# The Incidence of Payroll Taxation

Felipe Lobel *

April 2021

[VERY PRELIMINARY - PLEASE DO NOT CIRCULATE]

**Abstract**

I study a corporate tax reform in Brazil that reduces eligible firms' payroll tax liability from an average of 31% to 12% of their total wage bill. The tax cut generates a 10% increase on employment and zero effect on wages. However, the reform deteriorates within firm wage inequality. Results are not driven by formalization. Variation on the treatment intensity allows me to compute the employment elasticity with respect to labor cost, which is approximately -0.67.

*UC Berkeley, lobel@berkeley.edu. I am indebted to Emmanuel Saez, Patrick Kline, Alan Auerbach and Gabriel Zucman for their guidance and encouragement on this project. I would like to thank Chris Campos, David Card, Fred Finan, Patrick Kennedy, Valdemar Neto, Ted Miguel, Conrad Miller, Jesse Rothstein, João Thereze, Ricardo Perez-Truglia, Reed Walker and Chris Walters for very helpful comments. The findings expressed in this paper are solely those of the author and do not represent the views of any other institutions.

# 1 Introduction

Payroll tax cuts have been implemented by many countries[1] to reduce labor costs (OECD, 2019). This policy is motivated by the classical assumption on aggregate labor demand being more elastic than labor supply, which suggests that workers bear payroll taxes. Indeed the 2018 Congressional Budget Office (CBO) relies on this assumption to predict the impact of payroll taxes. However, in the policy space there is controversy around such a type of stimulus. Some may argue that payroll tax cuts favor firm owners, who do not share benefits with workers. At the center of this debate is the open question: what is the incidence of payroll taxes?

The literature provides mixed estimates for the incidence of payroll taxes (Saez et al 2019; Saez, Matsaganis, and Tsakloglou 2012; Gruber 1997; Mondino and Montoya 2004; Hamermesh 1979; Kugler et al 2017). Definitive conclusions on the payroll tax incidence on overall employment are scant for two main reasons. First, most payroll tax cut programs are targeted to specific workers (based on earnings, tenure or age), imposing additional difficulties to isolate constraints due to firm pay equity norms. Second, payroll tax cuts are typically implemented during crises, in which case the discriminatory eligibility can be useful to create control groups able to isolate macro shocks operating during economic recessions.

In this paper I explore a unique sector and product specific payroll tax reform in Brazil that addresses both of the previous concerns. The reform was applicable to all workers in eligible firms. The sector and product eligibility criteria provide clear control groups to isolate macro and time trend shocks. Average payroll tax rates in Brazil is 31% of the total wage bill. Eligible firms were able to substitute 20 percentage points on the payroll tax rate for a 1-4.5% tax on net of exports gross revenue. The motivation for the reform was to lower labor cost aiming to increase employment and competitiveness. A central characteristic of the reform is the imperfect take-up rate. Technically, treatment was optional in treated groups only after December, 2015. But in practice, there was no enforcement to participation, and compliance was not perfect since the early years of the program. The reform was valid for many years and created relevant tax benefits to certain firms, as can be depicted in figure 4.

In section 3, I present a difference-in-differences (DD) model to compute the average treatment effect of the payroll tax cut on participant firms. One caveat to the design is to deal with imperfect compliance, i.e. firms in treated sectors do not present perfect take-up and firms in non-treated sectors could be eligible due

[1] US, Brazil, Chile, Italy, Colombia, Greece and Sweden are recent examples. This is not an exhaustive list.

to the product criteria. I use firm level tax data instrumented by sector treatment assignment to adjust for imperfect compliance. Even though this is the first paper to use firm level tax data to analyze the Brazilian payroll tax reform, there are other studies on this policy (Dallava 2014; Scherer 2015)[2] and more recently Baumgartner et al. The latter assumes a perfect take-up rate on treated sectors and restricts the sample to a few sectors that are more likely not to be affected by the product eligibility criteria. Both of these assumptions don't match firm level tax data[3]. In terms of results, Baumgartner et al. find a 5% employment increase, Scherer 2015 finds 15% increase, while Dallava 2014 finds positive employment effects in only a few subsectors of the IT sector.

The main advantage of firm level tax data is to precisely observe treatment assignment in both eligible and non-eligible sectors, which allows for non-compliance adjustment. The combination of tax and labor market data provide an unique laboratory to study the incidence of payroll taxes on the labor market. Compared to the related literature on this reform, my analysis is also more comprehensive in two other aspects: (1) This paper considers the vast majority of economic sectors in Brazil, while previous work are restricted to a few specific sectors. (2) I analyze the reform from its beginning until recent years, while other studies were restricted to the three initial years of the program. I find an employment increase of 10% and a virtual zero wage increase due to the tax cut. There is extreme inequality in the distribution of wage gains. At the firm's top 1% earners, the wage treatment effect is 4%, while it is close to zero at the bottom of the distribution.

The results are robust to other identification strategies. Alternatively to the fuzzy DD approach, I also implement a matching DD strategy. Each treated firm is matched to a non-treated that belongs to the same deciles of employment, wages and hiring in the pre-reform period. Ties are broken with a propensity score. I perform a few robustness checks on the matching process. First, I match in multiple pre-reform periods. Second I run a placebo test, in which I randomly assign firms to the treatment group and, then I follow the matching algorithm based on this fake treatment assignment. As expected I find zero results when this placebo is implemented. The results are qualitatively similar in both fuzzy and matching approaches.

The rest of the paper is organized as follows. Section 2 presents the institutional

[2]Scherer 2015 uses firms in the "Simples" tax regime as control group. This is plausible because firms in the "Simples" regime were not eligible to the tax reform, but this approach doesn't account for relevant migration between the "Simples" and the regular regime.

[3]Even in public available sector level tax data published on the Tax Administration website it is possible to observe that take-up rate is not perfect in treated sectors and that there firms treated due to the product criteria in sectors that remain in Baumgartner et al.'s sample.

background and data sources. Section 3 presents the main empirical findings on the extensive margin responses. Section 4 discusses rent sharing and distributional implications of the tax reform. Section 5 computes relevant elasticities. Section 6 discusses the transition to employment, and section 7 concludes.

# 2 Institutional Background and Data

## 2.1 Payroll Tax Reform in Brazil

The Brazilian payroll taxes are designed to fund social security benefits, such as retirement pensions and unemployment insurance. The payroll taxes have three components, all of them are collected directly from firms. The main component is a 20% flat tax over the total wage bill, which is deposited in the social security fund, but doesn't directly affect specific workers' benefits. Secondarily there is an accident risk insurance that varies between 1% to 3%[4]. The last layer of contribution is a 8% to 11% tax on wages, which is employee specific and can vary within workers of the same firm. On $14^{th}$ December, 2011 the Brazilian Federal Government announced the payroll tax cut program[5] that imposed a substitution of the 20% flat payroll tax to a 1% to 2.5% taxes on net of exports gross revenue. Eligibility for the payroll tax exemption is sector and product specific. The reform applied in December, 2011 to a few Information and Technology sectors, but for most of the contemplated sectors, it applied in April, 2012.

Several bills[6] were passed including and excluding sectors from the reform.[7] The tax bills define eligible sectors and products according to the Mercosur Common Nomenclature (NCM). Most of the product eligible firms are in the manufacturing industry, but treatment due to NCM criterion is not restricted to the manufacturing sector. Indeed, vast majority of sectors in the Brazilian economy contain firms treated due to the product (NCM) criteria[8]. Treatment due to the NCM eligibility criterion only allows for partial payroll tax waive, according to the share of eligible products in the firms' gross income. The payroll tax cut program is still valid nowadays (2021), but there were changes in the policy since it was first created.

[4]This tax varies according to the activity associated risk

[5]Law 12546/2011 approved by the Congress confirms Executive bill 540/2011 passed on August $2^{nd}$, 2011.

[6]Law 12546/2011, Law 12715/2012, Law 12844/2013, Law 13161/2015, Law 13202/2015, and Law 13670/2018.

[7]IT, Call Center and Hotels were added in 2012. Retail, Construction and Maintenance were added in 2013. And a final wave in 2014 added Transportation, Infra-structure and Media sectors.

[8]This can be observed in the micro tax data, but also in sector level data publicly available on the tax authority website.

