also raise concerns about this particular way of financing the reform, especially for medium and large firms, which according to our data represent 70% of formal employment.

Keywords: Payroll taxes, tax reform, employment, Colombia.

JEL codes: H25; H32.

1. Introduction

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. In much of Latin America, high payroll taxes have been pointed at as one of the causes of high informality and high unemployment.[3]

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[3] This feature is not exclusive of Latin America. The average combined (employee and employer) payroll tax rate in the OECD was 22.6 percent in 2013, 6.7 percent higher than the U.S's combined rate of 15.9

In Colombia, payroll taxes have been used to finance not only health coverage for sickness, maternity leave provisions and pensions, but also monetary subsidies and in-kind transfers for low-income workers.[4] Employers are also responsible for mandatory bonuses and annual severance payments. Put together, these costs imposed by the regulation added more than 50% to a firm's wage bill by 2012 (Figure 1). This rate had been increasing over the last two decades from an already high 40% 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 has been a source of concern to analysts and policy makers, given its expected negative effects on employment and labor formality. Unemployment and informality, in fact, have been very high over the past two decades (Figure 1). In this context, Colombia's Congress approved, in December 2012, a tax reform that reduced employer contributions by 13.5 percentage points for workers earning below ten minimum monthly wages, who represent the vast majority of the Colombian workforce in numbers (98% of workers of private firms with at least two employees). In particular, the reform eliminated a 3% contribution to the National Family Welfare Agency, a 2% contribution to the National Adult Training Agency, and 8.5% of the employers' contributions for workers' mandatory health insurance. 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 in order to reduce their taxable income. In particular, the corporate income tax rate fell by 8pp, while a new 9pp tax on firm profits, called CREE, was

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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.

[4] Transfers are provided through public-private agencies in charge of education, recreation, health, subsidies for poor households and other services for families (Cajas de Compensación Familiar-CCF), the National Family Welfare Agency (ICBF) and the National Adult Training Agency (SENA)..

imposed. The tax base over which firms pay CREE, however, is larger thatthan 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.

Figure 1: Payroll taxes and health contributions (as % of wages), unemployment and informality
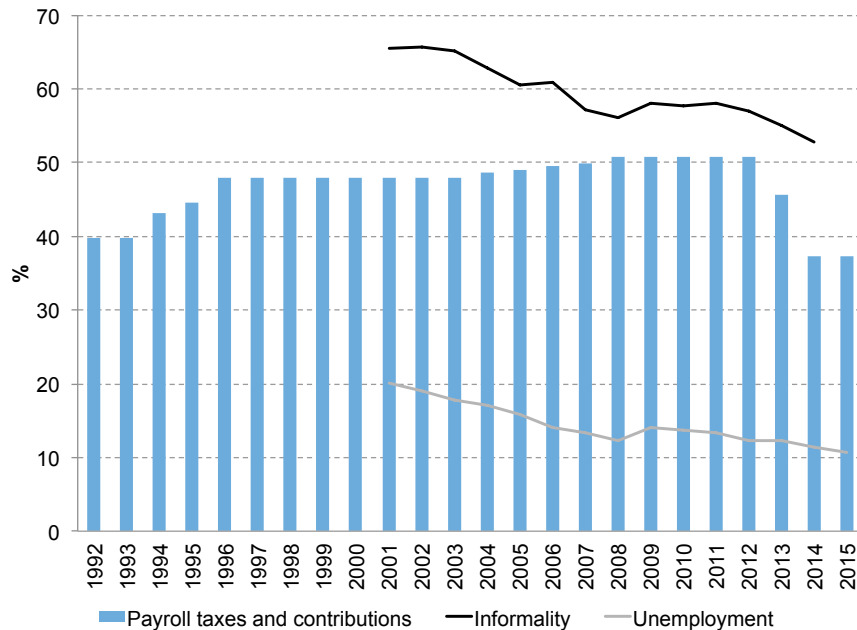
Notes: Payroll taxes and contributions: as % of wage. Own calculations based on legislation. Informality: Fraction of workers not contributing to pensions, own calculations based on household surveys. Unemployment: official unemployment rate, national level.

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 administrative firm-level 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 non-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 regime, we do know the sector to which a firm belongs, and use the fact that firms in a particular sector, education and training, are with few exceptions registered as non-for-profit, to construct a control group. We then rely on a difference-in-difference 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 (Kugler and Kugler, 2009; Korkeamäki and Uusitalo, 2009; Gruber, 1997; The World Bank, 2008). Examining this potential effect is not possible when analyzing data on individual workers and using the reform's eligibility threshold of ten monthly minimum wages as the basis for an identification strategy that focuses on workers around that threshold. As we point out in our results, wages turn out to be an important adjustment mechanism of 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% and its average wage by 2.7% 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 between -0.06% and 4.8% that has been previously estimated (Heckman and Pagés, 2003). The positive average employment effect is concentrated in micro and small firms, while all but the largest firms (200+ employees) display increased wages. We attribute the lack of employment effect for larger firms to the concurrent increase in corporate income taxation

associated with the 2012 tax reform, which likely affected these firms the most, though given the lack of data on firms' individual payments of income taxes we can offer only suggestive evidence on this potential channel.

We also find that more labor-intensive firms exhibited higher increases in formal employment and wages as a result of the reform, as we would expect. The positive employment effects of the reform that we identify concentrate in a few sectors. They are stronger in services, which are the sectors 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. Section 2 discusses existing evidence about the effect of payroll taxes on employment. Section 3 explains the relevant features of the 2012 tax reform. Section 4 sets out a basic conceptual framework. Section 5 describes the identification strategy for the treatment effects of the tax reform and the data used in estimation. Section 6 presents our estimation results, while section 7 concludes.

2. Payroll taxes and employment: existing evidence

Several studies have estimated the response of employment and wages to payroll taxes in different contexts. Results suggest that the effects of payroll are shared by incumbent and outsider workers: decreases in payroll taxes lead 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 (2009) 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. 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, and find that a 10% increase in payroll taxes reduced formal manufacturing wages between 1.4% and 2.3% and formal manufacturing employment between 4% and 5%. They find lower wage effects and greater negative effects on employment for production than for non-production 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 overall higher 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-difference 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 services sectors, for which we do find a significant response of employment to the 2012 reform.

Other studies have assessed the impact on employment and wages of changes to payroll taxes and social security contributions using reforms in other countries as natural experiments. They share with Kugler and Kugler's (2009) research the

feature that the reform under analysis in general reduced payroll taxes or social security contributions while also affecting the overall tax burden.

For instance, Gruber (1997) explores the effect of a reduction in payroll taxation that took place in Chile in 1981, when the Social Security and Disability Insurance programs were privatized and other changes to the system were introduced. The average payroll tax rate for manufacturing firms went from 30% to 8.5%. 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, and finds no effect on employment.

The World Bank report on the effect of labor costs on employment in Turkey (2009) also uses first-difference methodologies. 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 between -0.4 and -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 18 months. A significant portion of the reduced tax was captured by workers in the form of higher wages.

Korkeamäki and Uusitalo (2008) 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 to the National Health Insurance were reduced by 3 to 6 percentage points during three years, from January 1st 2003 to December 31st 2005, but only for firms located in high unemployment areas. The authors use propensity score matching 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.[5] Their findings indicate

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[5] The Confederation of Finnish Industry and Employers (TT) and from the Employers Federation of the Service Industries (PT),

that the decrease in payroll tax led to 2% faster wage growth in eligible regions in the services sector. For manufacturing, their results are not robust and generally non-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 sectors 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-difference identification strategy in the context of a literature faced with regulatory changes that do not lend themselves to well identified evaluations, since they generally affect all of the business sector.

