The Incidence of Payroll Taxation

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[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 9.1% increase on employment and zero effect on wages. However, the reform deteriorates within firm wage inequality. Suggestive evidence indicates that 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 -0.63.

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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 much 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 the gains 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, Schoefer, and Seim 2019, Saez, Matsaganis, and Tsakloglou 2012, Gruber 1997, Hamermesh 1979, Kugler, Kugler, and Prada 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 recessions, which present other macro shocks able to confound the causal effect of the tax cut on the economy.

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, which alleviates the pay equity concern. Secondly, 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 costs aiming to increase employment and domestic competitiveness. A central characteristic of the reform is the imperfect take-up rate. Technically, treatment became optional, 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 (2012). The reform was valid for many years and created relevant tax benefits to certain firms, as can be depicted in figure 5.

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 (working paper). 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 industry.

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 a 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 is 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 firm 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 the 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 discusses the transition to employment. Section 6 computes relevant elasticities, 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, and 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 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, the 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 in 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 2 where I test the parallel trends assumption. Other secondary patterns suggest that the legislator's choice of eligible sectors was as good as random. The balance observed in table 1 which presents descriptive statistics to compare eligible and non-eligible sectors is reassuring. Finally, a few examples from the sector's choice provide intuition about the "randomness" in the eligibility process. 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, Schoefer, and Seim 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 6 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

[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 is input.

program. Non-responsiveness is consistent with empirical findings in other countries (Kleven and Waseem 2013).

2.2 Data and Descriptive Statistics

Tax Data. The firm level tax data comes from three sources. (1) Tax form that informs the 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). (2) Tax form on total payroll taxes liability, 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. (3) Tax form on total revenue tax liability, namely *Contribuição Previdenciária Sobre a Receita Bruta* (CPRB). The base for this tax is the net of exports gross revenue, and only treated (payroll tax waived) firms are eligible to it. Therefore, the revenue tax data is used not only to compute the firm's total tax liability, but also to indicate whether they are treated or not. Each observation consolidates all the information from establishments that belong to the same group in a given year. The sample spans the period from 2008 to 2017 on an annual basis.

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 provides firm and worker level information covering every formal labor contract since 1976. I restrict the analysis to the period between 2008 and 2017, which allows me to track firms before and after the implementation of the payroll tax program. At the firm level, RAIS contains information on the tax regime[12], sector (at its most granular definition), age and location. At the worker level, it contains variables regarding employment status, earnings, gender, race, age, education and more. Workers and firms are uniquely identified based on tax codes (CPF and CNPJ) that do not change over time. The main shortcoming in RAIS is the lack of information about informal and non-employed workers.

Firm Level Sample. I merge the tax and labor market data to obtain a comprehensive laboratory to study the incidence of payroll taxes on the universe of 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

[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).

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 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 of interest, 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 out of 21 one-digit sectors of the Brazilian economy remain in the analysis. In the appendix, I show that results are robust to standard cleaning procedures such as winsorization and balanced panels. In the appendix, I repeat the analysis based on a winsorized data, in which wages and employment are winsorized at the 1 and 99% levels. In the second robustness check I keep only the balanced panel of firms (the ones that appear in all ten years of the sample) to avoid the effects from firms entry and exit.

Worker Level Sample. To maintain consistency between the firm and worker level analysis, I keep the same sample restrictions presented before to ensure an equivalent set of employers in both data sets. I follow (Jacobson, LaLonde, and Sullivan 1993), (Lachowska, Mas, and Woodbury 2020) and (Szerman 2019) to create a tenure restriction to track only workers that have been employed by the same employee for at least three years in the pre-reform period (2008-2011). This guarantees that results are driven by relatively stable employer-employee matches. In the appendix, I show that removing the tenure constraint doesn't imply major

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

changes to the main results. As in (Dix-Carneiro 2014), I construct the panel of workers by drawing a 1% sample from the list of all employees that appear in RAIS from 2008 and 2017.

Descriptive Statistics. In the firm level sample there are 2,396,669 observations in the pre period (2008-2011). These firms are allocated in 19 one digit sectors that are broken down into 1,072 seven digit CNAE industries. Table 1 provides summary statistics for eligible and non-eligible firms in the pre-period (2008-2011). Prior to the tax reform, firm's average employment on December 31^{st} of each year was 43.98 workers receiving an average monthly earning of $1,010 BRL (approximately $200 USD[15]). Each firm hired an average of 21 workers per year, and the labor force is 68% white. In terms of educational background, firms present an average share of 55% high school graduates. Detailed descriptive statistics for the worker level sample can be found in table 2.

3 Extensive Margin Responses

In this section I analyze employment and earnings consequences associated with enrollment in the payroll tax program. 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 imperfect compliance. The non compliers are non-treated firms in eligible sectors and treated firms in non-eligible sectors due to the product (NCM) eligibility criteria.

3.1 Firm Level Analysis

To estimate the employment effect I rely on a fuzzy difference in differences strategy, and fit the first stage described in equation 1.

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

where, D_{jt} indicates that firm j was treated in year t; $L_{s(j)t}$ indicates that firm j belongs to a sector that is eligible for treatment and that period t is after the starting eligibility date for sector s(j); X_{jt} are set of controls (e.g., education, gender, race, age and its square); $\xi_{s1,t}$ is 1-digit sector interacted with year fixed effect.

[15] As of the exchange rate in October, 1^{st}, 2021.

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

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

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

It is not desirable to estimate the earnings effect using firm level data because the employer average earnings can be affected by composition changes of its labor force. To address this concern, I take advantage of the granularity of the micro data, and estimate a similar model on the worker level.

3.2 Worker Level Analysis

It is not easy for firms to manipulate sectors, and in fact I don't observe in the data firms responding with sector changes in order to become eligible [17]. However, it is much easier for workers to move to an eligible sector, as they are mobile across firms. To address this concern, I define workers eligibility in the pre-period (2008-2011). In other words, I assign workers' eligibility status ($\{0,1\}$) according to the firms that they are allocated to before the reform. If an individual has worked in at least one eventually eligible firm in the pre-period, then she is considered as treated. Thus, $L_{s(j_0)t}$ is equal to one for all years after firm j's pre-reform sector ($s(j_0)$) becomes eligible.

Similarly to the firm level specification, I estimate equations 3 and 4, where i indexes workers, Y_{ijt} is worker's wage in year t and firm j; θ_i is the worker fixed effect; j_0 is the worker's firm in the pre-period; and the remaining variables and fixed effects are analogous to definitions in equations 1 and 2.[18]

$$D_{ijt} = \theta_i + \alpha_j + \gamma_t + \xi_{s1(j),t} + \pi L_{s(j_0)t} + X_{it} + u_{ijt} \tag{3}$$

$$Y_{ijt} = \theta_i + \alpha_j + \gamma_t + \xi_{s1(j),t} + \delta L_{s(j_0)t} + X_{it} + u_{ijt} \tag{4}$$

Table 3 presents the estimates. Results can be interpreted as a causal effect of

[17] For caution and transparency I run a robustness check that excludes from the sample firms that have ever changed sectors. The main result holds and doesn't present sensitivity to this robustness check.

