The Incidence of Payroll Taxation

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[VERY PRELIMINARY - PLEASE DO NOT CIRCULATE]

Abstract

I study a corporate tax reform targeted at the sector and product level, in Brazil. Difference-in-differences estimates instrumented by eligibility show that a 20 percentage point cut on payroll tax rates caused a 10% employment increase at the firm level, mostly driven by small firms. This expansion doesn't change the relative share of occupations employed, and is not driven by formalization of existing workers. In terms of earnings, it takes time for workers to benefit from a pass-through. On average there is a 1.8% (indistinguishable from zero) earnings increase. However, the event study estimates show a sharp zero earnings effect in the short run, and a significant 4% increase in the long run. Merging tax and labor data with the universe of collective bargaining agreements (CBAs) in Brazil, I provide suggestive evidence that the pass-through to earnings is augmented for unionized workers. The exogenous variation on labor cost allows me to compute the elasticity of labor demand with respect to wages of -0.54 and labor supply faced by firms of 6.67. In a framework of monopsony in the labor market, this result implies that Brazilian workers produce 15% more than their wage level.

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1 Introduction

Payroll tax cuts are an expensive and pervasive policy across the globe[1]. On average, payroll taxes are responsible for 25% of total tax collection in OECD countries (OECD, 2019). These expensive policies are often rationalized by the classical assumption on aggregate labor demand being much more elastic than labor supply, which suggests that payroll taxes are borne by workers. Indeed the 2018 Congressional Budget Office relies on this assumption to predict the impact of payroll taxes in the US.

However, the community of scholars lack consensus on the labor market implications of payroll tax cuts. Part of the literature points out that workers bear the incidence, by showing zero employment, but positive pass-through to earnings (Gruber 1997; Gruber and Krueger 1991; Gruber 1994; Cruces, Galiani, and Kidyba 2010). At the other extreme, recent studies point to positive employment and zero earnings response (Saez, Schoefer, and Seim 2019; Kugler, Kugler, and Prada 2017). A third strand of literature reports results in between, with a partial pass through to earnings (Hamermesh 1979; Holmlund 1983; Kugler and Kugler 2009). At the center of the policy debate there are two underlying questions: What are the labor market implications of a payroll tax cut? Why hasn't the literature arrived at a consensus yet?

One reason for the lack of consensus in the literature is that most of the reforms studied in the past face at least one, out of the two common identification concerns. First, most payroll tax cut programs studied in the past are targeted to specific workers (based on earnings, tenure or age), thus it is difficult to disentangle the effects of the reform from pay equity norms within firms. For example, if two workers perform similar tasks and differ across one dimension that is targeted by the policy, say worker's age, then it can be challenging for employers to differentiate wage of these similar workers (for a summary on the pay equity norms implications to labor market outcomes, see Dube, Giuliano, and Leonard 2019; Breza, Kaur, and Shamdasani 2018). 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 overcome these challenges by exploiting a large-scale payroll tax reform in Brazil that targeted sectors and products, rather than workers. The setting alleviates the pay equity concerns, as all employees in a given firm face the same tax variation. At the same time, the Brazilian reform provided identification

[1] US, Brazil, Chile, Italy, Colombia, Greece and Sweden are recent examples, just to cite a few.

because not all the firms became eligible for the tax cut. In December, 2011 the Government enacted a major corporate tax reduction aimed to reduce labor cost, and increase competitiveness of the domestic economy. Initially, the policy targeted a few sectors[2] and products[3], with gradual expansion of eligibility[4] in subsequent years. The staggered implementation aspect is exploited in the empirical design together with the fact that most firms are never treated, which allows me to use treatment take up and eligibility to estimate the causal effect of the reform based on an IV model.