3. The 2012 tax reform in Colombia

In the 1990s and 2000s Colombia exhibited two-digit unemployment rates, and labor informality above 50% (Figure 1). This came in a context of high and increasing payroll taxes and contributions. By 2012 mandatory payroll taxes and contributions added to 49.9% distributed as follows: 12% as employer contribution to the worker's pension; 8.0% as employer contribution for the worker's health coverage; 9% as payroll taxes (to finance SENA, ICBF and CCF, see footnote 1); 8.4% as severance payments; and 12.5% as vacation and "legal" bonuses ("primas").

In December 2012, Colombia's Congress approved a reform that:[6]

1. Reduced payroll taxes by eliminating some of their components. In particular, it eliminated employers' contributions to SENA (the public training agency) and

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[6] The reform also included other components affecting personal income taxes and the VAT. The impact of those components is beyond the scope of this study.

ICBF (the childhood services agency), previously set at 2% and 3% of firms' payrolls respectively. The reform also eliminated employers' contributions to the health system, previously set at 8.0% of payroll. These payroll reductions applied only for workers with wages below ten minimum monthly wages (over 98% of formal workers fall into this category, with very limited variability across firms, so this restriction is not useful in estimation).

2. Implemented a new corporate income tax (the "CREE") of 9% over total profits,[7] while reducing the existing corporate income tax from 33% to 25%.

3. Not only the 9% CREE rate is higher than the 8% reduction in the traditional corporate income tax, but the CREE is paid over a tax base subject to less exemptions and deductions than those that typically applied before the reform. 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; environmental protection activities; among others. In turn, several types of investments can be deducted from the tax, such as R&D investments; investments in agricultural activities; and investments in activities related to the protection of the environment. 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 ad within sectors. Figures 2 and 3 show effective tax rates for 2012 (prior to the reform) for the ten sectors that paid, respectively, the lowest and highest effective rates. (Sectors are defined at the four-digit level of the ISIC sector classification, given the level of disaggregation reported by the tax administration, DIAN). The sectors that paid lowest effective rates for corporate income taxes were, in turn, hit hardest by the introduction of CREE as the reform significantly decreased the number of exemptions that could be claimed.

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[7] The CREE rate later fell to 8%, but this change occurred beyond our estimation timeline.

These different components of the 2012 reform came into effect at different points in time between January 2013 and January 2014. In particular, corporate income from January 2013 onwards was subject to the newly introduced CREE, although casual observation suggests that firms took a few months before they fully understood the new tax and responded to it. SENA and ICBF contributions were eliminated by mid- 2013. Health contributions were eliminated starting in January 2014.

The reform was, therefore, 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 left out of the estimation. We should also note that the CREE was modified again by a reform discussed and passed in December 2014. The latter also introduced a new tax to corporate assets. Our data only covers the first five months of 2014.

Figure 2: Effective corporate income tax rates for sectors with lowest effective taxation
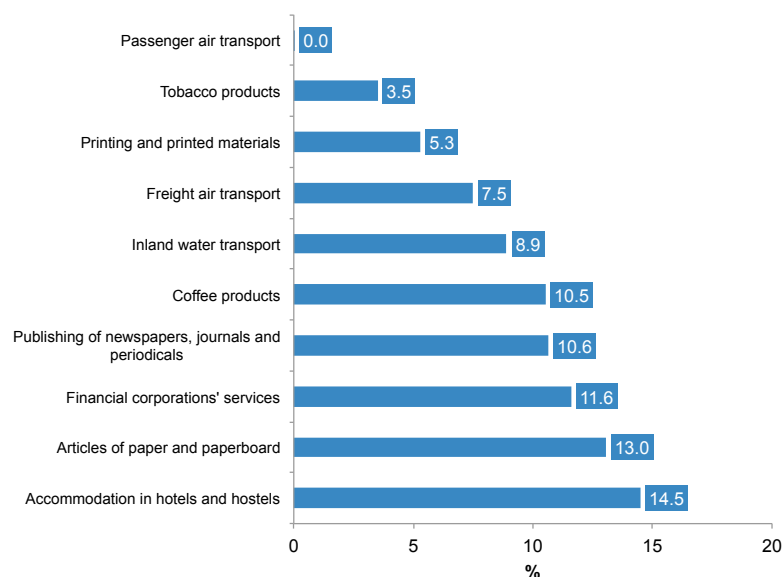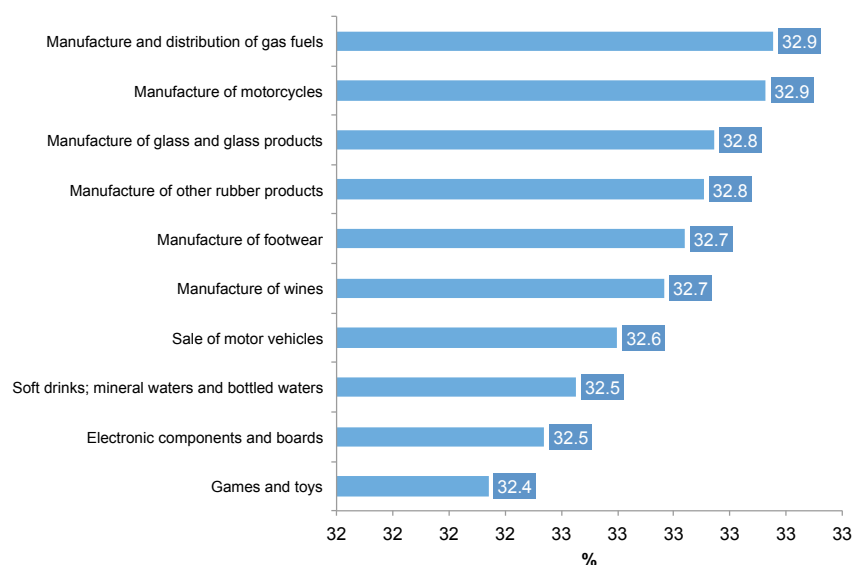

Figure 3: Effective corporate income tax rates for sectors with highest effective taxation

Notes to Figures 2 and 3: Own calculations based on DIAN reports. These reports show aggregates, at the four digit level of the ISIC classification, of corporate financial statements. The effective tax rate is calculated as income tax payments divided by gross income.

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 not subject to corporate income taxes because in their case the mechanism that should have compensated reduced payments of payroll taxes (i.e. greater corporate income taxes via CREE) was not available. This is the case, in particular, of non-for-profit organizations. Thus, firms under the non-for-profit regime constitute a potential control group. Though we cannot directly identify whether a particular employer is non-for-profit, we take advantage of the fact that education is a sector where the non-for-profit regime is known to be prevalent. Tertiary education can only be non-for-profit in Colombia by law, while training programs not leading to a degree, and primary and secondary education, are frequently provided by private institutions legally constituted as NGOs. This comes in a context where the non-for-profit regime is widely abused of: many services, and even goods providers are registered as NGOs to avoid corporate income taxes.[8]

Two concerns are raised by our use of the education sector as a counterfactual. First, to the extent that some of these institutions are in fact non-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 a large part of our control group we do not have information on whether they are private or

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[8] 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 and/or wages as a result of a reduction in labor costs.

public. In any case, as mentioned, anecdotal evidence suggests that much of the private education sector does correspond to institutions registered as non-for-profit that are de facto for-profit.[9]

A second concern relating the use of education as a counterfactual goes in the opposite direction: some institutions in the education sector may be actually registered as for-profit, pay taxes, and therefore be subject to the reform as much as other private firms. We also show the robustness of our result to excluding the education sub-sectors 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.