[18] In the main sample there a few incumbent workers that have worked in multiple firms in the same year. I ran a robustness check in which I kept the highest paid job per worker, and kept the longest tenured position to break ties in cases the same incumbent worker had multiple positions with the same pay.

the policy, i.e., participation in the payroll tax program causes a 9.1% employment increase (SE = 0.013) in treated firms relative to control, and statistically zero earnings effect to incumbent treated workers relative to control. Intuitively, this result reveals that firms expand labor demand, but do not pass through the gains to wages. The identification relies on the timing of a sector becoming eligible being uncorrelated with the outcomes of interest, conditional on the fixed effects. It requires that absent the reform, sectors would have followed parallel trends. 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 to formally test for parallel trends.

3.3 Event Study

The event study has two main goals. First, to validate the identifying assumption by showing that the pre-reform coefficients of interest are not statistically different from zero. Second, it 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.

3.4 Firm Level Analysis

In the firm level data, the first stage and reduced form are given respectively by, 5 and 6.

$$D_{jt}^{k} = \sum_{k=-3}^{-2} \delta_k L_{s(j)t}^{k} + \sum_{k=0}^{3} \delta_k L_{s(j)t}^{k} + \alpha_j + \gamma_t + \xi_{s1(j),t} + X_{jt} + u_{jt} \tag{5}$$

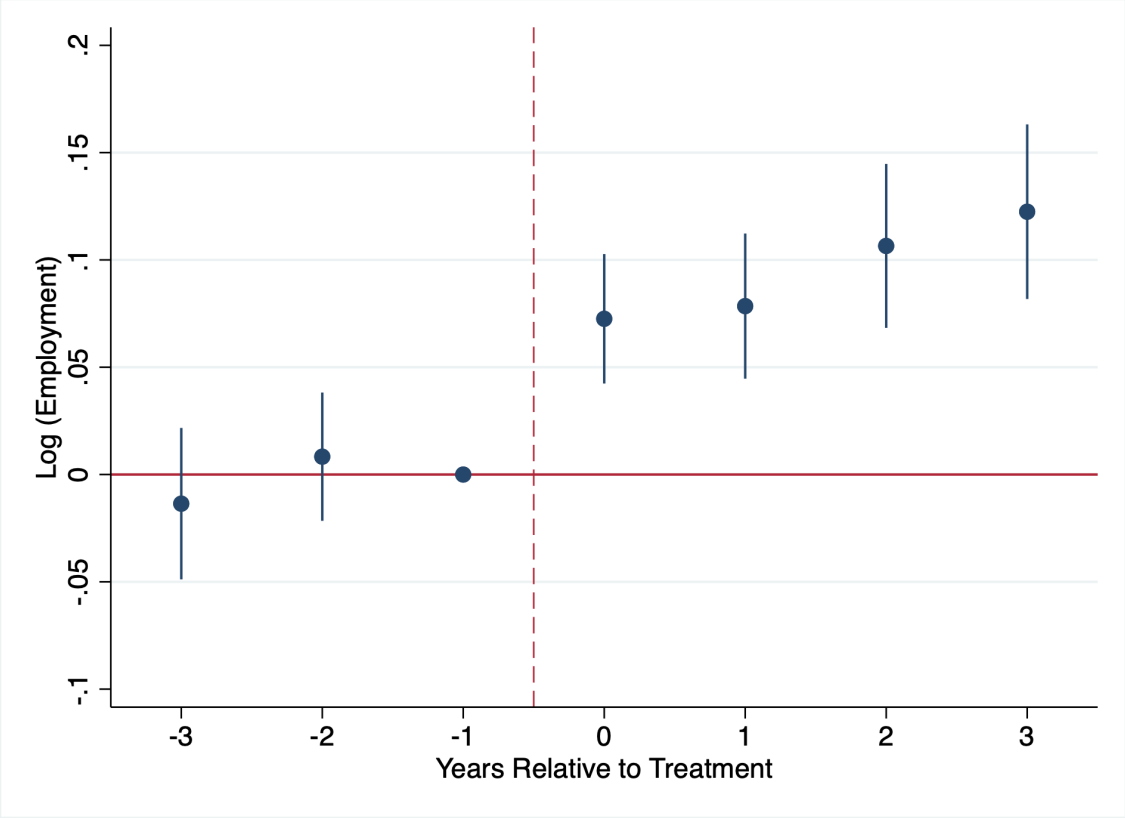
where $D_{jt}^{k} = 1$, if firm j is k years away from entering the treatment at time t, i.e., $t = e_j + k$ and e_j is the date when the firm enters treatment for the first time. Analogously, $L_{s(j)t}^{k} = 1$, if $t = e_{s(j)} + k$ and $e_s(j)$ is defined as the date when firm j's sector becomes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

$$Y_{jt} = \sum_{k=-3}^{-2} \delta_k L_{s(j)t}^{k} + \sum_{k=0}^{3} \delta_k L_{s(j)t}^{k} + \alpha_j + \gamma_t + \xi_{s1(j),t} + X_{jt} + u_{jt} \tag{6}$$

where Y_{jt} represents the log of firm j's labor market outcome in year t; α_j is the firm fixed effect, and $\xi_{s1(j),t}$ 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 -3 and +

3 years relative to entering treatment.

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 three years prior to entering the payroll tax cut program and three years after.

3.5 Worker Level Analysis

Similarly, in the worker level data, I fit equations 7 and 8.

$$D^k_{ijt} = \sum_{k=-3}^{-2} \delta_k L^k_{s(j_0)t} + \sum_{k=0}^{3} \delta_k L^k_{s(j_0)t} + \theta_i + \alpha_j + \gamma_t + \xi_{s1(j),t} + X_{it} + u_{ijt} \tag{7}$$

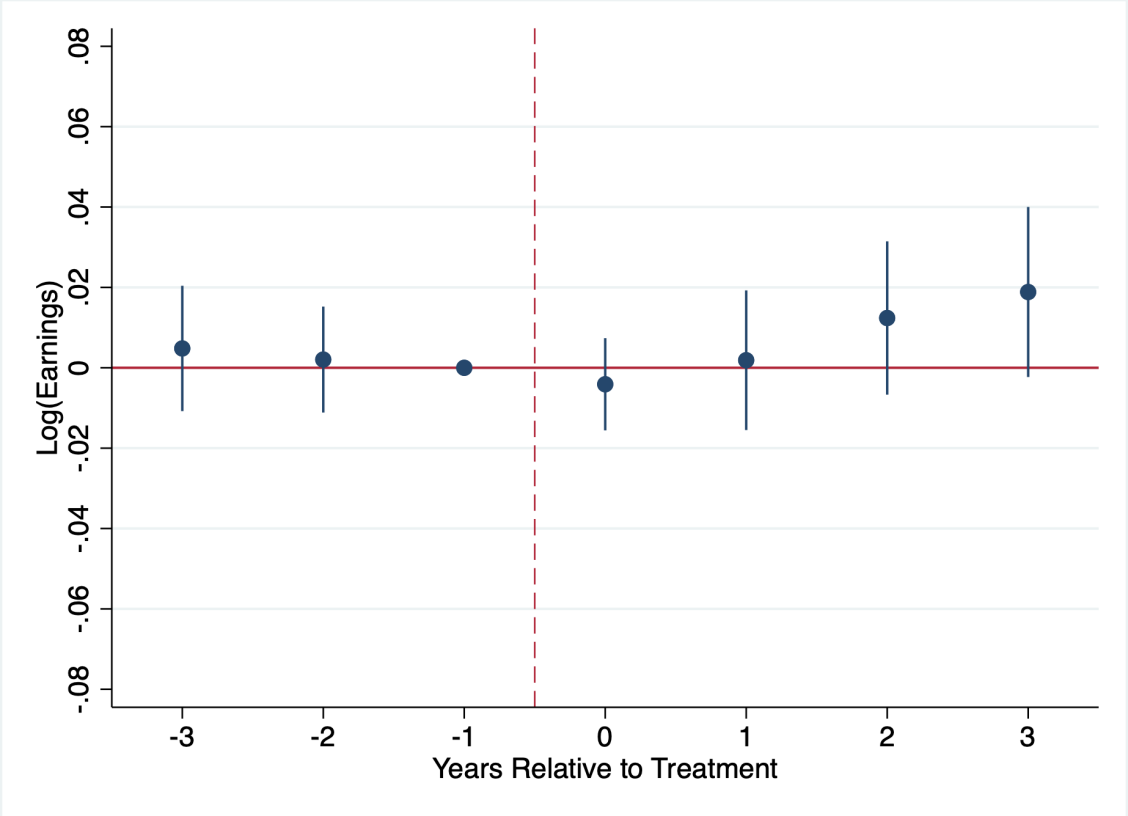
$$Y_{ijt} = \sum_{k=-3}^{-2} \delta_k L^k_{s(j_0)t} + \sum_{k=0}^{3} \delta_k L^k_{s(j_0)t} + \theta_i + \alpha_j + \gamma_t + \xi_{s1(j),t} + X_{it} + u_{ijt} \tag{8}$$