After the initial tax bill in 2011 there were 5 other bills on the same reform. Most of them promoted marginal changes to the program, such as modifying the revenue tax rates, or adding new sectors to the policy. One of the most relevant changes happened inn December, 2015 when the policy became less generous as the revenue tax rates increased from 1-2.5% to 1.5-4.5%[9]. At that moment, treatment assignment also became optional, which in practice is not a relevant change in the regime because even in the early years of the reform there was not perfect take-up in the treated sectors.

The legislative decision process to define eligible sectors was political, and didn't seem to anticipate sector specific labor outcome trends. I show this in figures 1 and 6 where I test the parallel trends assumption. A few patterns on the legislator choice of eligible sectors confirm that eligibility looks as good as random. For example, hotels were assigned to treatment, while motels were not. Finally, the reform was not intended to increase deficits in the social security system. The Federal Treasury committed to cover any potential losses to the social security system. This is to say that the reform didn't affect individuals' perception on the solvency of their retirement plans.

A central aspect of the reform that deserves more discussion is the take-up rate. A few facts can rationalize the imperfect take-up rate: (1) The tax bills never mentioned any punishment to non-compliers. Possibly because from the legislative point of view treatment was seen as beneficial to firms. Based on the Brazilian tax code it is implausible for prosecutors to suit firms that don't opt in a supposedly beneficial tax system. (2) Enrollment in the program was not automatic as in the Swedish case studied by (Saez et al, 2019)[10]. In Brazil, firms have to inform eligibility on Government provided software to enable tax exemptions[11]. In order to compute the final benefit, there are separate tax forms that are required to be filled out. Figure 5 illustrates tax forms instructions and the set of information requested in the tax platform. Even though, the tax substitution implied a net tax cut in most cases, the operational filling process can generate non responsiveness even in dominated tax regions, where firms would have net benefits for joining the program. Non-responsiveness is consistent with empirical findings in other countries (Kleven and Waseem, 2013).

[9] Law 13.161/2015

[10] In Sweden firms filled the same tax forms before and after the reform. Once firms provided information on their employees, the Tax Authority was the one computing firms' tax benefits.

[11] Firms inform eligibility on block 0 and this enables block P where tax relevant information are input.

## 2.2 Data and Descriptive Statistics

**Tax Data.** The firm level tax data comes from three sources. (1) Tax form on total firm level payroll taxes, namely *Guia da Previdência Social* (GPS). This form doesn't differentiate the three components of the payroll tax bill. It only informs the total amount of payroll taxes paid per firm. (2) Tax form in which firms report their total wage bill to the tax administration. This form is called *Guia de Recolhimento do Fundo de Garantia do Tempo de Serviço e Informações à Previdência Social* (GFIP). (3) Tax form on total revenue tax liability, namely *Contribuição Previdenciária Sobre a Receita Bruta* (CPRB). The net of exports gross revenue tax is triggered due to treatment, i.e., payroll tax waive is the only feature that qualifies firms to pay CPRB. Therefore, the revenue tax data is used not only to compute the firm's total tax liability, but also to indicate whether they were treated or not. Each observation consolidates all the information from subsidiaries that a firm might have, and it spans the period from 2008 to 2017 on an annual frequency.

**Labor Market Data.** For labor market data I use Relação Anual de Informações Sociais (RAIS), which is the matched employer-employee data set administered by the Ministry of the Economy. This data set provides worker level information covering every formal labor contract signed between 2008 and 2017. It provides workers employment status and wages at December of each year. This allows me to compute firm's employment and average wage per year. We can match this information with the tax data based on the firm's identification number. RAIS also contains background information such as worker's age, race, education. On the firm's side it contains information on its tax regime[12], sector and age. These information are useful to add controls in the regression specification.

**Sample.** Merging the tax and labor market data leads to detailed data on the universe of all formal firms operating in Brazil between 2008 and 2017. I exclude from this sample firms that belong to the "Simples", which is a special tax regime for small firms[13]. Firms in the "Simples" regime face a different tax tier which consolidates all tax liability in a single tax form with lower rates. Therefore, these firms are not eligible for the tax reform under analysis and neither are comparable to the firms in the regular tax tier. There are also two broad defined sectors (at

[12]There is a simplified tax regime ("Simples Nacional") targeted to small firms that are not subjected to the payroll tax cut under analysis.

[13]The current gross revenue eligibility threshold is BRL 4.8 millions (around USD 1 million).

the one digit level[14]) that are excluded from the analysis. Construction is excluded because the treatment assignment to this sector was atypical. The law allowed construction firms to be treated in only certain construction sites, according to the site's license date. This makes some of the construction corporations being partially treated, and, therefore, even with the firm level data it is not possible to observe the responses in treated sites. Even if it was possible to observe the construction site level of granularity, this could be confounded by spillovers from non-treated sites within the same firm. Also, construction was at the epicenter of the "Car Wash" operation, a massive corruption scandal revealed in the decade of this study, which revealed that economic transactions on that sector were not responses to standard economic incentives, but to illegal business negotiations.

Repair and sale of motor vehicles were excluded to avoid lurking effects with other tax benefits conceived to these sectors in the period of analysis. These sectors are excluded at the one-digit (broadest) level, which is helpful because it eliminates both treated and non-treated subsectors in these industries. The remaining 19 one-digit sectors of the Brazilian economy remain in the analysis. As standard in this literature, I winsorize outliers at 5% and 95% on the variables of interest (employment and wages). In robustness checks, I show that results are not sensitive to the winsorization process. Finally, I keep a balanced panel of firms in order to avoid their entrance and exit, to be a confounding factor. The focus of this study is the firms' labor adjustment to tax variation.

**Descriptive Statistics.** The baseline sample focuses on firms in the standard tax regime and contains a balanced panel of 1,864,070 firm $\times$ year observations. Firms are distributed over 611 seven-digit sectors, which is the thinnest sector definition according to the Statistics Bureau (IBGE[15]). On average, the firm's total employment on December $31^{st}$ of each year was 19.87 workers, which generated an average wage bill of \$31,795 BRL (approximately \$6,359 USD) per month. Each firm hired on average 8.66 workers per year and the labor force is 59% male. In terms of educational background, 48% have completed high school or higher degrees. The average worker age is 40.61 and 59% of the labor force is white. Detailed descriptive statistics can be found on table 1.

[14]Sectors are defined according to Classificação Nacional de Atividades Econômicas (CNAE), which is administered by the National Statistics Bureau.

[15]Instituto Brasileiro de Geografia e Estatística

# 3 Extensive Margin Responses

In this section I analyze employment and wage consequences of the payroll tax reform. Once firms receive an unexpected exogenous income shock, it is not obvious what the labor market responses are going to be. One possibility is that firms pass-through to workers the received benefits. Another scenario is that firms simply pocket the subsidy. From the policy perspective, there are no enforcement conditions attaching the firms' benefit to workers' pass-through. To answer this question I explore the extensive margin variation created by the policy, i.e., some sectors were exogenously assigned to treatment[16], while others were not. I use the sector treatment assignment as an instrument to adjust for the take-up rate, and treatment in outside sectors due to the product (NCM) eligibility criteria. The first stage regression is given by equation 1.

$$D_{jt} = \alpha_j + \gamma_t + \xi_{s1,t} + \pi L_{s(j)t} + X_{jt} + u_{jt} \tag{1}$$

where, $D_{jt}$ indicates if firm j was treated in year t; $L_{s(j)t}$ indicates if firm j belongs to a sector that is eligible for treatment in period t; $X_{jt}$ are set of controls such as education, race, age and its square; $\xi_{s1,t}$ is 1-digit sector $\times$ year fixed effect.

The first stage coefficient $\pi$ inflates as the take-up rate on treated sectors increases, and deflates as there are more treatments occurring due to NCM criteria in non-treated sectors. The associated reduced form is expressed in equation 2. Standard errors are clustered at the firm level.

$$Y_{jt} = \alpha_j + \gamma_t + \xi_{s1,t} + \delta L_{s(j)t} + X_{jt} + u_{jt} \tag{2}$$

Table 2 presents the estimates. Results can be interpreted as a causal effect of the policy, i.e., controlling for firm characteristics, receiving the treatment generates an employment response of 10% and virtual zero wage increase. Next, I evaluate the dynamics of the treatment effects in an event study specification.

## 3.1 Event Study

The main hypothesis in the DD model is the lack of pre-trends on employment and wages. A threat to this identification strategy is sector specific trends on employment anticipated by the Government, when choosing eligible sectors. I discuss in section 2.1 the political process involved in the policy design and the reasons why it seems not to anticipate sector specific trends. The event study design allows

[16]Discussion on the exogenous treatment assignment can be found on section 2.1

to formally test the parallel trends hypothesis. It also provides estimates for the treatment effect according to the time relative to treatment. I combine the event study set up and the 2SLS framework to estimate the average treatment effect on compliers. The first stage and reduced form are given by equations 3 and 4, respectively.