4. 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 value of capital (K) to solve:

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

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:

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$$L_{it} = \delta(1 - \varphi_{it})(1 + \tau_{it})^{-\hat{\beta}}\mu_{it}$$

where $\hat{\beta} = 1 - \alpha(1 - \gamma) > 0$, $\delta = (1 - \gamma)(1 - \alpha)\left(\frac{\alpha}{(1-\alpha)}\right)^{\alpha(1-\gamma)}$ and $\mu_{it} = \frac{A_{it}^{1-\gamma}D_{it}}{r_t}$.

Taking logs, and using $ln(1 + z) \approx z$ for small z, we can write:

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

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 the tax rate on corporate income and with payroll-specific taxes.

The Colombian 2012 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 whether this is the case.

5. Empirical approach

5.1. Research design

Our baseline estimation uses a standard difference-in-difference 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:

$$Y_{ijt} = \beta_0 + \beta_1 D_i + \beta_2 T_t + \beta_3 * D_i * T_t + u_{ijt} \qquad (1)$$

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 1 if firm *i* is not part of the education or training sector (based on self-reported sector of activity), and equal to 0 otherwise; and T_t is a set of period dummy variables (a period is a month-year combination from January 2014 and onward

We estimate equation (1) introducing firm and time fixed effects, so that coefficients β_1 and β_2 end up subsumed into these effects. That is, we effectively estimate equation (2):

$$Y_{ijt} = \alpha_i + \alpha_t + \beta * (D_i * T_t) + u_{ijt} \qquad (2)$$

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 above and beyond any change that the control group may have experienced over the same period.

To define our treatment dummy D_i we use the sector to which the firm reported to belong in July 2012, before the reform started to be publicly discussed. By setting our baseline 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.

We estimate equation (2) for all firms, and separately for micro firms (<10 employees), small firms (10 to <50 employees), and for medium and large firms (50 or more employees). 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.[10] In the regressions for the medium-large size category we allow for heterogeneous effects for large (over 200 employees) vs. medium firms because of small cell sizes if estimated separately.

Since, as discussed, it may be argued that firms offering education services are not an ideal control group because they follow a different logic than firms in other business sectors, or because they are different in terms of observed characteristics than other sectors, 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 services 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, and to the extent that precisely those firms in education not leading to a degree are more likely than other education establishments to operate as for profit for tax purposes, we also estimate our model in a different restricted sample where we exclude institutions offering programs that do not lead to a degree. We would obviously prefer to take out of our sample only those that do pay taxes as for-profit but unfortunately do not have access to this information, so the closest thing we can do is simply abstract from all institutions not offering formal degrees.

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[10] 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.

Finally, we use Propensity Score Matching in combination with our difference-in-difference approach in order 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.

After testing the robustness of our basic results to different sample definitions and refinements, we assess whether average treatment effects vary by characteristics of firms. In particular, we explore whether effects were stronger for firms that were, arguably, more exposed to the different components of the 2012 tax reform. Fist, more labor-intensive firms could have responded more to the reduction in labor costs. Second sectors previously benefitted by high corporate tax exemptions could have been more affected by 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.

Identifying the effect of the 2012 tax reform (TR) on formal employment is also made challenging by the fact that it partially overlaps in time with a previous reform in effect between January 2011 and December 2014, the "first employment" law (FE), by which 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. The difference-in-difference approach partially deals with the concern that the effects we estimate may be picking up the effect of the FE law rather than the TR law. We also 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 FE reform. Moreover, because our estimation focuses on firms that were active on July 2012, it excludes new employers, who benefit the most from the FE reform. (This does not make the sample balanced, however, as we still have firm exit from July 2012 onward.)

5.2. 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 *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. PILA contains detailed information about all formal workers, whether employed or self-employed, including wage income, and payroll taxes (for employed workers). We use information on the number of workers reported by the firm; the average wage for those workers; the fraction of the payroll corresponding to workers with reported wages below 10 minimum monthly wages; and the sector of activity for the firm. Notice that we only observe formal employment because only formal workers are registered in PILA, so we observe the creation of formal employment.

PILA data have monthly frequency, given that payroll taxes and social security contributions have to be paid every month. The data are available to us from January 2009 to May 2014 though, as stated, we only use information dating back to January 2011. Because the components of the reform entered into effect at different months during 2013, with the public having fuzzy information about this timing,[11] we exclude all information regarding 2013 from the database. By January 2014, all the components of the reform were fully in effect, and employers had had time to understand the implications of the income tax change for their tax payments. Our pre reform period is thus January 2011 to December 2012, and our post-reform period is January 2014 to May 2014. Though information about later periods will soon be available, a new change to corporate taxation from income and property was announced and approved by Congress at the end of 2014. Extending our estimation beyond 2014 would therefore make it

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[11] Though the text of the reform stated the dates at which each of these components was to become effective, part of the payroll tax reduction was effectively put into place before the anticipated dates.

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.

PILA provides an ISIC classification 4-digit code for each firm's sector of economic activity, and identifies each employer using its tax identification number. We use the sector code to distinguish the treatment group from the control group.

We use PILA aggregated at the employer level, for firms with more than one worker.[12] Single-employee-employers were explicitly excluded from the reduction in payroll taxes in the text of the reform. Because of our focus on firms, the self-employed and employers who are individuals (i.e. register personal rather than business tax IDs) are excluded from the database that we use.[13] To keep a focus on employers that should respond to an entrepreneurial logic, we also exclude institutions with sector codes that correspond to public administration; multilateral agencies; unions; providers of outsourced labor; hospitals. Finally, we exclude firms with outliers in employment changes, in particular those that in two consecutive months change size category between medium-large (50+ employees) and micro (10 or less), or between large (200+) and less than 20 employees. Appendix Table A1 describes these different exclusions and how they take the total number of employees in our database in July 2012 from above nine million to a number closer to four million employees.

Figure 4 shows how employment by firms in our dataset has evolved over time during our estimation period.[14] Table 1 provides additional descriptive statistics.

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[12] The Ministry of Health has granted us access to this dataset under restrictive conditions in order to comply with the Colombian data confidentiality regulation. In particular, all individual data are processed directly at the Ministry. No individual-level data are made available to the research team.

[13] A different component of the 2012 reform applied to individuals, and 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, likely important component, is beyond the scope of our investigation, which focuses solely on employment and wages (both for individuals in the firms' payroll) by firms with at least two employees.

[14] 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

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.

Figure 4: Employment by private institutional employers
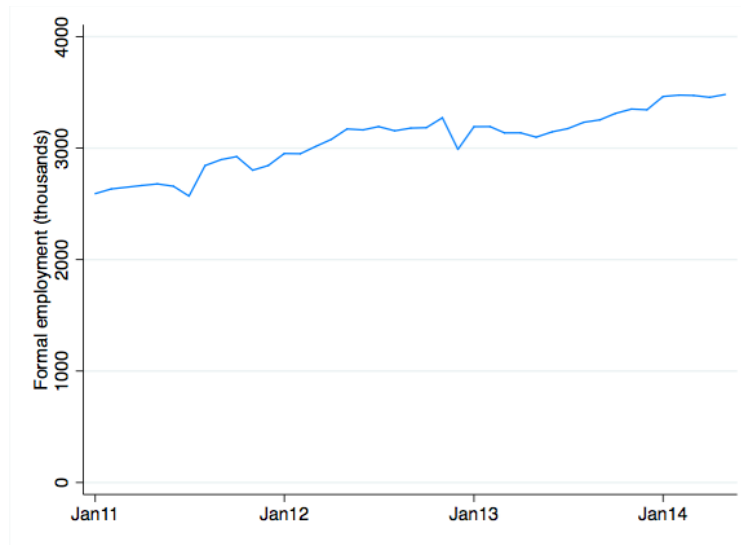
Notes: Own calculations based on the final database described in Appendix Table A1

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% 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.