where i indexes workers, j_0 is the worker's firm in the pre-period, $D^k_{ijt} = 1$, if

worker i in year t is in firm j, which is k years away from entering the treatment at time t, i.e., $t = e_{j_0} + k$ and e_{j_0} is the date when the firm enters treatment for the first time. Analogously, $L^k_{s(j_0)t} = 1$, if $t = e_{s(j_0)} + k$ and $e_s(j_0)$ is defined as the date when firm j's pre-period sector becomes eligible.

Results are consistent with equations 1, 2, 3 and 4. Figures 1 and 2 show the evolution of the employment and wages 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.

Figure 2: Wages: Event Study Estimates



Note: This figure presents the event study estimates for average earnings effect for workers that were employed for at least three years in the same firm during the pre-reform period. I normalize the results with respect to one year prior to the treatment event. The analysis spans three prior to entering the payroll tax cut program and three years after.

In the appendix, I repeat the extensive margin analysis with a different identification strategy. Instead of using an instrument, as in the fuzzy difference in differences presented above, I rely on matching difference in differences. 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 is fitted in the pre-reform years and it 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 not driven by any inconsistency in the matching algorithm. A second test is to compare treatment and control pre-reform characteristics. This test shows that the groups are similar before the reform. The post differential outcomes between treated and control are due to the tax cut. The matching estimates are qualitatively similar to the fuzzy difference in differences.

4 Implications to the Within Firm Inequality

To evaluate the distributional consequences of the wage effects observed in the previous section, I fit the event study models in equations 5 and 6 for a new set of outcome variables: average earnings per percentiles of the within firm distribution. This analysis relies on the firm level sample, thus employment composition affects the earnings effect. However, it is helpful to provide intuition about the earnings treatment effect across heterogeneous hierarchical categories within firms. Table 4 displays the aggregate estimates from equations 1 and 2. Column (1) shows that average earnings on treated firms increase by 1.6% (SE = 0.0043) relative to control. This result encompasses both the pass through and the composition effects of the reform. The following columns break down the earnings impact per quantile of the within firm earnings distribution. As we move from the top to the bottom quantiles the earnings effect monotonically shrinks to zero.

Column (2) reports the impact to the payroll tax waived firm's 99^{th} earnings percentile, which presents a large and statistically significant increase of 4.32% (SE = 0.0068), compared to the control. Typically, this represents the income of the top 1% workers in the organizations' hierarchy. At the 90^{th} percentile (column 3), the payroll tax cut still created a large significant response of 2.65% in the treated firms compared to the control. The effect shrinks as we move to the bottom and it is not statistically distinguishable from zero in percentile 20, as displayed in column (5). The distributional analysis is also implemented in an event study fashion to test for each outcome of interest that the parallel trend assumption holds, i.e., absent the payroll tax reform both groups would have followed the same trends. This can be verified by estimating the equation 5 and 6. The results are presented in figures 7, 9 and 10, and as one can notice the pre-event coefficients are not statistically

different than zero.

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. This paper aims to discuss alternative policies able to reconcile lower labor costs and less pay inequality.

It is work in progress to disentangle the underlying mechanisms to rationalize the implications to the within firm inequality. 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 as a response to the firms' labor demand shock. 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, Cremers, and Peyer 2011).

5 Transition to Employment

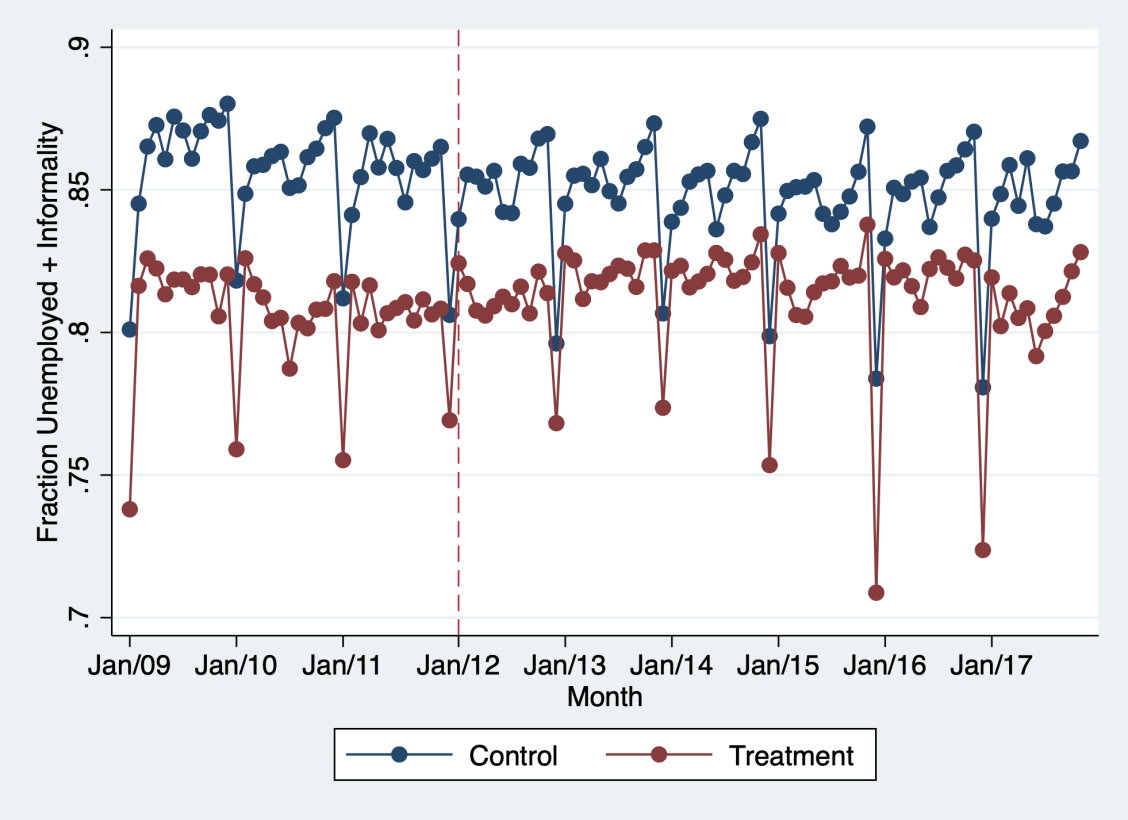
To have a better understand of the policy impacts in the labor market, it is key to evaluate whether the large and significant employment effects are due to the formalization (just an accounting effect), or if there is real employment increase. The standard way to study informality in Brazil is based on households surveys (Ulyssea 2018). The Brazilian Statistical Bureau (IBGE) administers a public available survey[19], on 300,000 households and cointains questions regarding earnings, 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 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 expense of losing statistical power.