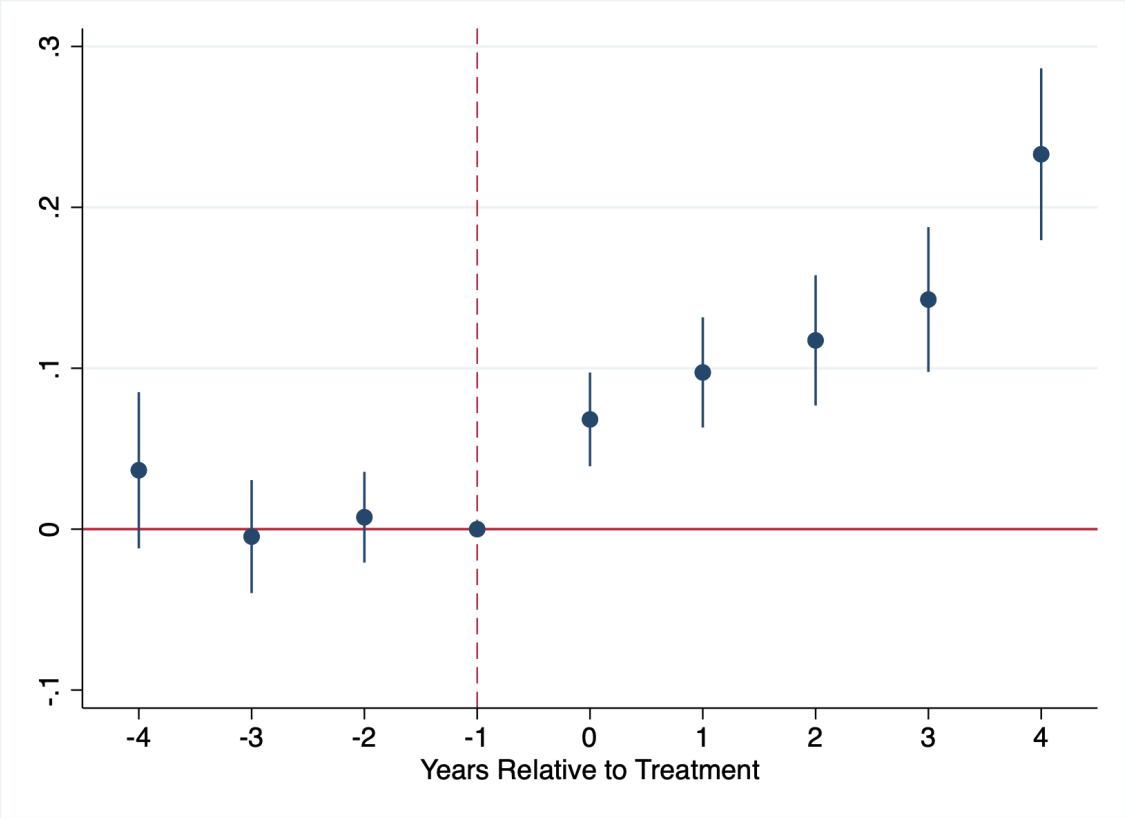
$$D_{jt}^{k} = \sum_{\underline{C}}^{k=-2} \delta_k L_{s(j)t}^{k} + \sum_{k=0}^{\bar{C}} \delta_k L_{s(j)t}^{k} + \alpha_j + \gamma_t + \xi_{s1,t} + X_{jt} + u_{jt} \tag{3}$$

where $D_{jt}^{k} = 1$, if firm j is k years away from entering the treatment at time t, i.e., $t = e_j + k$ and $e_j$ is the date when the firm enters treatment for the first time. Analogously, $L_{s(j)t}^{k} = 1$, if $t = e_{s(j)} + k$ and $e_s(j)$ is defined as the date when firm j's sector becomes eligible.

$$Y_{jt} = \sum_{\underline{C}}^{k=-2} \delta_k L_{s(j)t}^{k} + \sum_{k=0}^{\bar{C}} \delta_k L_{s(j)t}^{k} + \alpha_j + \gamma_t + \xi_{s1,t} + X_{jt} + u_{jt} \tag{4}$$

where $Y_{jt}$ represents the log of firm j's labor market outcome in year t; $\alpha_j$ is the firm fixed effect, and $\xi_{s1,t}$ is 1-digit sector $\times$ year fixed effect. I keep the same set of controls on education, race, age and its square. I impose endpoints on $\underline{C}$ = -4 years, and $\bar{C} = +4$ years relative to entering the treatment. Results are consistent with equations 1 and 2, and are summarized on figures 1 and 6 that show the evolution of the average treatment effects. Conditions for the LATE Theorem hold, thus IV estimates can be interpreted as average causal effects of tax cuts on employment and wages for compliers. The wage regressions are weighted based on pre-reform firm size. The time invariant weighting provides a worker level interpretation to the wage effects.

Figure 1: Employment: Event Study Estimates




*Note:* This figure presents the event study estimates for employment. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans four years prior to entering the payroll tax cut program and four years after.

In the appendix, I repeat the extensive margin analysis with a different identification strategy. Instead of using an instrument, I rely on matching DD. For this alternative specification, I match each eventually treated firm to one never treated firm. The matching algorithm goes as follows. First I match firms that belong to the same deciles on employment, wages and number of hires per year. A propensity score fitted in the pre-reform years is applied to break eventual ties. I run a few robustness tests in the matched sample. In one of them I assign placebo treatment at random and follow the same matching process to the placebo treated firms. As expected, the placebo tests generate zero employment and wage effects, providing evidence that the results are responses to the actual treatment, instead of any inconsistency in the matching algorithm. In another test, I analyze the balance on the matched sample, by comparing treatment and control pre-reform characteristics. This test suggests that the matching results are not driven by pre-reform

differences among the groups. The matching estimates are qualitatively similar to the IV ones presented in this section.

# 4 Rent Sharing

To evaluate the distributional consequences of the wage effects observed in the previous section, I fit the model represented in equations 3 and 4 for new outcomes: average wage on different percentiles of the within firm distribution. The average wage effect is zero, but there is positive effects at top percentiles of the wage distribution. Figure 7 and 8 plots the effect on percentile 20 and 30, where there is virtually no wage increase. The wage increase is concentrated at the top wage percentiles. As shown in table 2, the wage average treatment effect is zero, but boosted up to 4% for the top 1% workers. Figure 9 and 10 show the evolution of the average treatment effect at the top percentiles.

These results shed light to an important consequence of the tax policy, the within firm wage inequality. As the Government reduces payroll tax rates to lower labor cost, it increases the wage gap between high and low hierarchical levels. The discrepancy is even larger when considering the share of the wage bill paid to high versus low earnings workers. At the top of the distribution, wages were higher in the first place, and they are the ones receiving a higher percentage increase due to the tax reform. In this paper I want to discuss alternative policies able to reconcile lower labor costs and less pay inequality (*to be done*).

I am still investigating the underlying mechanisms to rationalize the rent sharing results. There are two main avenues under consideration. One is that highly skilled workers face a tighter labor market, and competition to hire and retain talent drives their large wage increase. An alternative mechanism is that workers in high hierarchical levels have more power to set their own wages, so that when firms are positively shocked with a tax cut, these workers have the ability to extract more rents from the firm (Bebchuk and Cremers, 2011).