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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

Variable	Treated firms		Control firms	
	Pre-reform	Post-reform	Pre-reform	Post-reform
	Mean	**Mean**	**Mean**	**Mean**
	(Std. Dev.)	(Std. Dev.)	(Std. Dev.)	(Std. Dev.)
Log Employment	**2.08** (1.39)	**2.18** (1.42)	**2.62** (1.51)	**2.7** (1.56)
Log Wage	**13.66** (0.58)	**13.73** (0.57)	**13.68** (0.46)	**13.74** (0.48)
Employment (No. of workers)	**31** (194)	**36** (236)	**61** (305)	**77** (443)
Average wage (COP$)	**1,038,913** (993,768)	**1,114,873** (1,059,532)	**979,257** (589,318)	**1,046,209** (635,323)

Total employment at baseline	Micro	241,856 (8%)			
	Small	650,103 (21%)			
	Medium+Large	2,252,326 (71%)			

Observations	2,015,609	445,119	71,011	15,301
No. Firms	101,170	91,022	3,333	3,119
No. ISIC 4 digits sectors	304	304	8	8

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 11-month hiring cycle that is common in education, there is no clear pre-reform difference in trends between the treatment and the control group.

Figure 5: Average employment and wages

Employment

Wages

Figure 6: Pre treatment trends

Employment

Wages
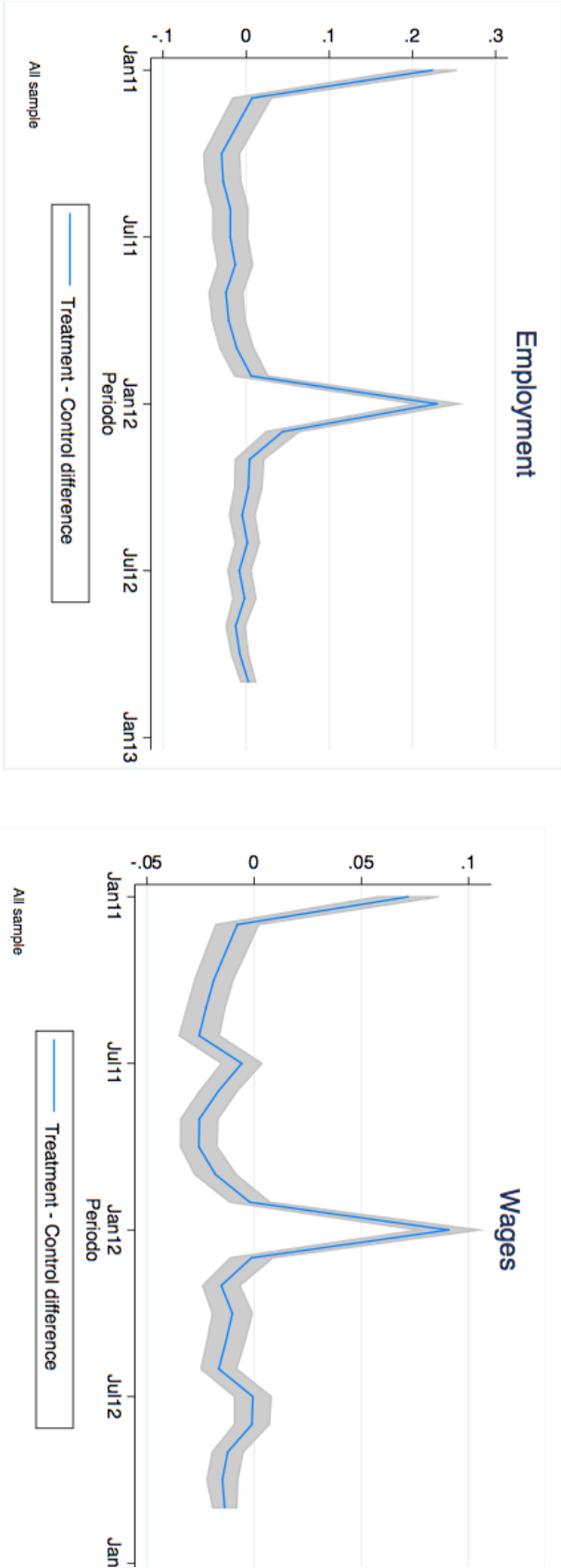
Notes: this figure presents the difference between treatment and control in Figure 5. Shadowed areas: 95% confidence intervals.

6. Estimation results

6.1. Baseline specification

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% and 2.7%, respectively. The effect on employment is mainly driven by micro and small firms, which experienced respective increases of 3.7% and 3.1% with respect to firms in the education sector within the respective firm size category, while no significant change is observed for medium and large firms.[15] 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% and 3.7%, respectively, while the increase for medium firms is around 1.6%.

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

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[15] 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.

Table 2: Effects of the reform on firm employment and wages: baseline specification.

| | Log Employment | | | | | Log Wage | | | | |
	All firms (1)	Micro (2)	Small (3)	Medium and Large (4)	Medium and Large (5)	All firms (1)	Micro (2)	Small (3)	Medium and Large (4)	Medium and Large (5)
Treatment	0.0426***	0.0367**	0.0311**	-0.0267	-0.0289	0.0269***	0.0218***	0.0367***	0.0128**	0.0162***
	(0.00985)	(0.0174)	(0.0147)	(0.0199)	(0.0204)	(0.00361)	(0.00698)	(0.00518)	(0.00567)	(0.00583)
Treatment*(Dummy = 1 if large)					0.00971					-0.0145***
					(0.0168)					(0.00409)
Observations	2,548,240	1,481,014	788,061	279,165	279,165	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	0.022	0.019	0.030	0.026	0.026
Firm fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Equation (2) estimated by OLS with firm and period fixed effects. Standard errors, clustered at firm level, in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

A simple "back of the envelope" calculation of the potential fiscal effect of the reform suggests that the reform partly undid the negative impact on fiscal revenue from reducing payroll tax rates by increasing employment and wages. Using our baseline estimation results for the whole sample, columns 1 and 6 in Table 2, the payroll of the average firm increases by approximately 7%.[16] This increase compensates a good fraction of the 13.5% decrease in the payroll tax rate, for the average firm. On the other hand, however, this compensating effect concentrates on micro and small firms (i.e. the average firm is micro-small), which represent about 28% of employment and 22% of payroll, therefore covering a small fraction of the payroll tax revenue. As a result, only about 11.4% of the forgone revenue from payroll taxation is recovered via an increase in employment and wages.[17] 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, both for the control group and the treatment group. First, we present results for a subsample of sectors in control and treatment that we believe are closest in nature ("Restricted I" in columns 1 and 6 in Table 3). This subsample includes treated firms in I&T consultancy, R&D 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 degree, and private providers of tertiary education.[18] Second, we present results comparing the complete treatment group to control firms in education and training services leading to a formal degree ("Restricted II" in columns 2 and 7 in Table 3). The exclusion of those not leading to formal degree deals with the concern that many of these may actually declare a for-profit status and thus be affected by the

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[16] The 7% increase results from combining the increases in employment and wages: 1.07=1.0426*1.0269.
[17] The 11.4% is the result of calculating (0.07/0.135)*0.22.
[18] For primary and secondary education we lack access to information on private/public ownership.

reform. Third, we also show results of matched difference-in-difference which uses a Propensity Score Matching model where probability of treatment was estimated as a function of firm characteristics observed in July 2012 ("PSM" in columns 3 and 8 in Table 3). We match the samples by using the five nearest neighbors in the treatment group for each firm in the control group. Finally, we estimated the model balancing the sample by excluding the firms that exited PILA after July 2012 (columns 4 and 9). Participation models used in the PSM models, as well as the corresponding density functions and balance tests are shown in the Appendix.