In the context of the payroll tax reform under analysis, and given the data structure that allows me to track and follow firms and workers over time, there are other methods that can be used to estimate the informality channel without losing statistical power. First, I can observe the share of new hires that were previously

[19]Pesquisa Nacional por Amostra de Domicílios (PNAD)

employed by another formal firm (Sh_f). The complement of this share (1 - Sh_f) is the proportion of new hires coming from non-employment and informality. To test the hypothesis that the treatment employment boost after the reform is driven by a formalization response, one can compare Sh_f between treated and control groups before and after the reform. Figure 3 suggests that there is not a sharp difference in Sh_f between treatment and control, before and after the reform. There is a shrinkage in the distance between the two curves, however it might not be enough to rationalize the treatment effects. Formally, a standard difference-in-differences is able to test this hypothesis (*work in progress*). More data is needed to further disentangle the informality versus non-employed workers.

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.

6 Intensive Margin and Elasticities

This section focuses on the intensive margin variation, i.e., variation on labor cost due to the tax reform. To precisely compute the firm level labor cost, it is required detailed data on all types of payroll and revenue taxes that Brazilian firms are subjected to. While for the first part of the paper I had access to revenue tax (CPRB)[20] data for the universe of all Brazilian firms, for this section I only have detailed tax data for the subset of balanced firms. There are 252,103 unique balanced firms, i.e., the ones that appear every year (2008 - 2017) in the matched employer employee data (RAIS). Out of those, 225,493 appear in the tax records, and can be tracked over time. In the appendix I show that the parallel trend assumption holds for this sample, in order to eliminate selection concerns.

In the context of the Brazilian payroll tax reform, there are two possible labor cost definition to be considered. Labor cost can be computed as the wage bill × (1 + payroll tax rate). Alternatively, it can be defined the "comprehensive labor cost" which is the wage bill × (1 + payroll tax rate + revenue tax rate). The intuition for the latter approach is that the payroll tax reform creates a revenue tax that substitutes in. For the sake of evaluating the impact of the tax reform on labor market outcomes, it might be of interest to break down the analysis by payroll and comprehensive taxes. Figure 4 plots firm level labor cost. Since control firms have not been benefited by the payroll tax cut, these firms present higher labor costs as shown in figure 4. Average labor cost for control firms is 131%, whereas for treated firms is 112%, which is consistent with the statutory rates. The richness of the data allows me to compute relevant elasticities of interest for economists and policy makers. The first step is to compute the effect of the reform on firms' labor cost, by estimating equations 9 and 10.

$$D_{jt} = \alpha_j + \gamma_t + \xi_{s1(j),t} + \pi L_{s(j)t} + X_{jt} + u_{jt} \tag{9}$$

$$\log(1 + \tau_{jt}) = \alpha_j + \gamma_t + \xi_{s1,t} + \pi L_{s(j)t} + X_{jt} + u_{jt} \tag{10}$$

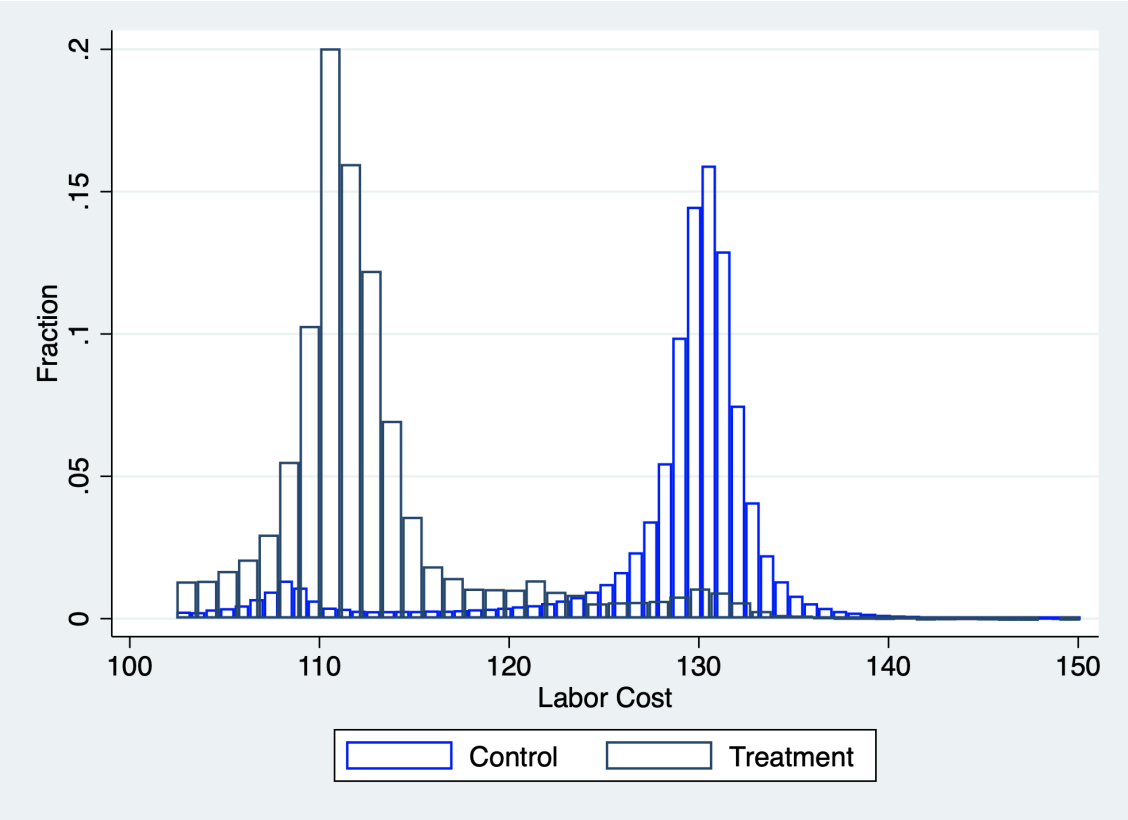
where, τ_{jt} is the tax paid[21] by firm j in year t; all other variables and fixed effects are identical to equations 1 and 2. To estimate the impact of the reform on labor cost, I rely on the IV defined by equations 9 and 10. Equation 9 estimates the first

[20] The revenue tax (CPRB) was specifically created in the scope of the payroll tax reform. Any company that waives from the payroll taxes due to the reform, have to pay CPRB.