At the firm level, the effect on average wages can be driven by pass through to wages and composition of the labor force. To disentangle these two underlying forces, I constructed two samples, one at the firm level and one at the worker level. I combine a granular set of tax and labor administrative data on the universe of formal firms operating in Brazil between 2008 and 2017. The firm identifier allows me to also merge the data to the universe of union contracts signed in the country, in order to leverage heterogeneity analysis. To exploit regional variation on informality, I merge the data to the national Census. The final dataset provides a comprehensive laboratory of the Brazilian economy, and allows me to have a clear understanding of the responses to the tax reform.

I use this data to fit an event study model instrumented by sector eligibility to estimate the causal effect of the reform on the labor market. The importance of the IV in this context is because there is imperfect take-up in eligible sectors[5], and also because treatment is observed in non-eligible sectors due to the product eligibility criteria. I show that being agnostic about these two margins of adjustment lead to bias in the OLS estimates. The IV approach is only possible due to the firm level data, which allows me to observe treatment on the firm level. The data and econometric method allows me to conduct heterogeneity analysis at the worker, firm and market level.

I find that the corporate tax cut causes a sharp expansion on firms' employment, with limited effects on earnings. The employment analysis is leveraged at the firm level to capture the effect of the tax reform on businesses. I find a 10% employment increase, which is mostly driven by small firms. This result is consistent with findings in the Industrial Policy literature, which finds that Government subsidies to firms are more effective to boost employment on small business (Zwick and

[2]IT, call center and lodging

[3]Mostly manufactured goods.

[4]Maintanance, transportation and media became eligible between 2013 and 2014

[5]Kleven and Waseem 2013 provide evidence that some firms don't respond even in tax dominated regions.

Mahon 2017; Criscuolo et al. 2019; Howell 2017; Bronzini and Iachini 2014). The setup of the Brazilian payroll tax reform is appropriate to connect with Industrial Policies, since both of them offer shocks at the firm, rather than worker level.

Given the underlying payroll tax variation induced by the reform, the implied elasticity of employment with respect to labor cost is -0.68. The large employment effect doesn't affect the between occupation sorting of workers, and leads to a statistically significant positive effect on the average earnings at top percentiles of the within firm wage distribution. To analyze the pass through to earnings, I follow the displacement literature (Jacobson, LaLonde, and Sullivan 1993, Lachowska, Mas, and Woodbury 2020) to build a sample of incumbent workers, who are assigned to treatment based on their pre-reform employers. At the worker level, I find a 1.8% increase in earnings, which is indistinguishable from zero at standard confidence levels. The earnings effect remains statistically insignificant across multiple workers characteristics, such as tenure, gender and race.

The estimates are robust to a wide variety of approaches, and I provide evidence that the identification assumption holds. The identifying hypothesis is that eligibility is uncorrelated with the outcomes of interest, conditional on fixed effects. There are two main threats to identification. First, as in standard difference in differences, the design is compromised if parallel trends do not hold. This would be violated if the Government selects eligibility in a way that anticipates trends on the outcomes of interest. Second, the results would be biased if there were strategic selection into eligible sectors.

The formal and standard test for parallel trends is evaluating the statistical significance of pre-trends. I show not only that the pre-trends aren't statistically significant for any of the outcomes, but also that eligibility is balanced in levels. Eligibility is not correlated with firms and workers characteristics in the pre-reform period. The result is robust to multiple estimation methods. As an alternative identifying strategy, I leverage a matching difference in differences to show that the results are qualitatively similar to the main empirical design. In this approach, I match each treated firm to a never treated one that is similar in the pre-reform period. In table 3, I provide a list of sectors across eligibility groups, showing that there is no remarkable difference between them.[6]

The second threat is about strategic selection into eligible sectors. I show that results are robust to eligibility assignment in the pre-reform period. Also, as a robustness check, I restrict to firms that have never changed sectors and the results are similar. I noticed from this exercise that very few firms actually change sectors,

[6]For example, open television is eligible, but cable television is not.

which suggests that this is not an easy margin of manipulation. When I focus on the firms that have changed sectors, I can show that there is not a trend of switching towards eligible sectors. All of these together is reassuring that the results are not driven by firms self selecting into eligible sectors.

I discuss a few models that are helpful to interpret the results (in progress).