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 those firms that are closer in terms of their economic activities, and limiting control firms to those effectively unaffected by the reform, yields slightly higher effects than those reported in our baseline specification using the complete sample (4.8% and 5% versus 4.7% in the case of employment, and 3.3% and 3.1% versus 2.7% in the case of average wages). The sample obtained using the PSM model, which matches firms 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% and 3.3% for the average firm, and present for firms of all sizes. It also increased employment, in a range of 3.3% to 5% for the average firm, but this effect is concentrated on smaller firms so that in aggregate terms it means little employment creation (about 1% in the average post-reform month in our sample period). In addition to

this important caveat, the employment effect turns out to be more difficult to identify with precision. [19]

We also note that 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 (Appendix Table A2).

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[19] For example, the statistical significance of the estimated employment effect is lost for specific 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 TR. In this matched sample, the positive effect on wages is also positive and significant, but the effect on employment is not statistically significant and 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 11-month hiring cycle similar to that of education, where by construction the immediate effect of the reform (January 2014) is lost. For this reason, we do not treat this as a preferred specification. Still, we note that the effect on employment is not as robust as that on wages.

Table 3: Effects of the reform on firm employment and wages: robustness.

	Log Employment				Log Wage			
	Restricted Sample I	Restricted Sample II	PSM	Balanced sample	Restricted Sample I	Restricted Sample II	PSM	Balanced sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.0480**	0.0497***	0.0338***	0.0351***	0.0327***	0.0310***	0.0238***	0.0218***
	(0.0222)	(0.0107)	(0.0114)	(0.0116)	(0.00907)	(0.0039)	(0.0041)	(0.0038)
Observations	160,750	2,532,171	443,579	1,429,232	160,750	2,532,171	443,579	1,429,232
Adjusted R-squared	0.019	0.008	0.018	0.009	0.031	0.03	0.028	0.037
Firm fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	yes	yes	yes	yes

Notes: Equation (2) estimated by OLS with firm and period fixed effects. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Restricted Sample I: Treatment: I&T consultancy, R&D services, cultural activities, other personal services. Control: private providers of tertiary education, education not leading to a formal degree.
Restricted Sample I: Treatment: all. Control: excluding education and training not leading to a formal degree.
PSM: Probability of treatment at baseline (July 2012) as function of characteristics in July 2012. Estimation sample restricted to control and five nearest treated neighbors.
Balanced sample: all firms reporting to PILA every period between July 2012 and May 2014.

6.2. 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 labor-capital ratio, and by potential exposure to the change in corporate income taxation of the form as measured by the effective tax rate. In particular, we expand specification (2) to allow the effect of the reform to vary with the degree of exposure of treated firms to each if its components, by including interactions of our treatment indicator with exposure measures. We measure exposure to the reduction of payroll taxes through sector-level L/K ratios at baseline (2012), and exposure to the increase in corporate income taxation via with the (inverse of the) sector-level effective tax rate in 2012, TE. We also estimate a version of the model in which L/K ratios are substituted by the share of each firm's payroll represented by workers earning less than 10 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:

L/K exposure: The elasticity of employment and wages to payroll tax reductions depends on how labor-intensive the technology is (1-α in Section 4): employers that use more labor-intensive technologies are expected to increase their payroll more as a result of reduced payroll taxes. Since 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 an administrative publicly-available dataset: the Supersociedades dataset.[20] For these firms, we construct L/K ratios using the capital stock reported to Supersociedades in 2012 and the employment in PILA

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[20] 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% of all formal small firms). In our dataset, 20% of firms present in PILA in 2012 are also found in the Supersociedades data.

in July 2012. We then average across these firms within each four-digit level sector (on the basis of sector code reported in July 2012) to obtain a sector-level average L/K ratio. We apply this ratio to all firms in the sector, as measure of pre-reform exposure to the payroll tax reduction. The use of sector-level—rather than firm-level—K/L is not only imposed by the fact that we lack information on K for most of the sample, but also natural to the extent that through L/K we attempt to measure a characteristic of the technology that is usually though-off as common to firms producing the same goods. It also helps us address concerns about endogeneity of the K/L choice. However, our sector L/K measure is noisy both because it corresponds to a selected sample of firms in each sector and because K comes from financial statements subject to under-reporting 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: Because the change in corporate income taxes corresponds to a reduction in applicable exemptions, it is firms that previously enjoyed those exemptions, or equivalently those with lower effective income tax rates, that are suffered a largest increase in taxation. We, therefore, measure effective tax exposure, TE, for sector *j* as

$$TE_{j,2012} = 1 - \frac{\tau_{j,2012}^{eff}}{\tau_{j,2012}^{nom}} = 1 - \frac{\tau_{j,2012}^{eff}}{0.33} \qquad (3)$$

where $\tau_{j,2012}^{eff}$ is the effective tax rate in 2012 and 0.33 is the nominal corporate income tax rate pre-reform. Sector-level data from the Colombian tax authority, DIAN, are used to calculate the average income tax rates effectively paid by firms in each sector in 2012. The data, which is publicly available through DIAN's website, reports 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 TE is

closer to 1 (the firm claimed more exemptions prior to 2012) and weaker when TE is closer to 0. Effective tax rates for selected sectors were presented in Figures 2 and 3. Because within sectors some firms take more advantage than others of certain exemptions and reductions (e.g. firms in tax-free zones, or those making deductible investments) we would have ideally measured tax exposure at the firm level. Unfortunately, researchers are not granted to firm-level tax data due to reserve regulations.

Heterogeneous effects are estimated for the sample of our baseline specification (Table 2). Results are presented in Table 4 and summarized in figures 6 to 10 for different levels of L/K and TE. 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 L/K ratio or average TE.

Table 4: Heterogeneous effects by pre-reform exposure levels.

VARIABLES	Log Employment			Log Wages		
	(1)	(2)	(4)	(1)	(2)	(4)
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)
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

Notes: equation (2), expanded with interactions, estimated by OLS with firm and year fixed effects. (L/K) and tax exposure at the sector level (4-digits). (L/K) and Tax exposure have ben de-meaned, so that coefficient on "Treatment" is effect at average exposure. Tax exposure calculated as in equation (3). Standard errors clustered at the firm level.

Results for average exposure levels are consistent with those in Table 2. As observed in Figure 6 and Figure 8, the effect on both employment and wages is generally increasing with L/K, even after controlling 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 in general not statistically significant. Something similar can be stated regarding heterogeneous effects by tax exposure (Figures 7 and 8): 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. We thus take our results as only suggestive of heterogeneous effects by degree of exposure in the expected directions. We speculate that this 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 K only for a few selected firms.[21]

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[21] One could also run an alternative heterogeneous effects model substituting the L/K ratios by the share of payroll represented by workers earning less than 10 minimum monthly wages, which are the only workers for which payroll taxes were reduced. However, over 98% of workers and close to 90% of the payroll in our dataset fall in this category, with little variation across firms, placing 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 in our sample yields an estimated overall effect on wages increasing in the payroll fraction for exposed workers, with statistically significant differences between low and high levels of exposure, and no heterogeneous effect in employment.