[21] Depending on the specification τ_{jt} can be defined as payroll tax rate or comprehensive tax rate (payroll + revenue tax rate).

stage, which captures the take up rate, while equation 10 estimates the reduced form, that measures the labor cost impact on eligible firms. Table 6 reports the tax cut impact on the labor cost. Column (1) shows that labor cost declines by 14.3% (SE = 0.0012) according to the IV estimate. This estimate aligns with the reform's statutory payroll tax cut, which cuts labor costs from 1.31 to 1.12 of the total wage bill, i.e., $d\ln(1+\tau)$ = -0.145. This is reassuring that firms are biding to the legal statutory tax rates, and also serves as a sanity check to confirm that the IV is properly adjusting for the take up rate. Column (2) displays a 10.2% (SE = 0.0004) decline according to the OLS estimate, and column (3) reports a 8.5% (SE = 0.001) decline in the labor cost for eligible firms due to the tax program. Notice that the labor cost impact on eligible firms is smaller than in the IV estimate because there is imperfect compliance among the eligible group. Table 7 reports the impact on labor cost measured by the comprehensive tax cut. Column (1) reports that labor cost decreases 7.01% (SE = 0.0013) due to the reform. Column (2) displays the OLS estimate of -3.4% (SE = 0.0005), and finally column (3) presents the labor cost impact on eligible firms, which is -4.18% (SE = 0.0009).

Figure 4: 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 labor cost distribution.

In order to compute the elasticity of employment with respect to the labor cost I can fit an IV model where the first stage and reduced form are given by equations 11 and 12, respectively. The intuition for the IV is to estimate in the reduced form the percentage change in employment due to eligibility, while the first stage gives the percentage change in labor cost due to eligibility. The IV coefficient is the ratio $(\frac{\delta}{\pi})$.

$$\log(1+\tau_{jt}) = \alpha_j + \gamma_t + \xi_{s1,t} + \pi L_{s(j)t} + X_{jt} + u_{jt} \tag{11}$$

$$Y_{jt} = \alpha_j + \gamma_t + \xi_{s1,t} + \delta L_{s(j)t} + X_{jt} + u_{jt} \tag{12}$$

Notice that the IV requires detailed tax data to be estimated. However, the balanced sample of firms, which I have full tax data on, is more elastic than the

ones in the main firm level sample described in section 2.2. Table 5 computes the extensive margin responses[22] for the balanced sample, and reports in column (1) that the employment effect is 11.7%[23], instead of the 9.1% as estimated in section 3. Thus, for the main sample, the elasticity of employment with respect to the labor cost (1 + payroll tax rate) is equal to -0.63.[24] Defining labor cost based on the comprehensive tax rate, the elasticity is -1.3.[25]

The elasticity for the sample of balanced firms, which I have full tax information on, can be calculated by estimating the IV presented in equations 11 and 12. Table 8 reports the results. In this restricted sample, the elasticity of employment with respect to labor cost (1 + payroll tax rate) is -0.823 (SE = 0.111), as displayed in column (1). Column (2) reports the elasticity that accounts for the comprehensive tax rate, and it is equal to -1.671 (SE = 0.226). Equivalently, an alternative way to compute these elasticities is dividing the IV coefficient from column (1) in table 5 by the first stage coefficient from column (1) in table 6, which gives the same -0.82. The estimates align well under the two methods because they capture the same elasticity, the difference is that in the first approach it is measured responses by the eligible firms, whereas the alternative approach evaluates the elasticity based on the actual treated firms (adjusted by the IV).

In (Saez, Schoefer, and Seim 2019), they find a smaller elasticity of employment with respect to labor cost (-0.21). However, there are two caveats in order to compare these results. First, they estimate the elasticity for young workers that can be different from the overall economic elasticity. It is reasonable to imagine that the labor demand elasticity for youth workers is smaller because as a cheaper labor force, their hiring decision might be less dependent on tax incentives. Second, in the Swedish tax reform studied by (Saez, Schoefer, and Seim 2019) there might be pay equity constraints limiting firms' ability to respond to the policy, thus implying lower elasticities.

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. I show that the tax

[22] These estimates are based on the model presented in equations 1 and 2.

[23] Saez, Schoefer, and Seim 2019 also find larger employment effect when restricted to a balanced sample of firms.

[24] This is the percentage variation in employment (9.1%) divided by the percentage variation in the labor cost (-14.5%).

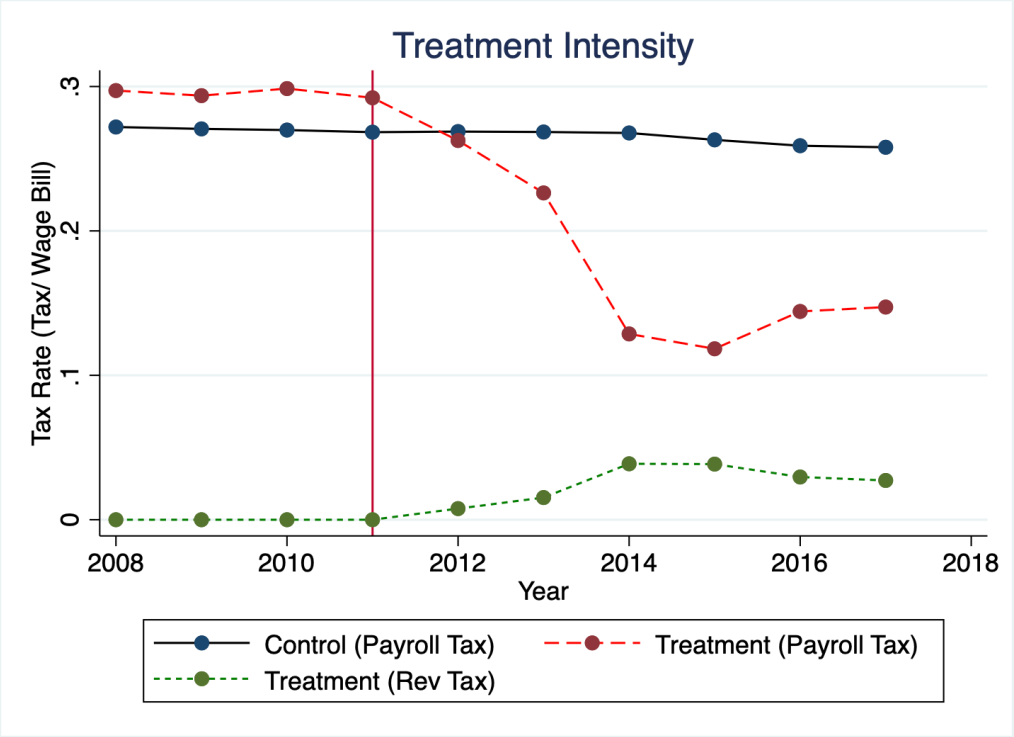
[25] This is the percentage variation in employment (9.1%) divided by the percentage variation in the labor cost (-7.01%).

benefit granted to firms is not passed through to incumbent workers' earnings, but firms expand in firm size by increasing the number of employees by 9.1%. I find an elasticity of employment with respect to labor cost of -0.63 which has implications for tax incidence and policy design.