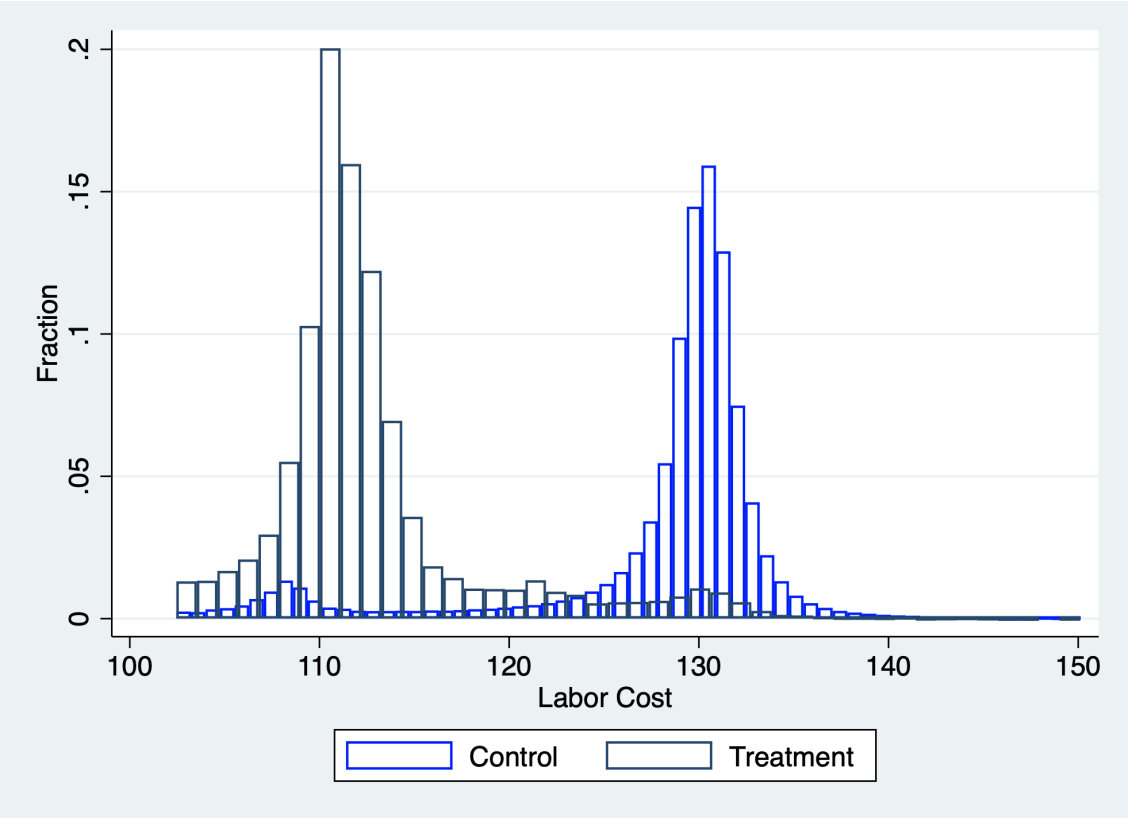
# 5 Intensive Margin and Elasticities

Up to this point I have explored the extensive margin, i.e., variation on whether firms have joined the tax program or not. It hasn't been discussed yet the variation on the intensive margin, which means the treatment intensity. The size of the payroll tax cut varies within treated firms based on pre-reform characteristics and whether the firm was treated via the product (partial treatment), or sector (full treatment)

eligibility criteria. Technically the tax cut due to the reform is the reduction on payroll taxes added by the revenue tax that substitutes in. For simplification, I am not accounting for the impact of the revenue taxes on the hiring decisions. The revenue taxes directly impacts the final output price, and depending on the production function indirectly impacts the labor demand. On a first passing I consider the labor cost variation as the relevant one to assess the overall employment and wage impacts of the reform.

Figure 2 plots the average labor cost for treated and control firms. The histogram trims the top and bottom 1% tax rates. The average labor cost observed for control firms is 131%, which is consistent with the statutory rates. Under treatment, the average labor cost is 112%, which is also consistent with the statutory rates in the program.

Figure 2: Histogram on Treatment Intensity




*Note:* This histogram compares the distribution of labor cost (defined as wages plus payroll tax) between treated and control firms. The average labor cost during treatment is 112%, whereas 131% out of treatment. The distribution for the two groups are centered on the average, but present dispersion. This histogram trims the top and bottom 1% on the net-of-tax rate distribution.

In order to compute the elasticity of employment with respect to the labor cost, I first compute the treatment percentage variation on labor cost ($d\ln(1+\tau)$), which is -14.9%. Based on the labor cost variation and the average treatment effects presented in table 2, I can compute the elasticity of employment with respect to labor cost, which is -0.67. Saez et al., 2019 find youth employment elasticity equal to -0.38.

Even if I break down my estimates between young and old workers it is not obvious how to compare with Saez et al., 2019 because in their case only the young workers were targeted by the policy. There is no reason to believe that the youth employment response would be similar to the one where all employees are eligible for the tax benefit. It is reasonable to imagine that the labor demand elasticity for youth workers is smaller, as they are a cheaper labor force, and their hiring decision is less dependent on tax cuts. The use of these elasticities should be interpreted with caution. In order to think about macro implications on the labor market, or welfare analysis it is crucial to account for jobs transferred from previously existing jobs in other firms. This is the case, when treated firms poach workers from their competitors. Another possibility is that employment increases due to formalization of existing informal jobs, this is especially important to be considered because the study takes place in Brazil where more than 40% of the labor force is sheltered in informal markets. Next section is dedicated to carefully study the transition to employment.

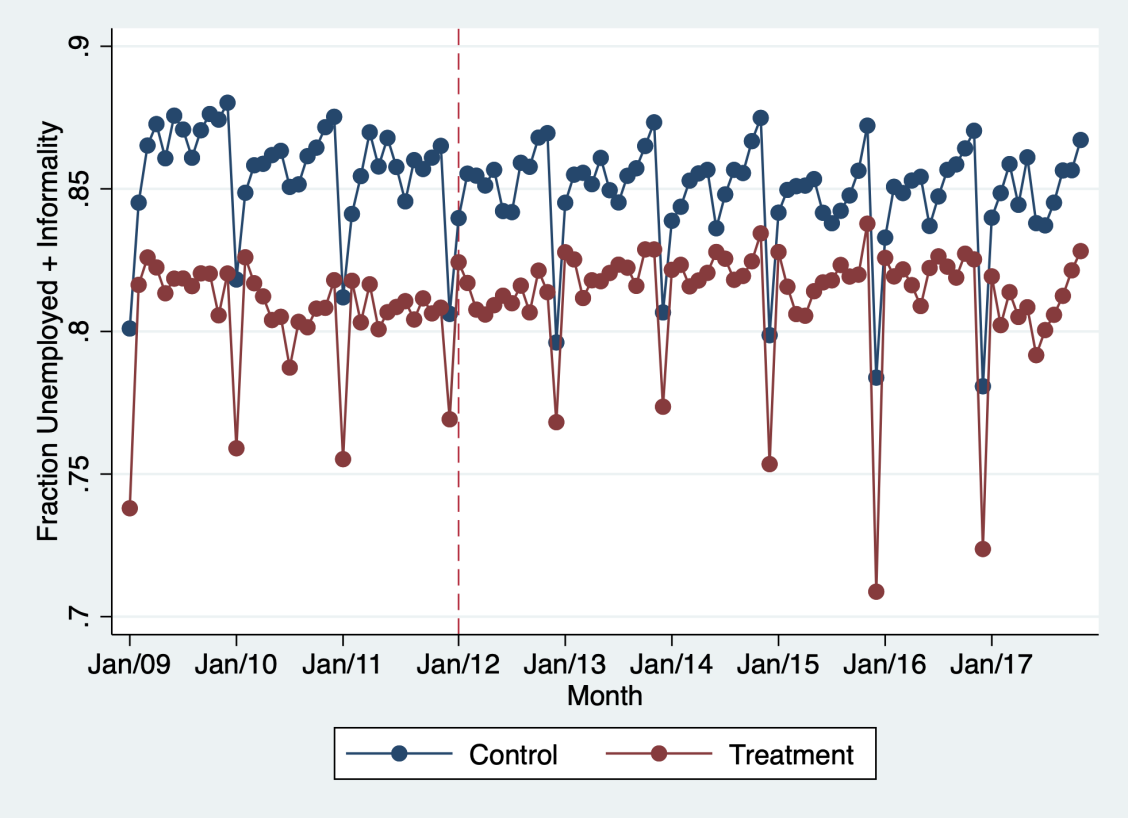
# 6 Transition to Employment

Where is the employment increase coming from? The first step towards the bottom of this question is whether the employment result is driven by more hires or less layoff in treated firms. I have suggestive evidence that roughly half of the effect comes from more hires and the other half from less layoff in treated firms relative to control, but I am still working on this analysis. The following step is to understand what fraction of the new hires comes from existing informal jobs? The standard way to study informality in Brazil is to use households surveys (Ulyssea, 2018). The Brazilian Statistical Bureau (IBGE) administers a public available survey, Pesquisa Nacional por Amostra de Domicílios (PNAD), on 300,000 households and questions regarding wages, employment and informality. The problem of PNAD is that the worker is not identified so we cannot link it with matched employee-employer (RAIS) data. The standard alternative solution is to match these two datasets on the 3-digit sector level, the thinnest sector aggregation in PNAD, which is still considerably

broad. This approach can provide some intuition with regard to the formal and informal labor market interactions, at the cost of losing granularity and estimation power.