Figure 6: Heterogeneous effects by ln(L/K).
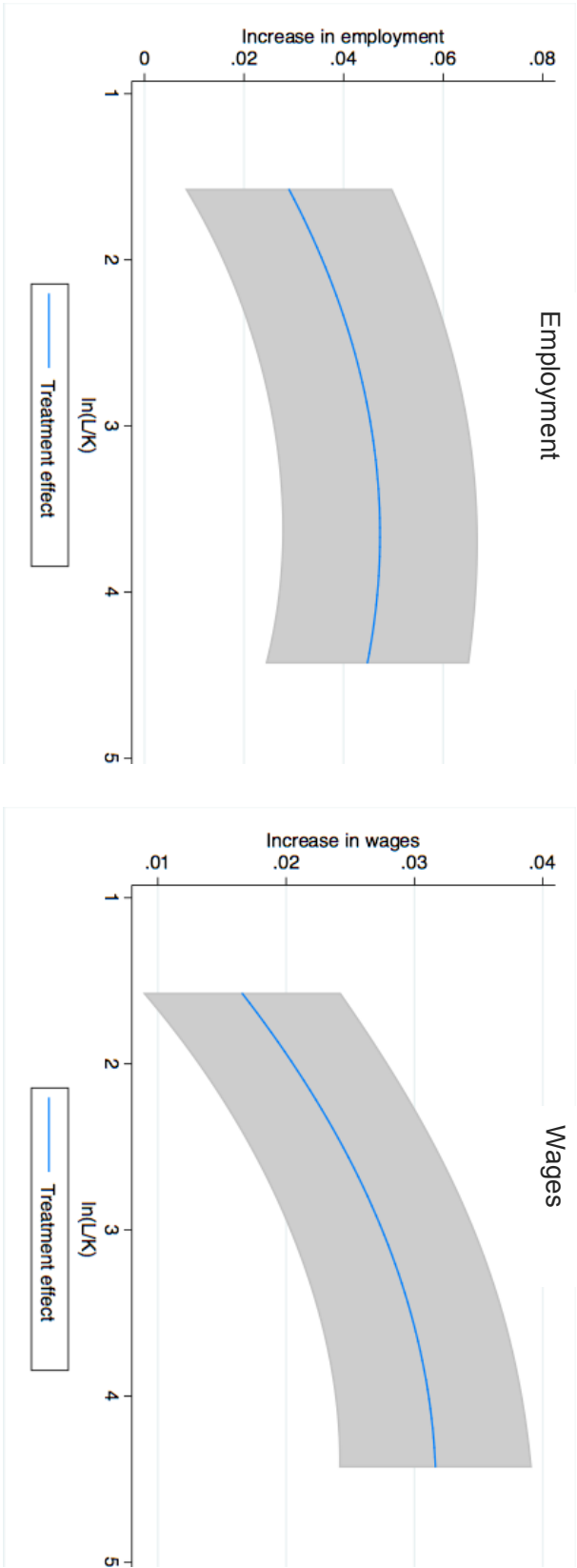
Employment

Wages

Notes: This Figure presents estimated average treatment effects for different levels of L/K, estimated in Table 4, columns 1 and 4. The shaded area represents a 95% confidence band.

Figure 7: Heterogeneous effects by Tax exposure.
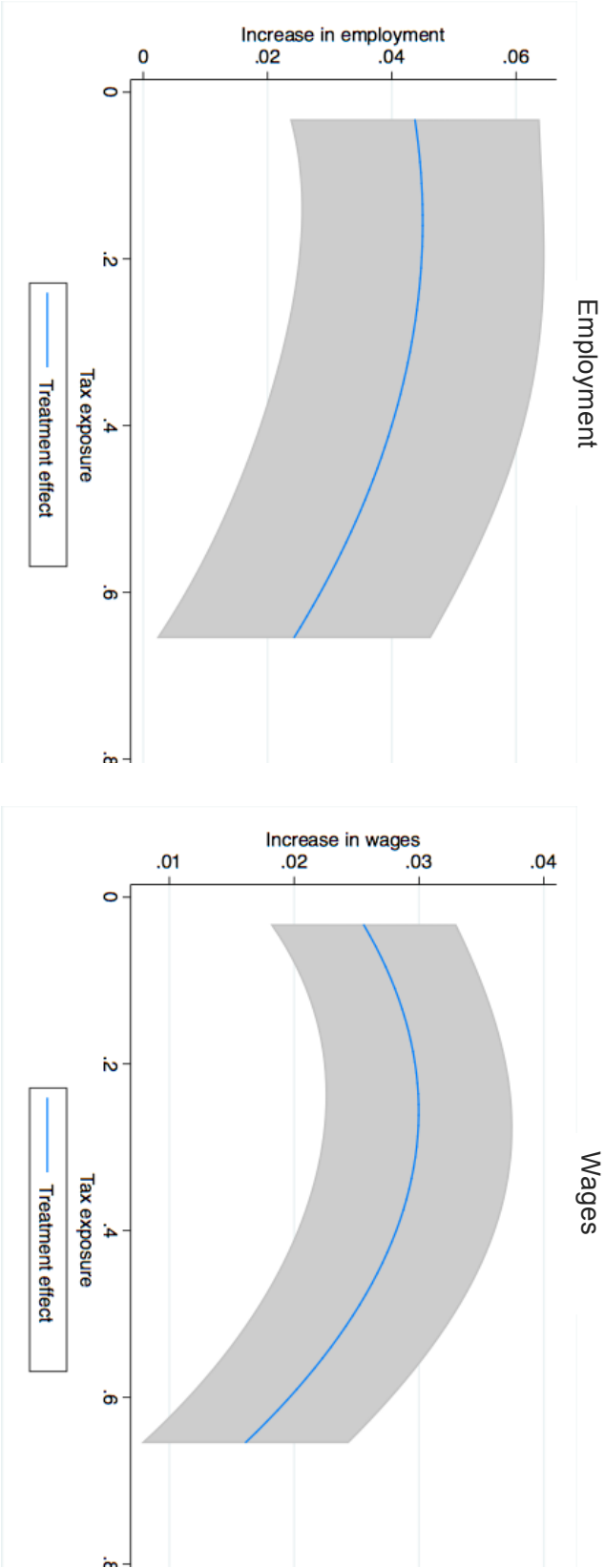
Employment

Wages

Notes: This Figure presents estimated average treatment effects for different levels of Tax Exposure, estimated in Table 4 columns 2 and 5. The shaded area represents a 95% confidence band.

Figure 8: Heterogeneous effects by ln(L/K) and Tax exposure.
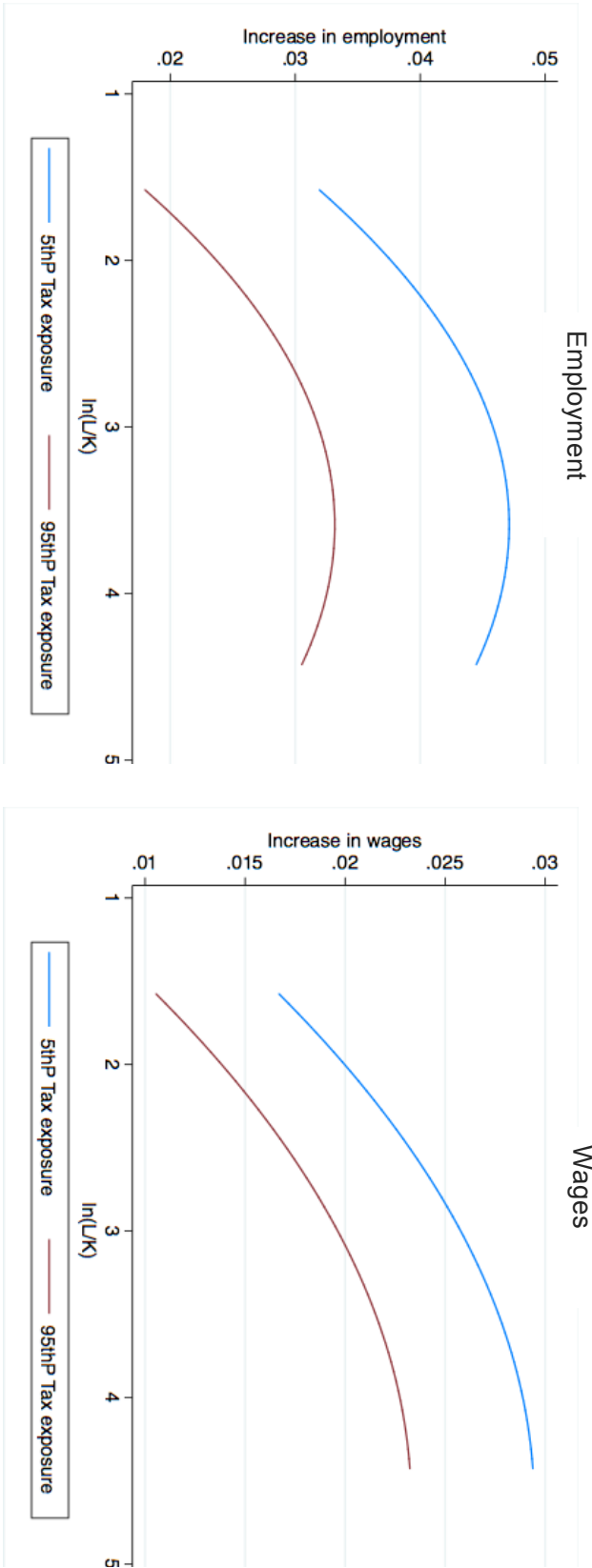
Employment