8 Appendix

8.1 Figures

Figure 5: 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 6: Tax Forms Information

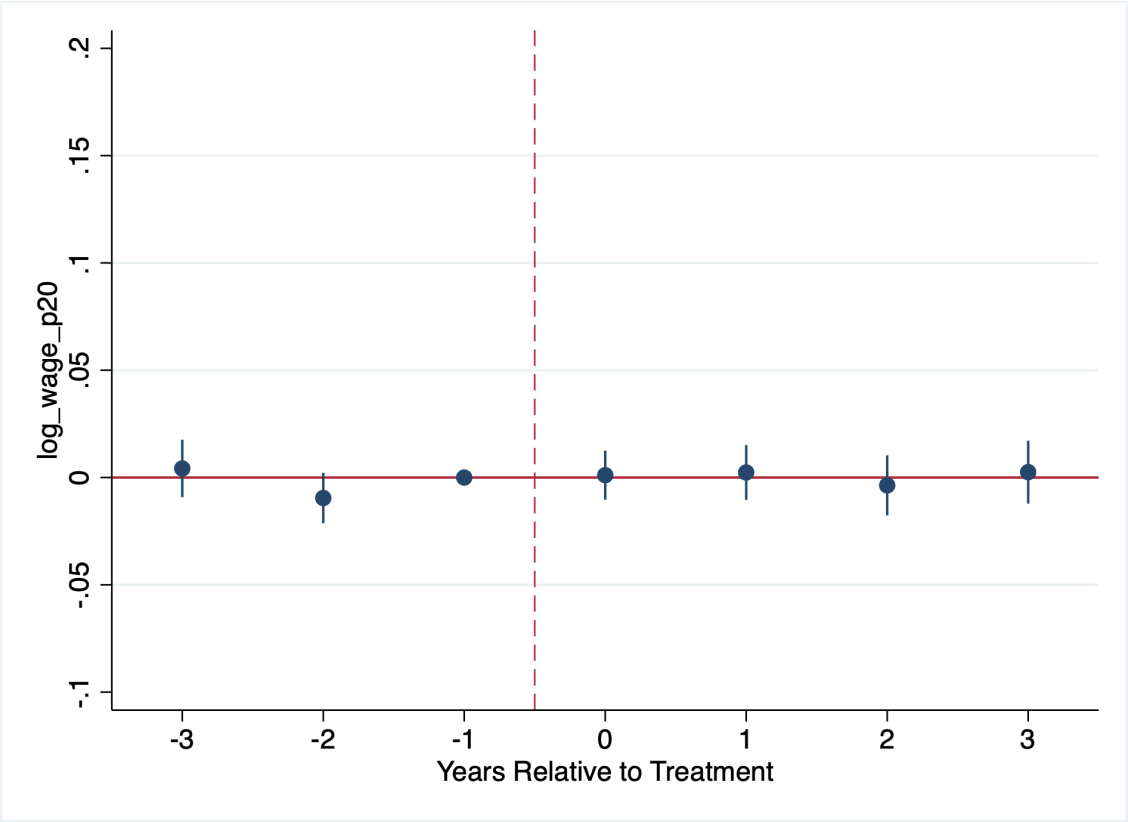
Nº	Campo	Descrição	Tipo	Tam	Dec	Obrig
01	REG	Texto fixo contendo “0145”.	C	004*	-	S
02	COD_INC_TRIB	Código indicador da incidência tributária no período: 1 – Contribuição Previdenciária apurada no período, exclusivamente com base na Receita Bruta; 2 – Contribuição Previdenciária apurada no período, com 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_DEMAIS_ATIV	Valor da Receita Bruta da(s) Atividade(s) não Sujeita(s) à Contribuição Previdenciária sobre a Receita Bruta	N	-	02	N
06	INFO_COMPL	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_TOT_EST	Valor da Receita Bruta Total do Estabelecimento no Período	N	-	02	S
05	COD_ATIV_ECON	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_ATIV_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_CONT	Valor da Base de Cálculo da Contribuição Previdenciária sobre a Receita Bruta (Campo 08 = Campo 06 – Campo 07)	N	-	02	S
09	ALIQ_CONT	Alíquota da Contribuição Previdenciária sobre a Receita B ruta	N	008	04	S
10	VL_CONT_APU	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_COMPL	Informação complementar do registro	C	-	-	N

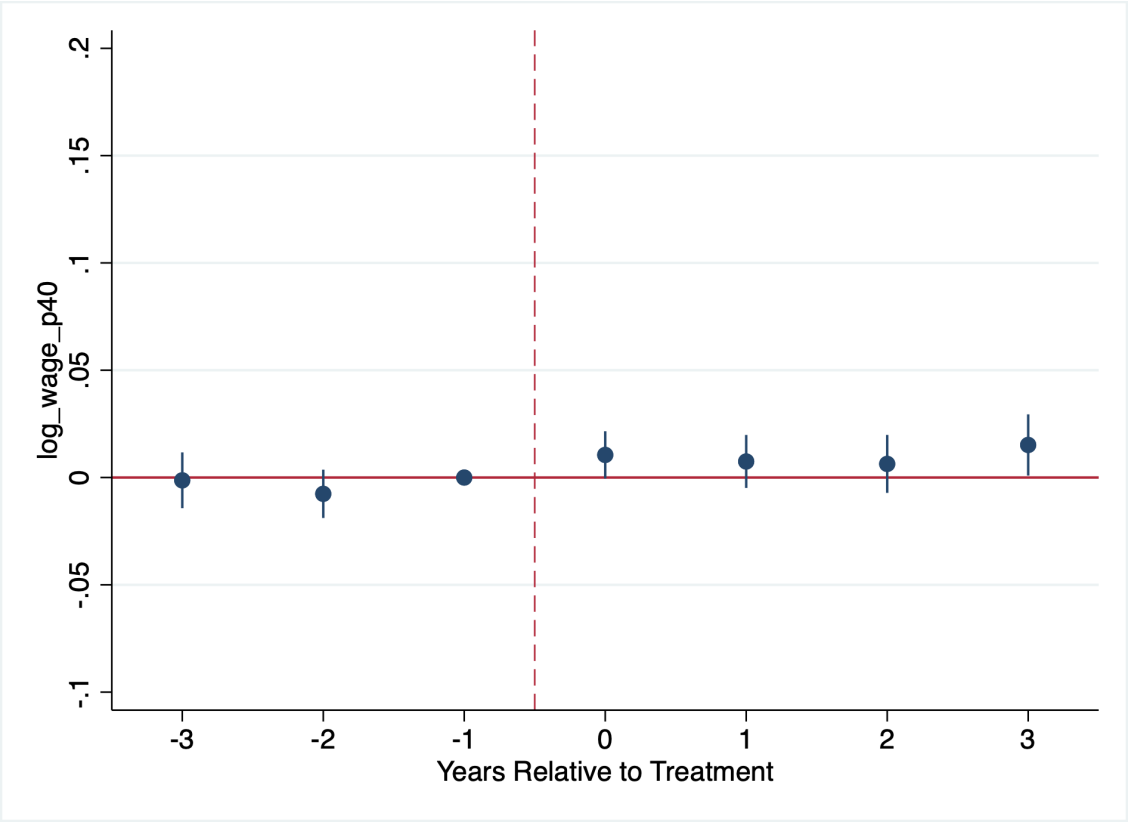
Note: This figure shows instructions for eligible firms to request the payroll tax benefit. It describes detailed information to be provided in Tax Administration software, in order to substitute part of the payroll tax by revenue taxes.