This section provides a novel method to assess the interaction between formal and informal markets. It benefits from the panel structure and the worker level granularity in RAIS, in order to flag new hires employment status in the period before the hire occurs. If it was the case that the observed employment treatment effects were due to formalization, then we should see a sharp decrease in the share of hires from formal jobs between groups, once the treatment period begins. This is not what figure 3 suggests. Even though the difference between the two curves shrinks after the treatment period, this drop is not enough to rationalize all the employment treatment effects. More work is in progress, in order to disentangle the precise fraction of treatment effect that could be explained by formalization.

Figure 3: Poaching Index




*Note:* This figure plots the share of new hires that were formally employed on the month prior to the hire. This figure restricts to the sample of hired workers each month. It compares the evolution of shares in eventually treated versus control firms.

New hires that were previously employed in the formal sector could be transi-

tioning from treatment to control firms, or vice versa. To understand the pattern of the poaching behavior, the first step is to understand whether firms positively shocked by the reform are poaching employees from non-treated firms that are disadvantaged in terms of tax obligations, or are poaching workers from treated firms. Figures 11 and 12 show that most of the new hires (in both treatment and control groups) come from non-employment or informality. Additionally one can notice that treated firms tend to hire more from treated firms, and control tend to hire more from control firms. This is consistent with employee skills that are sector specific. There is no clear change on this behavior before and after the payroll tax reform takes place.

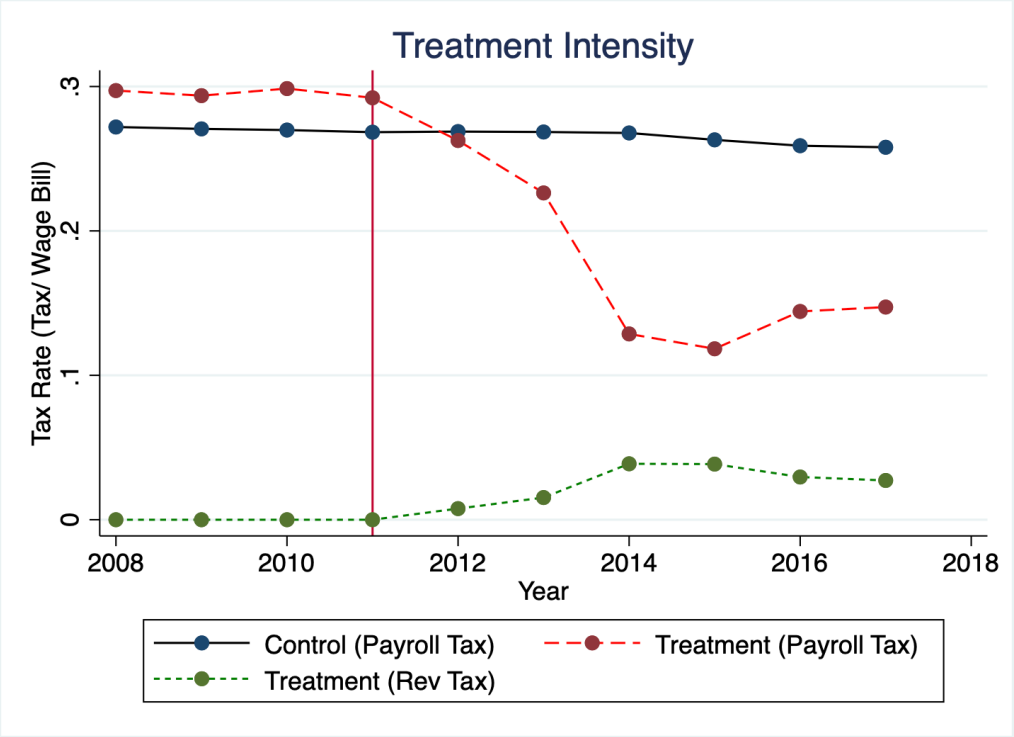
# 7 Conclusion

This paper precisely estimates the labor market effects of a payroll tax cut, and computes the employment elasticity with respect to labor cost. In terms of policy recommendations, caution is needed to interpret the large employment effect. First because it generates negative distributional impacts on the within firm pay distribution, and second because I am still investigating the origin of the employment gains. The estimates of this paper can also be used to calibrate structural models to design counterfactual policies. To some extent there is segregation on the labor markets, and I will explore this variation on the tightness of each labor market.

# 8 Appendix

## 8.1 Figures

Figure 4: Tax Implication of the Reform




*Note:* This figure presents evolution of tax rates for eventually treated vs control firms over the years. The blue line depicts payroll tax rates for control (never treated) firms, which slightly declined over the years years, following global trends (OECD, 2019). The dashed red line represents the payroll tax rates for treated firms. The dashed green line presents the revenue tax rates that substituted in once treatment takes place. Revenue tax rates are computed as a function of the total wage bill in order to facilitate comparisons.

Figure 5: Tax Forms Information

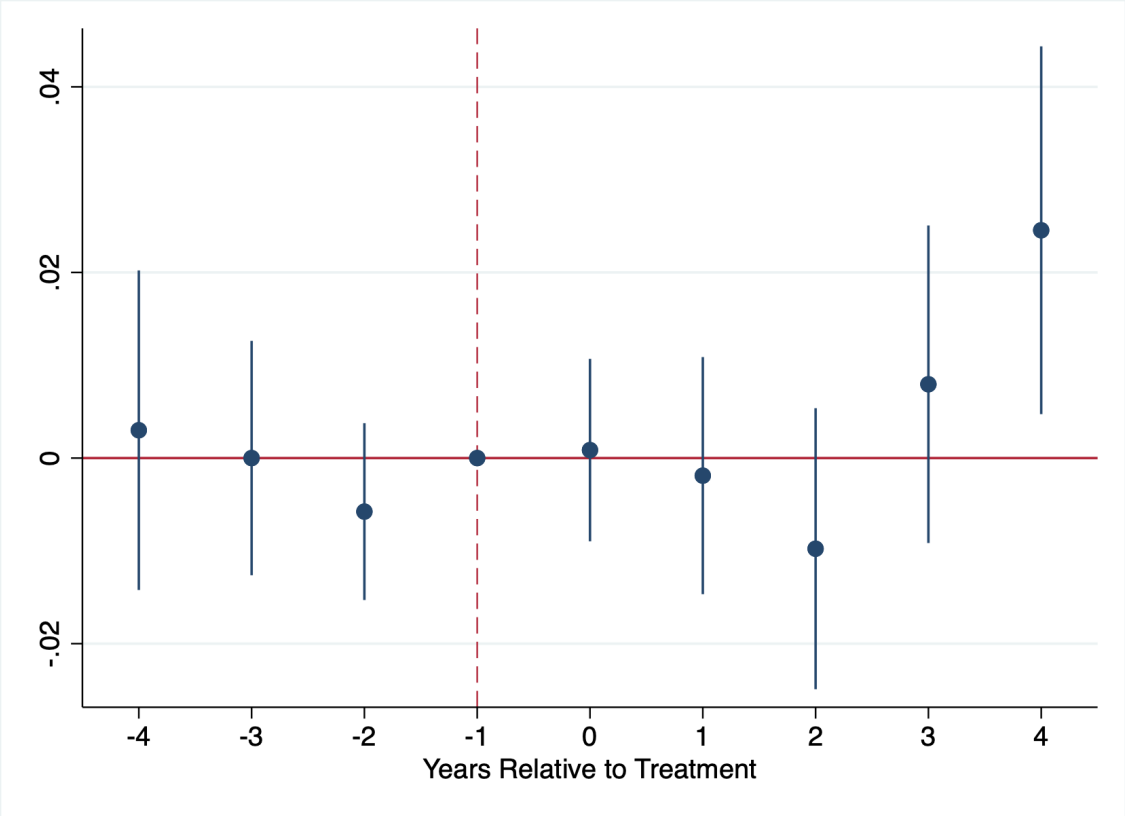
| Nº | Campo | Descrição | Tipo | Tam | Dec | Obrig |
|---|---|---|---|---|---|---|
| 01 | REG | Texto fixo contendo “0145”. | C | 004* | - | S |
| 02 | COD_IN C_TRIB | Código indicador da incidência tributária no período:<br>1 – Contribuição Previdenciária apurada no período, exclusivamente com base na Receita Bruta;<br>2 – Contribuição Previdenciária apurada no período, com base na Receita Bruta e com base nas Remunerações pagas, na forma dos nos incisos I e III do art. 22 da Lei nº 8.212, de 1991. | N | 001* | - | S |
| 03 | VL_REC _TOT | Valor da Receita Bruta Total da Pessoa Jurídica no Período | N | - | 02 | S |
| 04 | VL_REC _ATIV | Valor da Receita Bruta da(s) Atividade(s) Sujeita(s) à Contribuição Previdenciária sobre a Receita Bruta | N | - | 02 | S |
| 05 | VL_REC _DEMAI S_ATIV | Valor da Receita Bruta da(s) Atividade(s) não Sujeita(s) à Contribuição Previdenciária sobre a Receita Bruta | N | - | 02 | N |
| 06 | INFO_C OMPL | Informação complementar | C | - | - | N |