Wages

Notes: This 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. The shaded area represents a 95% confidence band.

We try alternative, but weaker, heterogeneous effects models on which we briefly comment here (detailed results presented in the Appendix), that are generally supportive of the findings just reported. First, we substitute the L/K ratios by the share of payroll represented by workers earning less than 10 minimum monthly wages, which are the only workers for which payroll taxes were reduced. A major problem with this approach is that over 98% of workers and close to 90% of the payroll in our dataset fall in 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 in our sample estimates an effect on wages strongly increasing in the payroll fraction for exposed workers, with statistically significant differences between low and high levels of exposure, and no heterogeneous effect in employment.

Second, we re-estimate our model by 2-digit sectors of the ISIC classification (revision 3), as an alternative way of approaching the question of whether more labor-intensive sectors respond 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 concentrated on services sectors, including construction, wholesale and retail, transportation, hotels and restaurants, IT&T, financial services, real estate, and professional services. For manufacturing and agriculture we estimate much smaller coefficients of about 1%, statistically insignificant in both cases. Interestingly, for mining, we find a large and negative effect of the reform. Mining is dominated in Colombia by the oil industry, a highly capital-intensive sector.

7. Conclusions

The Colombian 2012 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 level of revenue from firms, but in a way that is less biased against employment. Because of this feature, and unlike the case of 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 in all of Latin America, and likely in other parts of the world.

To take advantage of these unique features of the Colombian 2012 tax reform, we analyze the impact that the reform had on employment and wages. Our findings suggest that employment in micro and small firms is highly sensible to employment-biased taxes, while medium and large firms seem to react mainly to overall taxation, which manifests in our data in the fact that firm-level employment did not increase in this size category. Micro and small firms increased employment by over 3%, implying the creation of 145K new formal jobs by businesses that employ at least two workers in the first five months after the reform. On the other hand, wages increased in firms of all size categories, in a magnitude close to 2.7% for the average firm.

While these findings are supportive of additional efforts to reduce payroll taxes, they also raise concerns about employment in Colombia being highly sensible to the overall taxation faced by the firm. This sensitivity to taxes is probably not independent of the fact that businesses in Colombia face an extremely high overall tax rate (over 70%, according to the World Bank's Doing Business report). In the current context of reduced tax revenue due to falling oil prices, these findings suggest extreme caution against further increases in corporate taxes

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Appendix

Table A 1: Steps to obtain dataset used in estimation

Step	Database	Number of observations	Number of firms	Total employment 2014-5
1	Pila 2009-1 to 2014-5 without duplicates. Aggregated by employer ID	9,730,804	426,191	8,310,883
2	After dropping government and hospitals	9,462,386	416,580	7,913,132
3	After dropping unions, multilateral agencies and labor outsourcing firms	7,273,782	341,253	6,183,637
4	After dropping employers who are individuals	5,890,622	257,598	5,901,029
5	After dropping firms reporting only one worker, that cannot benefit from the payroll reform, and firms with jumps in employment from one year to the next, from one month to the next or every two months. A jump in employment is defined as going from more than 200 workers to less than 20, or going from more than 100 workers to less than 10, or vice-versa.	4,125,173	113,043	3,909,936
6	After dropping 2013	2,873,925	113,042	3,909,936
7	After dropping firms that did not exist in July 2012	2,548,240	104,551	3,481,177

Table A 2: PSM Model: Propensity Score Matching model and balance tests

Variable	Matched	Mean		%bias	% reduct |bias|	t-test	
		Treated	Control			t	p>|t|
$Log(L)$	Unmatched	2.632	2.015	41.7		25.26	0
	Matched	2.632	2.683	-3.5	91.70%	-1.31	0.191
$Log(Average\ Wage)$	Unmatched	13.68	13.639	8.2		4.16	0
	Matched	13.68	13.685	-0.5	93.90%	-0.22	0.827
$L\ MinWage/_L$	Unmatched	0.38	0.442	-15.7		-8.6	0
	Matched	0.38	0.393	-3.3	78.90%	-1.39	0.166
$L < 10*MinWage/_L$	Unmatched	0.994	0.986	14.7		6.65	0
	Matched	0.994	0.993	2.4	83.80%	1.4	0.16
$Payroll\ MinWage/_{Payroll}$	Unmatched	0.326	0.383	-14.7		-8.18	0
	Matched	0.326	0.342	-4	73%	-1.67	0.094
$Payroll < 10*MinWage/_{Payroll}$	Unmatched	0.979	0.96	16.4		7.8	0
	Matched	0.979	0.975	3.5	78.90%	1.82	0.068
$Average\ \Delta L$	Unmatched	0.0103	0.0058	8		4.2	0
	Matched	0.0103	0.0103	0	99.50%	-0.02	0.986
$Average\ \Delta Wage$	Unmatched	0.00693	0.0072	-0.5		-0.26	0.796
	Matched	0.00693	0.00696	-0.1	89%	-0.03	0.978