Next, I turn the discussion to two heterogeneities relevant to developing economies. The first one is regarding informality. One might be concerned that the employment result is mechanically driven by formalization of existing workers, rather than an additional rise in employment caused by the reform. I exploit the Brazilian regional diversity in terms of informality to provide evidence that the employment expansion is not driven by highly informal areas. Second, I compare the treatment effect on firms unionized versus non-unionized[7] to provide a novel contribution on the effect of unions on the implications of corporate tax reforms. I find that the tax cut in unionized firms leads to (in progress).

Finally, I expand the tax reform analyses beyond the tension between employers and employees, to evaluate the tensions between employers and the Government. It turns out that the policy design offers pervasive incentives for revenue misreporting, which leads to a set of unintended consequences on tax evasion. An interesting element of the Brazilian tax reform is that the payroll tax reduction is followed by a small positive revenue tax. Based on previous literature it is widely known that perturbations on the revenue tax schedule generates incentive for firms to under report revenue (Lobel, Scot, and Zúniga 2020; Bachas and Soto 2021; Londoño-Vélez and Ávila-Mahecha 2019). In the Brazilian context, I can offer suggestive evidence of this type of evasion response as an unintended consequence of the reform.

The rest of the paper is organized as follows. In Section 2 I discuss the institutional background and the data. Section 3 presents the empirical strategy and the main findings, including heterogeneity analysis. Section 4 analyzes the impacts of institutions on the implications of tax reforms. Section 5 discusses a few models in light of the empirical findings. Section 6 focuses on unintended consequences of the tax reform and issues related to tax evasion. Section 7 concludes.

2 Institutional Background and Data

The Brazilian payroll taxes are designed to fund social security programs, such as retirement pensions and unemployment insurance. In December 2011, the Government enacted a major corporate tax reduction aimed to reduce labor cost, and

[7] defined in the pre-period

increase competitiveness of targeted sectors. The reform provides interesting variation because eligible and non-eligible firms present similar trends and levels in the period immediately before the reform.

2.1 Brazilian Payroll Tax System and the 2012 Reform

The Brazilian payroll tax system is similar to most OECD countries, however the tax reform was different. In Brazil, it was targeted at the firm level, while most of the reforms studied in the past were targeted at the worker level. This type of targeting provides an advantageous quasi-experimental design to study the labor market implications of payroll taxes on the labor market, rather than on a specific type of worker. It is also advantageous because reforms the pass-through to wages on worker targeted reforms can be confounded by pay equity.

The Brazilian payroll tax schedule has three components, and all of them are collected directly from firms. The main component is a 20% flat tax over the total wage bill. Secondly, there is an accident risk insurance component that varies between 1 to 3%[8]. 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. All of these tax components are deposited in a social security fund that pools resources together. This implies that the public social security system does not provide individual savings accounts, where resources are traceable and mapped to specific workers' benefits.

On 14^{th} December, 2011 the Brazilian Federal Government announced the payroll tax cut program[9] that waived the main component of the payroll taxation, which means a tax cut equivalent to 20 percentage points of the total wage bill. To provide slightly compensation to the Government budget in face of this large drop in tax collection, the benefited firms were imposed to pay a small 1 to 2.5% taxes on net of exports gross revenue. Figure 1, provides evidence that the reform should be interpreted as a large corporate tax cut, rather than a tax substitution. Eligibility for the payroll tax exemption is sector and product specific. The first tax bill outlining the policies and the eligible sectors was passed in December 2011, and implemented a few months immediately after, April 2012. This type of corporate tax cut has never been implemented previously in Brazil, so this was not an expected policy by firms and workers. The policy is still valid nowadays[10], and