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



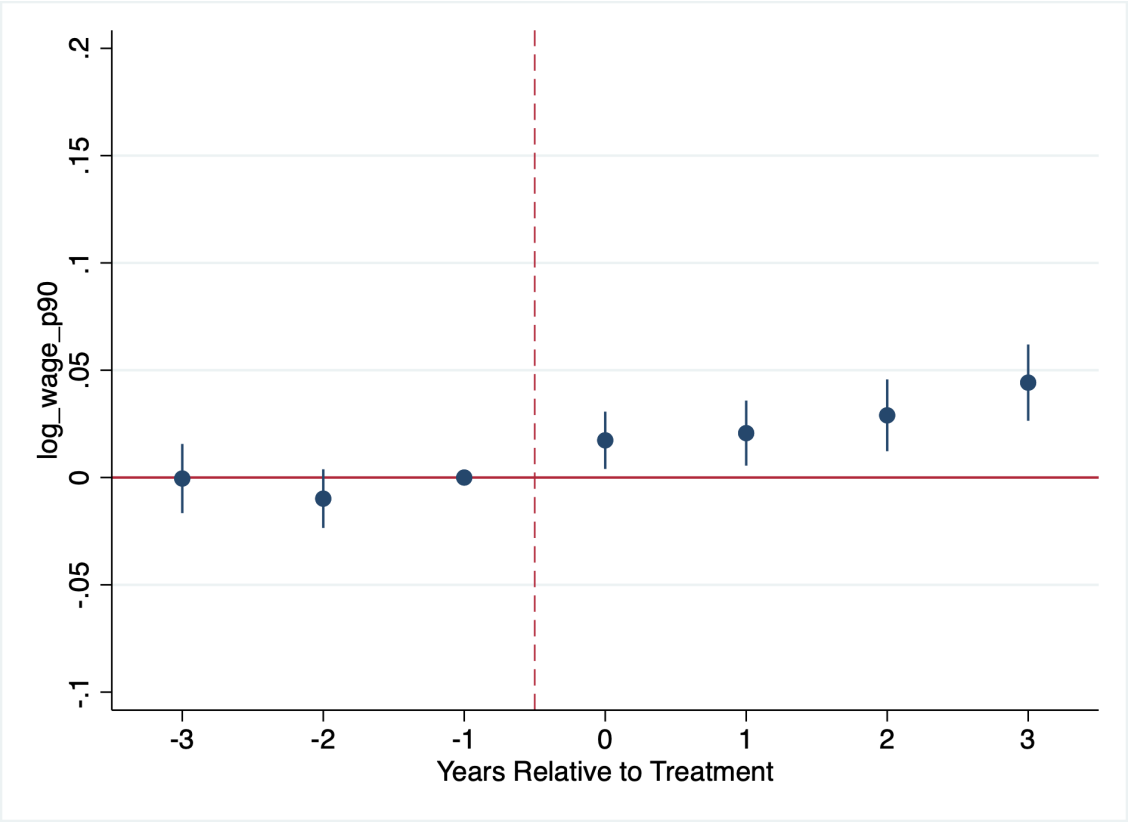
Note: This figure presents the event study estimates for wages at the percentile 20 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans three years prior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

Figure 8: Wages (Percentile 40): Event Study Estimates



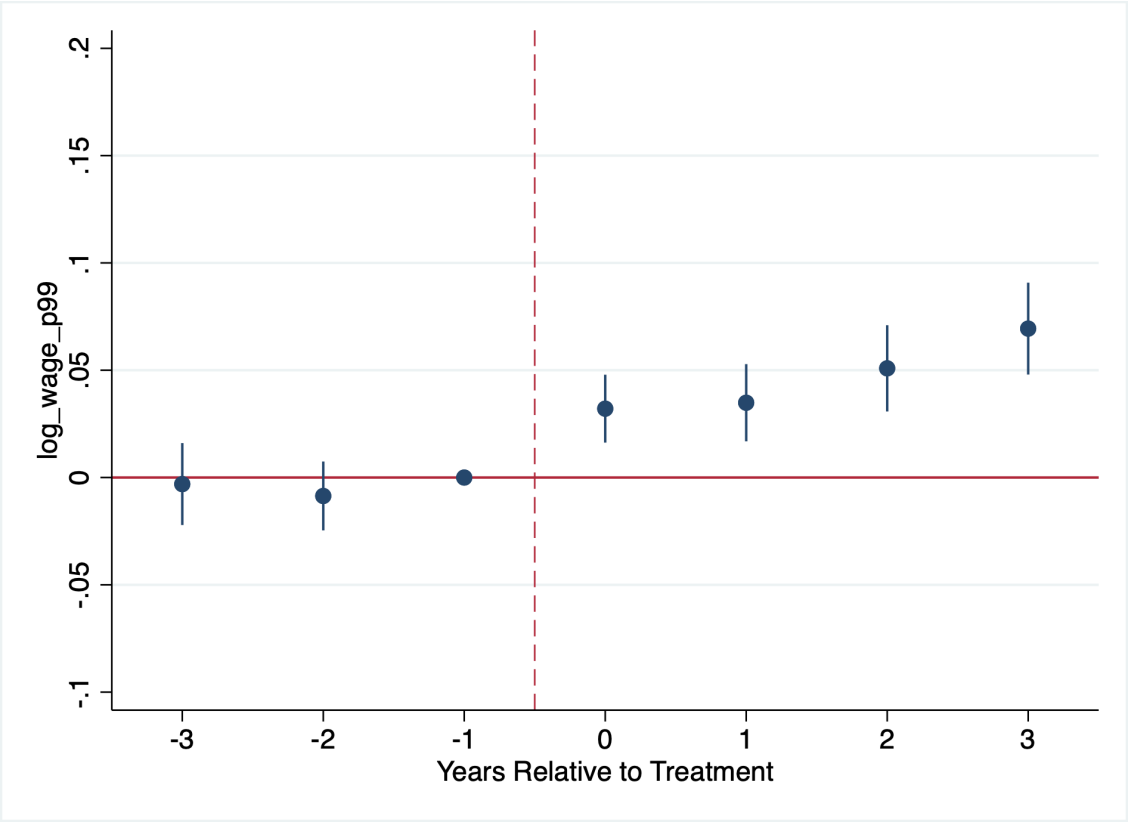
Note: This figure presents the event study estimates for wages at the percentile 40 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans three years prior to entering the payroll tax cut program and three 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 three years prior to entering the payroll tax cut program and three 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 three years prior to entering the payroll tax cut program and three years after.

8.2 Tables

Table 1: Descriptive Statistics: Firm Level Sample

	(1) Control (pre)	(2) Treated (pre)	(3) Avg (pre)
Descriptive Statistics			
Employment	44.19	41.09	43.98
	(905.57)	(274.76)	(876.96)
Earnings	996.84	1,196.75	1,009.97
	(1,023.58)	(1,093.45)	(1,029.51)
Hiring	21.01	20.65	20.99
	(325.89)	(140.89)	(316.68)
Tax Rate	28.46	28.99	28.49
	(8.70)	(9.11)	(8.72)
Gender	0.53	0.76	0.55
	(0.40)	(0.30)	(0.40)
Employees Age	36.37	35.80	36.33
	(8.84)	(7.79)	(8.78)
Firm Age	22.28	19.72	22.11
	(10.33)	(9.71)	(10.31)
High School +	0.55	0.58	0.55
	(0.41)	(0.38)	(0.40)
Share White	0.68	0.73	0.68
	(0.37)	(0.33)	(0.36)
N	2,232,526	164,143	2,396,669

Note: This table presents descriptive statistics of the baseline sample in the pre-reform period (2008 to 2011). The variable tax rate informs the average payroll tax rates in (%). 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. The variable "Share White" informs the average share of white workers per firm.