| Nº | Campo | Descrição | Tipo | Tam | Dec | Obrig |
|---|---|---|---|---|---|---|
| 01 | REG | Texto fixo contendo "P100" | C | 004* | - | S |
| 02 | DT_INI | Data inicial a que a apuração se refere | C | 008* | - | S |
| 03 | DT_FIN | Data final a que a apuração se refere | C | 008* | - | S |
| 04 | VL_REC_TO T_EST | Valor da Receita Bruta Total do Estabelecimento no Período | N | - | 02 | S |
| 05 | COD_ATIV_E CON | Código indicador correspondente à atividade sujeita a incidência da Contribuição Previdenciária sobre a Receita Bruta, conforme Tabela 5.1.1. | C | 008* | - | S |
| 06 | VL_REC_ATI V_ESTAB | Valor da Receita Bruta do Estabelecimento, correspondente às atividades/produtos referidos no Campo 05 (COD_ATIV_ECON) | N | - | 02 | S |

| Nº | Campo | Descrição | Tipo | Tam | Dec | Obrig |
|---|---|---|---|---|---|---|
| 01 | REG | Texto fixo contendo "P100" | C | 004* | - | S |
| 07 | VL_EXC | Valor das Exclusões da Receita Bruta informada no Campo 06 | N | - | 02 | N |
| 08 | VL_BC_CON T | Valor da Base de Cálculo da Contribuição Previdenciária sobre a Receita Bruta<br>(Campo 08 = Campo 06 – Campo 07) | N | - | 02 | S |
| 09 | ALIQ_CONT | Alíquota da Contribuição Previdenciária sobre a Receita B<br>ruta | N | 008 | 04 | S |
| 10 | VL_CONT_A PU | Valor da Contribuição Previdenciária Apurada sobre a Receita Bruta | N | - | 02 | S |
| 11 | COD_CTA | Código da conta analítica contábil referente à Contribuição Previdenciária sobre a Receita Bruta | C | 255 | - | N |
| 12 | INFO_COMP L | Informação complementar do registro | C | - | - | N |

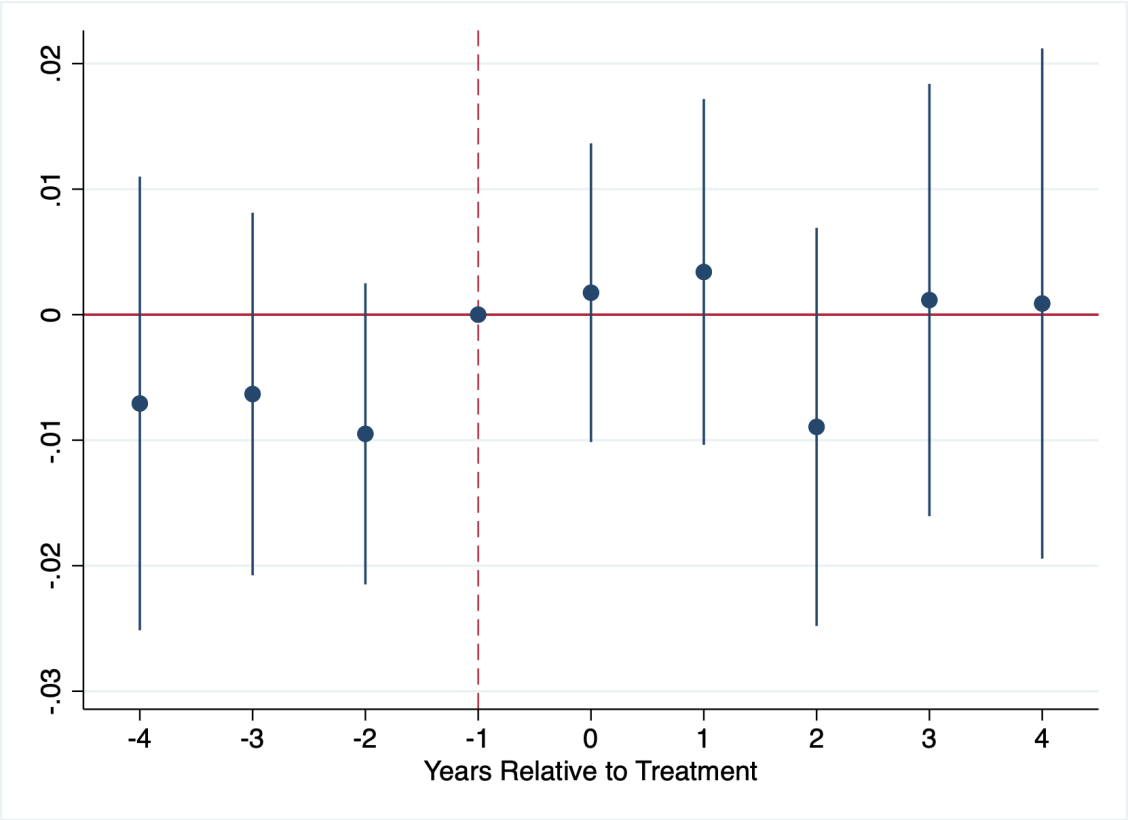
*Note:* This figure shows instructions for eligible firms to request the payroll tax benefit. It describes detailed information to be provided in Tax Administration software, in order to substitute part of the payroll tax by revenue taxes.

Figure 6: Wages: Event Study Estimates



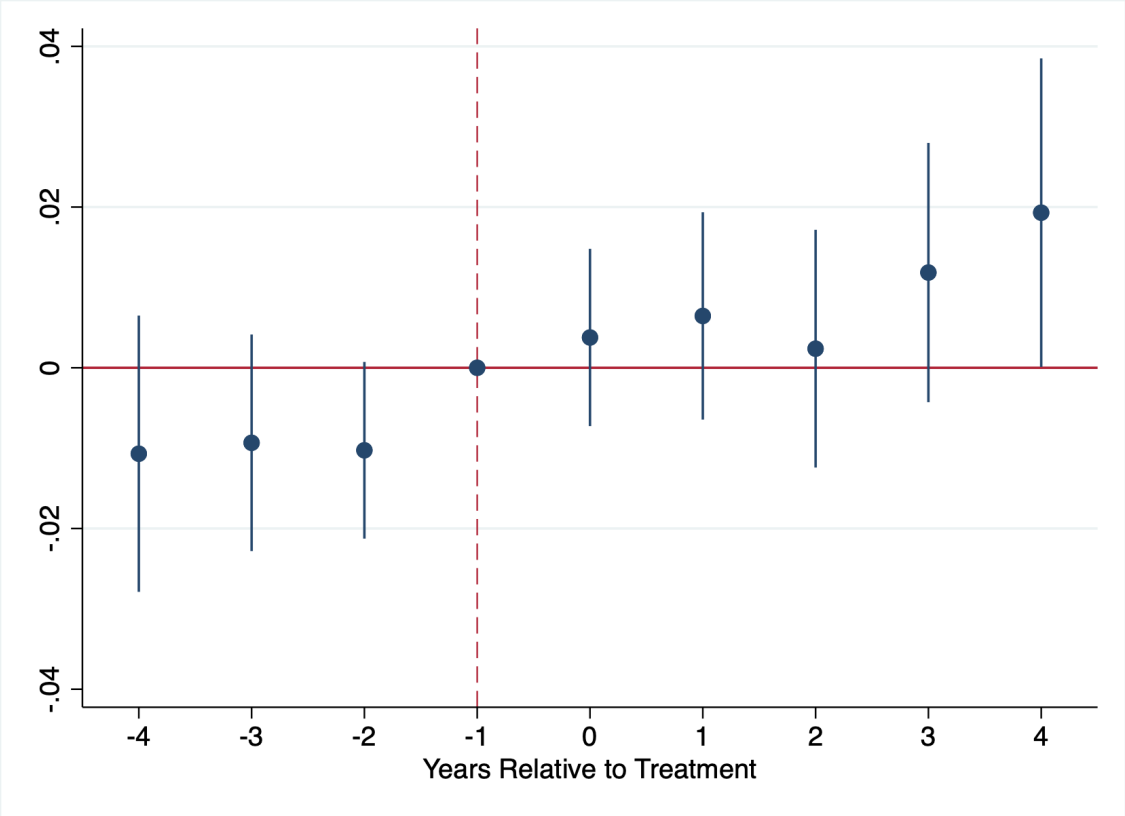

*Note:* This figure presents the event study estimates for mean wages, which is defined as the firm's mean wages on Dec/31$^{st}$ of each year. The results should be interpreted on a worker level because the regressions are weighted based on time invariant measures of firm size in the pre-period. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans four years prior to entering the payroll tax cut program and four years after.