Figure A 1: PSM Model: Distributions of Propensity scores.
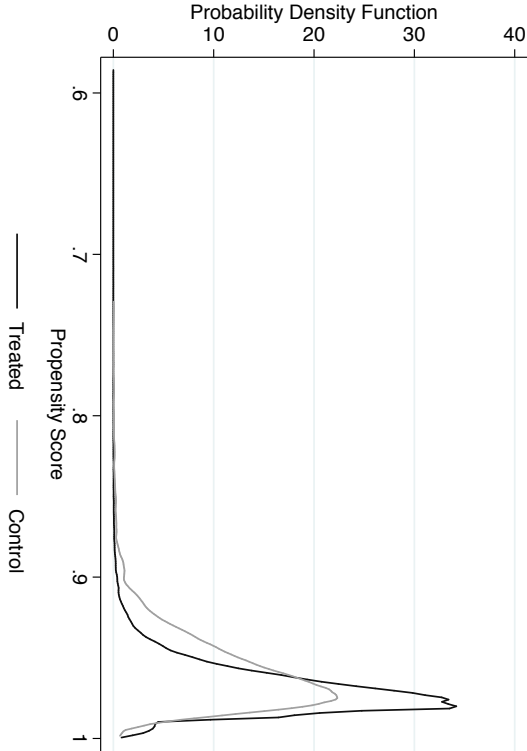
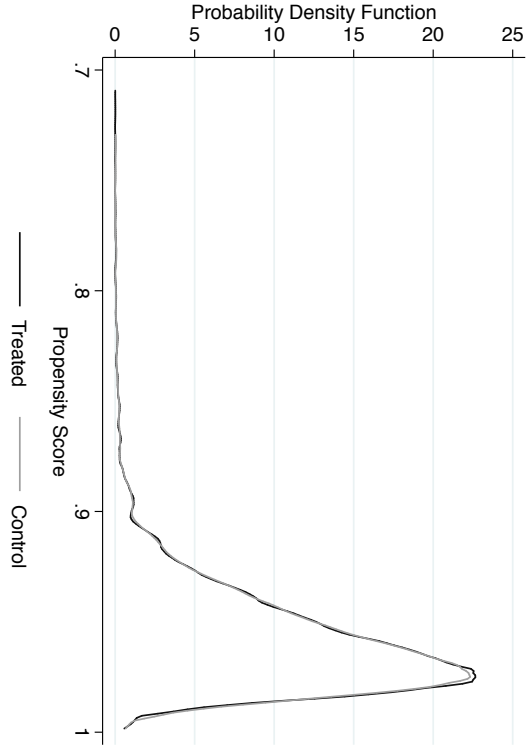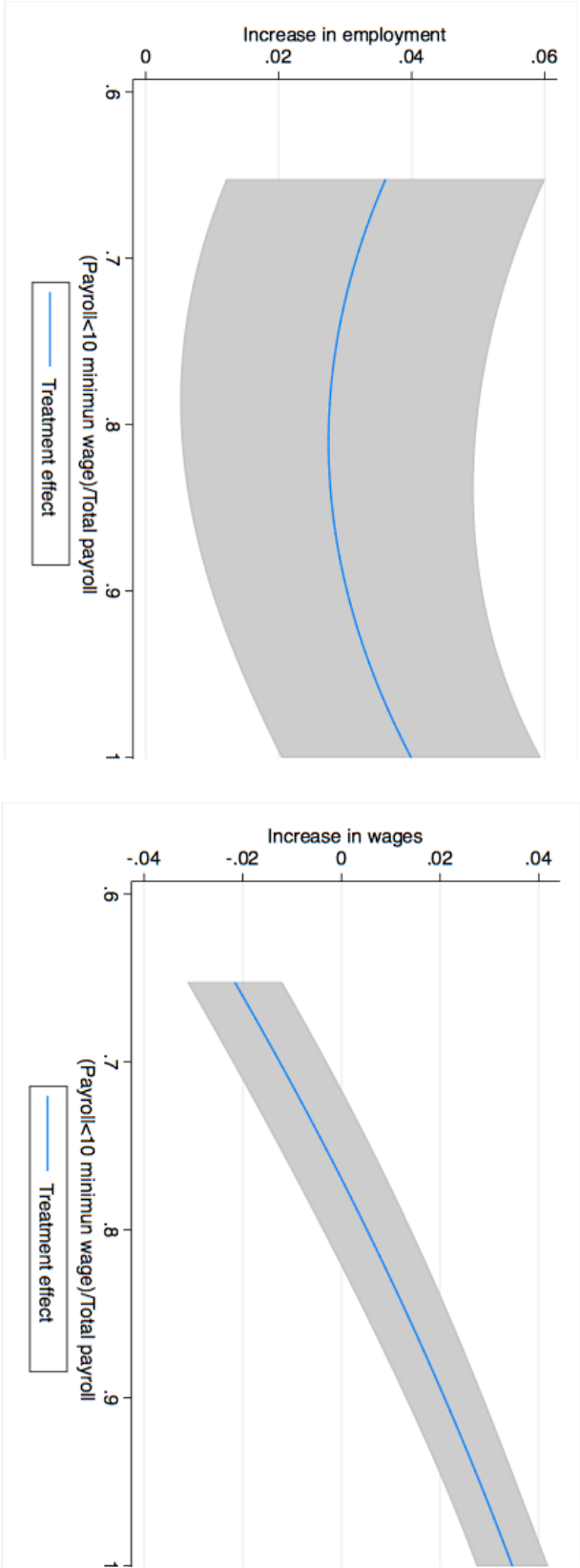Figure A 2: Heterogeneous effects by fraction of the payroll represented by workers who earn under ten minimum monthly wages.

Notes: this Figure shows estimated treatment effects for different levels of fraction of the payroll represented by workers who earn under ten minimum monthly wages. Estimates are obtained replacing (L/K) by (Payroll<10MW / Payroll) in Table 4. Shadowed areas are 95% confidence bands.

Table A 4: Heterogeneous effects on log employment by 2-digit sector of activity

Dependent variable: Log employment	Agriculture, forestry and fishing	Mining and quarrying	Manufacturing	Energy supply	Water supply and related activities	Construction	Wholesale and retail trade	Transportation and storage
Treatment	0.0154	-0.0872***	0.0111	0.00447	0.0739***	0.0374**	0.0377***	0.0547***
	(0.0157)	(0.0318)	(0.0110)	(0.0555)	(0.0225)	(0.0168)	(0.0103)	(0.0149)
Observations	144,549	115,864	443,467	91,981	107,204	208,996	728,218	185,953
Adjusted R-squared	0.018	0.020	0.006	0.036	0.030	0.007	0.010	0.016

Dependent variable: Log employment	Hotels and restaurants	Information and communication	Financial and insurance activities	Real estate activities	Professional, scientific and technical activities	Administrative and support service activities	Arts, entertainment and recreation	Other service activities
Treatment	0.0544***	0.0728***	0.0618***	0.0374***	0.0889***	0.0375	-0.0565	0.0197
	(0.0177)	(0.0201)	(0.0176)	(0.0137)	(0.0131)	(0.0734)	(0.0400)	(0.0161)
Observations	135,429	126,484	127,914	159,010	198,928	90,706	95,563	138,817
Adjusted R-squared	0.022	0.026	0.025	0.019	0.022	0.037	0.034	0.020
Size	All	All	All	All	All	All	All	All
NIT FE	yes	yes	yes	yes	yes	yes	yes	yes
Period FE	yes	yes	yes	yes	yes	yes	yes	yes

Equation (2) estimated by OLS with firm and period Fixed Effects. Standard errors clustered at the firm level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A 5: Heterogeneous effects on log wage by 2-digit sector of activity

Dependent variable: Log wage	Agriculture, forestry and fishing	Mining and quarrying	Manufacturing	Energy supply	Water supply and related activities	Construction	Wholesale and retail trade	Transportation and storage
Treatment	0.00131	-0.0102	0.0133***	-0.0120	0.00137	0.0666***	0.0204***	0.0281***
	(0.00593)	(0.0122)	(0.00401)	(0.0189)	(0.00820)	(0.00647)	(0.00383)	(0.00559)
Observations	144,549	115,864	443,467	91,981	107,204	208,996	728,218	185,953
Adjusted R-squared	0.027	0.027	0.023	0.043	0.036	0.025	0.023	0.026

Dependent variable: Log wage	Hotels and restaurants	Information and communication	Financial and insurance activities	Real estate activities	Professional, scientific and technical activities	Administrative and support service activities	Arts, entertainment and recreation	Other service activities
Treatment	0.0164***	0.0300***	0.0339***	0.0437***	0.0521***	0.0438	0.00706	0.0214***
	(0.00579)	(0.00908)	(0.00900)	(0.00653)	(0.00578)	(0.0312)	(0.0102)	(0.00601)
Observations	135,429	126,484	127,914	159,010	198,928	90,706	95,563	138,817
Adjusted R-squared	0.030	0.032	0.030	0.034	0.033	0.043	0.042	0.032
Size	All	All	All	All	All	All	All	All
NIT FE	yes	yes	yes	yes	yes	yes	yes	yes
Period FE	yes	yes	yes	yes	yes	yes	yes	yes

Equation (2) estimated by OLS with firm and period Fixed Effects. Standard errors clustered at the firm level in parentheses. *** p<0.01, ** p<0.05, * p<0.1