Table 2: Descriptive Statistics: Worker Level Sample

	(1) Control (pre)	(2) Treated (pre)	(3) Avg (pre)
Descriptive Statistics			
Earnings	2,204.00	2,076.64	2,196.44
	(2,974.08)	(2,766.35)	(2,962.31)
Employees Age	39.69	37.92	39.58
	(10.89)	(10.68)	(10.89)
Share White	0.66	0.65	0.66
	(0.47)	(0.48)	(0.47)
Gender	0.53	0.79	0.54
	(0.50)	(0.41)	(0.50)
High School +	0.69	0.60	0.69
	(0.46)	(0.49)	(0.46)
College +	0.28	0.17	0.28
	(0.45)	(0.37)	(0.45)
N	662,292	41,795	704,087

Note: This table presents descriptive statistics of the baseline sample in the baseline period from 2008 to 2011. The variable tax rate informs the average payroll tax rates in (%). 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. The variable "Share White" informs the average share of white workers per firm.

Table 3: Aggregated Estimates

	Log (Employment) Firm Level	Log (Earnings) Worker Level
Currently Treated	0.0910***	-0.00304
	(0.0130)	(0.00887)
Observations	5,280,162	1,345,969
Firm FE	Yes	Yes
Sector (1 digit) x Year FE	Yes	Yes
Worker_FE	No	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 first column is at the firm level, while the second is at the workers level. The instrument is the interaction between two indicators: one that flags sector eligibility and the other indicates if the time is post eligibility.

Table 4: Earnings Estimates (Firm Level)

	(1) Log(Earnings) firm level (avg)	(2) Log(Earnings) firm (99p)	(3) Log(Earnings) firm (90p)	(4) Log(Earnings) firm (40p)	(5) Log(Earnings) firm (20p)
Currently Treated	0.0160***	0.0432***	0.0265***	0.0115*	0.00120
	(0.00429)	(0.00687)	(0.00570)	(0.00448)	(0.00453)
Observations	5,280,162	5,280,162	5,280,162	5,280,162	5,280,162
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Sector (1 digit) x Year FE	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 sample is structured at the firm level, thus results are subjected to composition effects. The instrument is the interaction between two indicators: one that flags sector eligibility and the other indicates if the time is post eligibility.

Table 5: Employment - Balanced Sample

	(1) Log (Employment)	(2) Log (Employment)	(3) Log (Employment)
Currently Treated	0.117***		0.127***
	(0.0158)		(0.00500)
Post x Eligible		0.0703***	
		(0.00950)	
Age	0.000546	0.000634	0.000540
	(0.000722)	(0.000723)	(0.000722)
Gender Composition	1.211***	1.213***	1.211***
	(0.0120)	(0.0121)	(0.0120)
High School	0.433***	0.433***	0.433***
	(0.00786)	(0.00787)	(0.00785)
College	0.780***	0.781***	0.780***
	(0.0154)	(0.0154)	(0.0154)
White	0.903***	0.905***	0.903***
	(0.0103)	(0.0111)	(0.0108)
Constant			1.837***
			(0.0163)
Observations	2,230,536	2,230,536	2,230,536
Model	IV	Reduced	OLS
Firm FE	Yes	Yes	Yes
Sector (1 digit) x Year FE	Yes	Yes	Yes

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

Note: This table presents the extensive margin employment effects for the balanced panel of firms that appear in the tax records. Column (1) presents the IV estimates, while Column (2) displays the impact on eligible firms and column (3) the OLS results.

Table 6: First Stage: Cost of Labor (1 + payroll tax rates)

	(1) Log(Labor Cost) (1 + Payroll Tax)	(2) Log(Labor Cost) (1 + Payroll Tax)	(3) Log(Labor Cost) (1 + Payroll Tax)
Currently Treated	-0.143***	-0.102***	
	(0.00123)	(0.000478)	
Post x Eligible			-0.0850***
			(0.00101)
Age	-0.000503***	-0.000524***	-0.000607***
	(0.0000528)	(0.0000528)	(0.0000550)
Gender Composition	-0.00216*	-0.00300***	-0.00447***
	(0.000898)	(0.000899)	(0.000927)
High School	-0.000979	-0.00123	-0.00145*
	(0.000652)	(0.000652)	(0.000684)
College	-0.00456***	-0.00510***	-0.00616***
	(0.00111)	(0.00111)	(0.00116)
White	-0.00524***	-0.00589***	-0.00731***
	(0.000821)	(0.000858)	(0.000880)
Constant		0.252***	
		(0.00119)	
Observations	2,252,356	2,252,356	2,252,356
IV	Yes	OLS	Reduced
Firm FE	Yes	Yes	Yes
Sector (1 digit) x Year FE	Yes	Yes	Yes

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

Note: This table reports the first stage impact of the reform, i.e., how much payroll tax rates were affected by the reform. Column (1) presents the IV results, which adjust eligibility by the take up rates. Column (2) displays the OLS coefficient, and finally, column (3) displays the payroll tax changes in eligible firms due to the reform.

Table 7: First Stage: Cost of Labor (1 + comprehensive tax rates)

	(1) Log(Labor Cost) (1 + Comprehensive Tax)	(2) Log(Labor Cost) (1 + Comprehensive Tax)	(3) Log(Labor Cost) (1 + Comprehensive Tax)
Currently Treated	-0.0701***	-0.0383***	
	(0.00133)	(0.000483)	
Post x Eligible			-0.0418***
			(0.000865)
Age	-0.000518***	-0.000535***	-0.000569***
	(0.0000535)	(0.0000535)	(0.0000539)
Gender Composition	-0.00217*	-0.00282**	-0.00330***
	(0.000914)	(0.000913)	(0.000917)
High School	-0.000989	-0.00119	-0.00122
	(0.000663)	(0.000662)	(0.000668)
College	-0.00484***	-0.00525***	-0.00562***
	(0.00113)	(0.00113)	(0.00114)
White	-0.00518***	-0.00569***	-0.00619***
	(0.000850)	(0.000866)	(0.000867)
Constant		0.252***	
		(0.00120)	
Observations	2,252,356	2,252,356	2,252,356
IV	Yes	OLS	Reduced
Firm FE	Yes	Yes	Yes
Sector (1 digit) x Year FE	Yes	Yes	Yes

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

Note: This table reports the first stage impact of the reform, i.e., how much comprehensive tax rates (payroll + revenue tax rates) were affected by the reform. Column (1) presents the IV results, which adjust eligibility by the take up rates. Column (2) displays the OLS coefficient, and finally, column (3) displays the payroll tax changes in eligible firms due to the reform.

Table 8: Employment Elasticities

	(1) Log (Employment)	(2) Log (Employment)
Log Cost of Labor (payroll tax)	-0.823***	
	(0.111)	
Log Cost of Labor		-1.671***
		(0.226)
Age	0.000154	-0.000271
	(0.000722)	(0.000730)
Gender Composition	1.210***	1.208***
	(0.0120)	(0.0120)
High School	0.432***	0.432***
	(0.00784)	(0.00788)
College	0.777***	0.772***
	(0.0153)	(0.0154)
White	0.899***	0.894***
	(0.0104)	(0.0105)
Observations	2,230,536	2,230,536
Instrument	Eligible	Eligible
Firm FE	Yes	Yes
Sector (1 digit) x Year FE	Yes	Yes

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

Note: This table presents IV estimates to compute employment elasticities with respect to the cost of labor for the balanced sample of firms that I have detailed tax data on. In column (1) labor cost is defined as (1 + payroll tax rate), while in column (2) it is (1 + comprehensive tax rate). The comprehensive tax rate is the payroll plus the revenue tax rate (CPRB). The CPRB was created in the reform, and the only firms subjected to it, are the ones that waive from payroll taxes.

8.3 Robustness Checks

[TO BE INCLUDED]

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