Figure 7: Wages (Percentile 20): Event Study Estimates



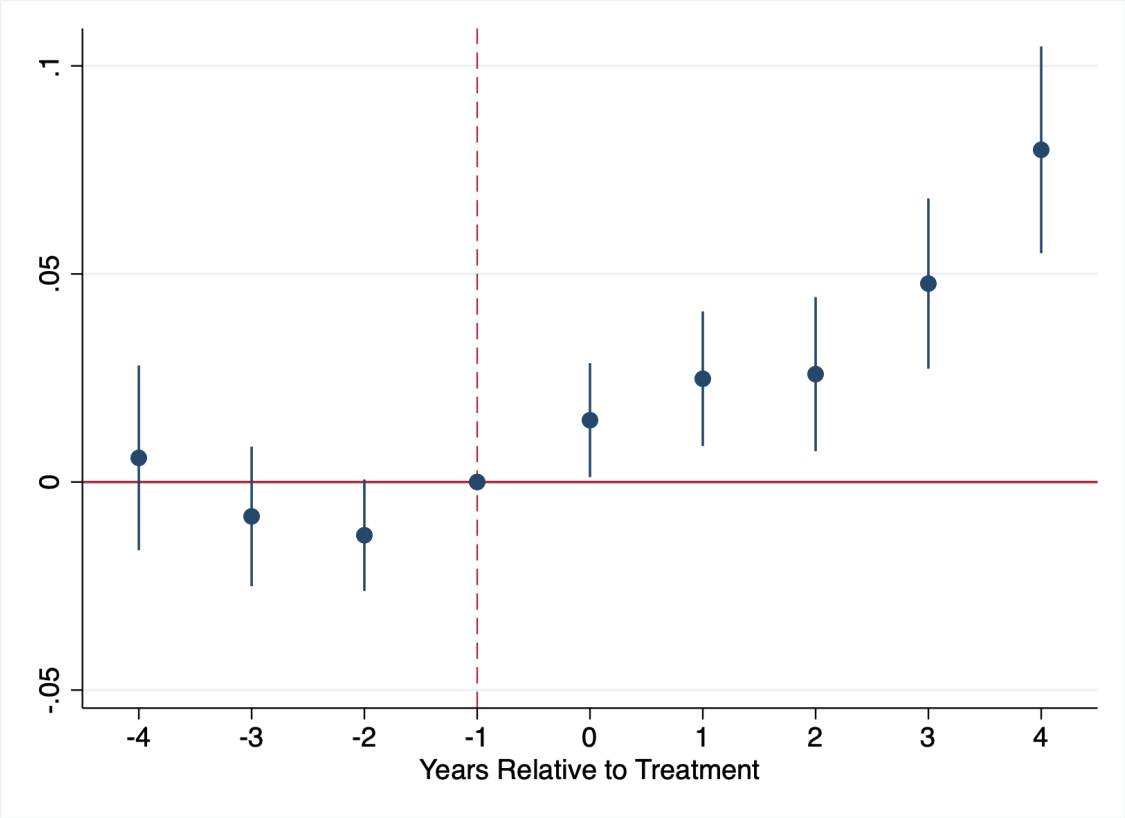

*Note:* This figure presents the event study estimates for wages at the percentile 20 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans four years prior to entering the payroll tax cut program and four years after.

Figure 8: Wages (Percentile 30): Event Study Estimates



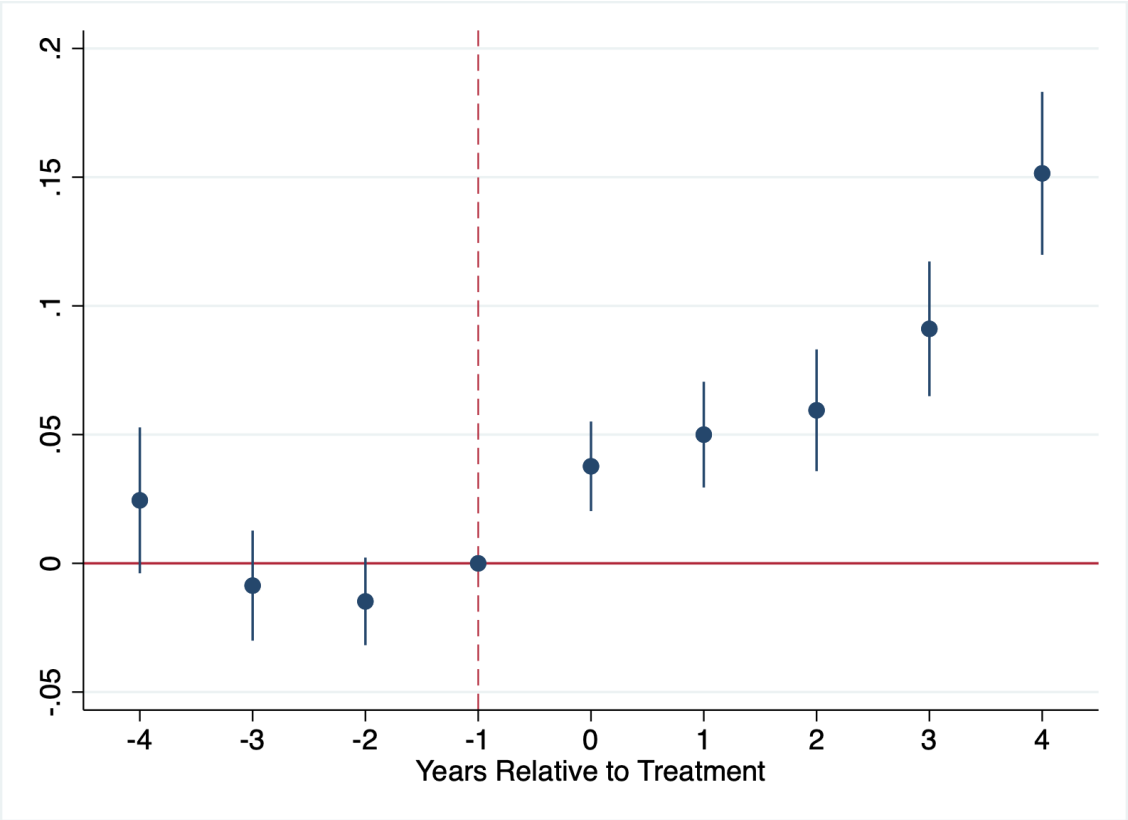

*Note:* This figure presents the event study estimates for wages at the percentile 30 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans four years prior to entering the payroll tax cut program and four years after.

Figure 9: Wages (Percentile 90): Event Study Estimates



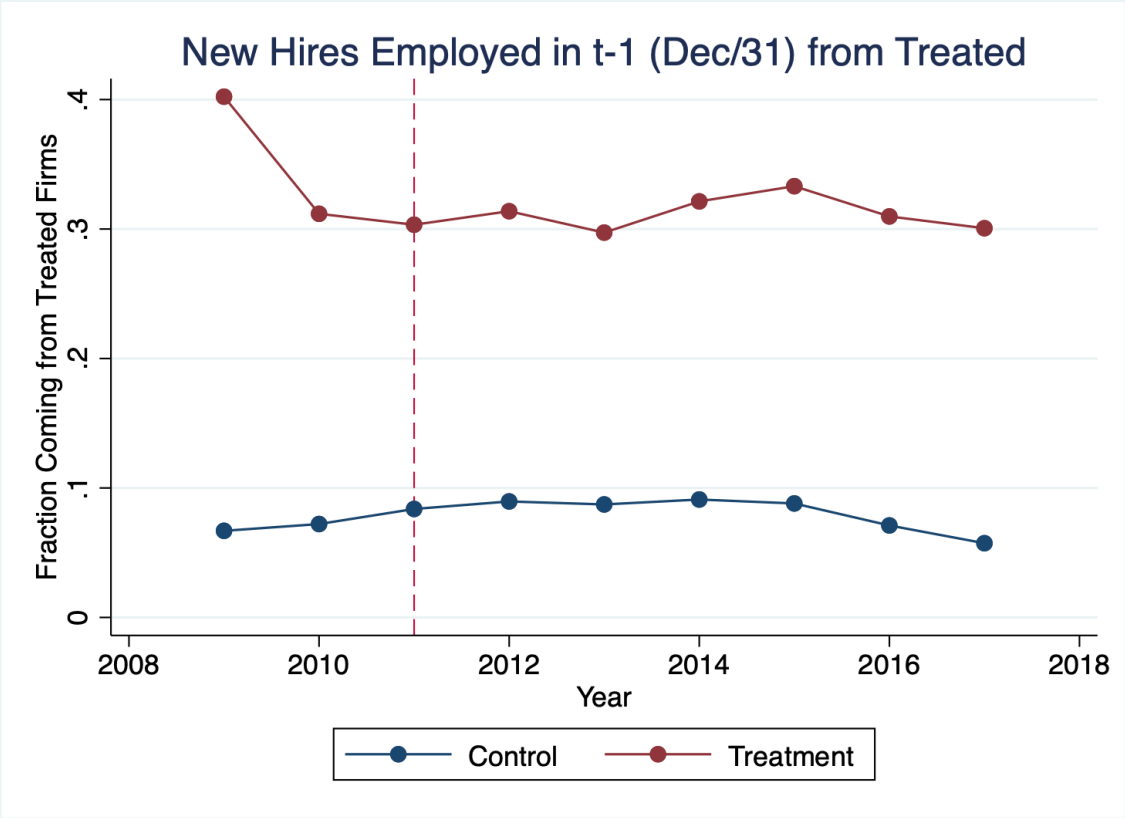

*Note:* This figure presents the event study estimates for wages at the percentile 90 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans four years prior to entering the payroll tax cut program and four years after.

Figure 10: Wages (Percentile 99): Event Study Estimates



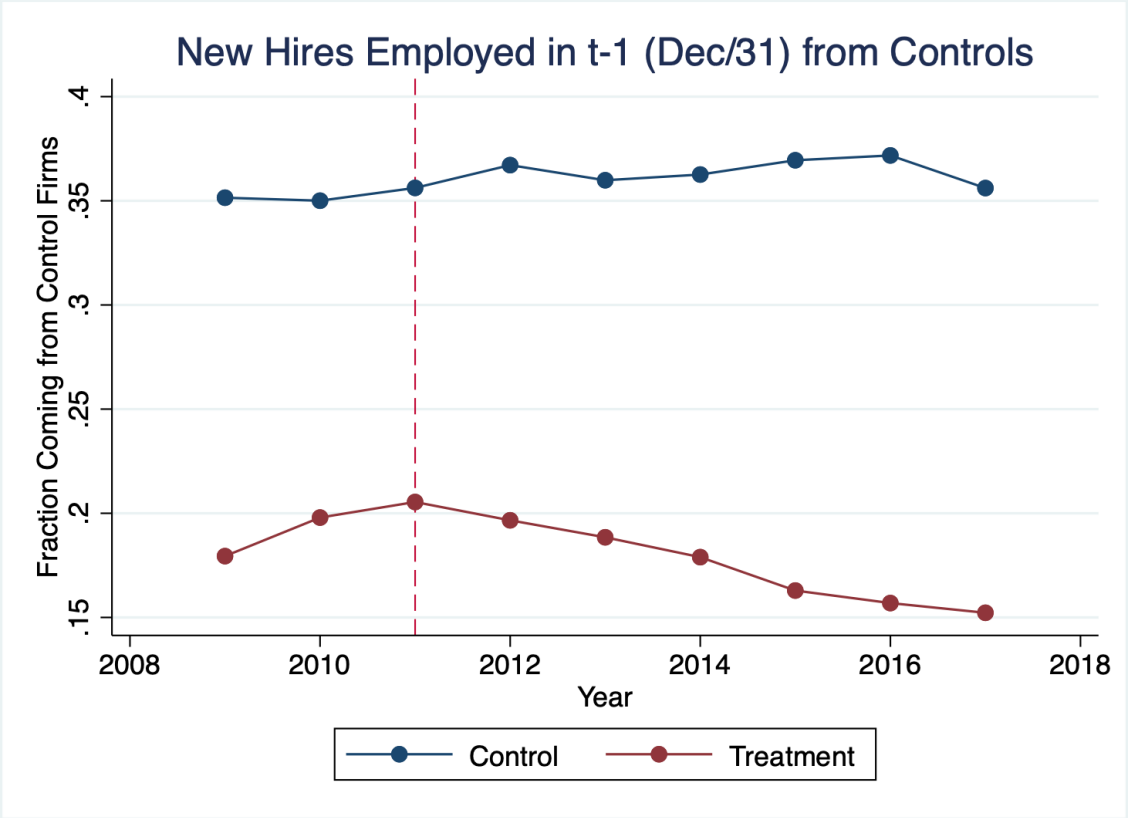

*Note:* This figure presents the event study estimates for wages at the percentile 99 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans four years prior to entering the payroll tax cut program and four years after.

Figure 11: New Hires Employed from Treated Firms




*Note:* This figure plots the share of new hires that were formally employed in treated firms on Dec/31 of the year prior to the hire occurs. This figure restricts to the sample of hired workers in each year. The blue curve depicts control firms, while the red is treatment. The sum of the two curves represent the share of new hires formally employed in the previous period. To add up to 100% would be necessary to include the fraction of new hires hired from informality or non-employment.

Figure 12: New Hires Employed from Control Firms




*Note:* This figure plots the share of new hires that were formally employed in control firms on Dec/31 of the year prior to the hire occurs. This figure restricts to the sample of hired workers in each year. The blue curve depicts control firms, while the red is treatment. The sum of the two curves represent the share of new hires formally employed in the previous period. To add up to 100% would be necessary to include the fraction of new hires hired from informality or non-employment.

## 8.2 Tables

Table 1: Descriptive Statistics

| | Average |
|---|---|
| *Baseline Sample Characteristics* | |
| Employment | 19.87 |
| | (49.82) |
| Total Payroll (Dec/31) | 31,795.40 |
| | (93,692.44) |
| New Hires | 8.66 |
| | (46.76) |
| Gender Composition | 0.59 |
| | (0.38) |
| Age | 40.61 |
| | (8.38) |
| Firm Age | 25.65 |
| | (9.65) |
| High School | 0.48 |
| | (0.39) |
| White | 0.59 |
| | (0.39) |

*Note:* This table presents descriptive statistics of the baseline sample in the period from 2008 to 2017. The variable "High School" reports the share of workers that achieved high school education or higher. The variable "Gender Composition" reports the share of male workers.

Table 2: DD Estimates

| | Employment | Wages | Wages (99p) | Wages (90p) | Wages (40p) | Wages (20p) |
|---|---|---|---|---|---|---|
| Currently Treated | 0.0993*** | 0.00676 | 0.0400*** | 0.0164* | 0.00397 | 0.00679 |
| | (0.0148) | (0.00933) | (0.00808) | (0.00649) | (0.00566) | (0.00563) |
| Observations | 2,563,276 | 2,563,166 | 2,563,276 | 2,563,276 | 2,563,276 | 2,507,349 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Sector (1 digit) x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

*Note:* This table presents IV estimates, which informs causal impacts of the reform on outcomes labeled on each column. The instrument is the interaction between two indicators: one that flags sector eligibility and the other indicates if the time is post inclusion. The instrument is based on exogenous variables that predicts treatment assignment.

# References

Bachas, Pierre Jean, and Mauricio Soto. 2018. *Not(ch) your average tax system : corporate taxation under weak enforcement* [in en]. Technical report WPS8524. The World Bank, July.