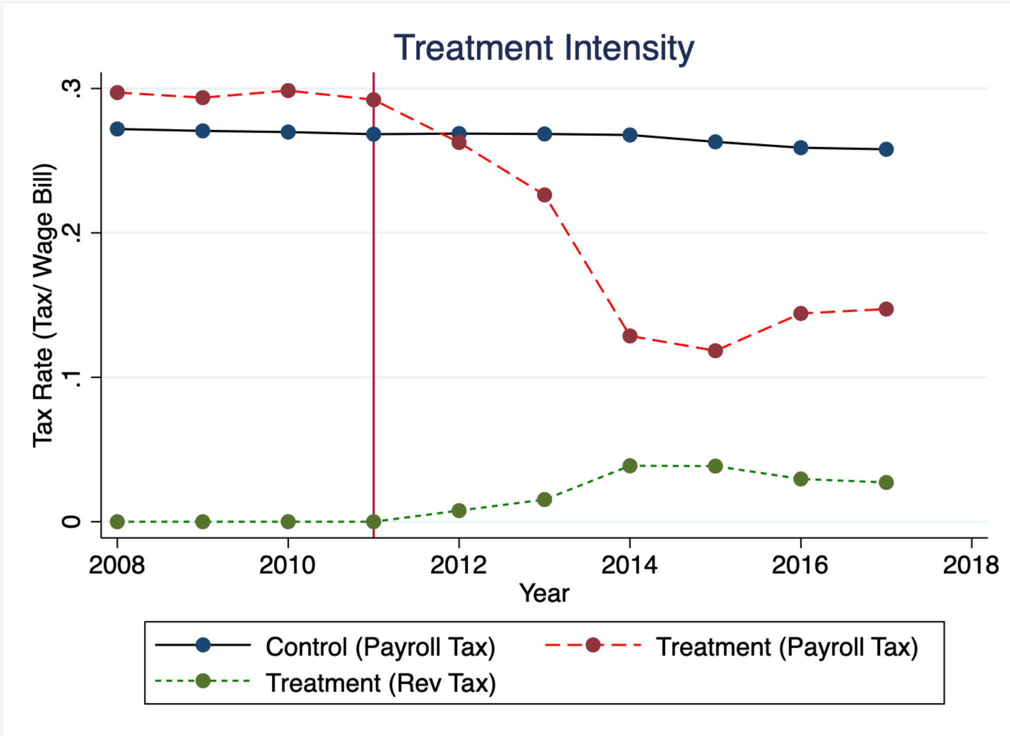
[8]This tax varies according to the activity associated risk

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

[10]As of March, 2022

there is no expectation of being eliminated in the near future.

Figure 1: Tax Implication of the Reform



Note: This figure presents the 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, 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 are substituted in once treatment takes place. Revenue tax rates are computed as a function of the total wage bill in order to facilitate comparisons.

The reform has a staggered implementation characteristic because after the first cohort of sectors that became eligible in 2012, there were several other tax bills including more sectors to the reform[11] Another interesting variation is that within broad defined sectors, the reform did not provide eligibility to all subsectors. For example, in 2012 the lodging industry became eligible to the reform, as the subsectors of hotels were contemplated. However, other subsectors in the lodging industry, such as motels, did not become eligible. Similarly, table 3 provides multiple examples of similar subsectors in broad defined industries, where one of them became eligible and the other not.

Regarding the product eligibility criteria, the tax bills define eligibility based on the Mercosur Common Nomenclature (NCM). Most of the product eligible firms are

[11]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.

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[12]. 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.

Over the years, 5 other tax bills[13] were passed promoting 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%[14]. 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 eligible sectors. Indeed, the imperfect take-up rate is a central aspect of the reform that deserves more discussion, as one might be puzzled to understand why an eligible firm wouldn't take such generous Government benefits.

There are a few facts that help to rationalize the imperfect take-up. First, the tax bills never mentioned any punishment to non-compliers. Possibly because from the legislative point of view eligibility 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. Second, enrollment in the program was not automatic as in the Swedish case studied by (Saez, Schoefer, and Seim 2019)[15]. In Brazil, firms have to self-report eligibility on Government provided software to enable tax exemptions[16], through separate tax forms that are required to be filled out. Figure 10 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, empirical findings in other countries (Kleven and Waseem 2013) suggest that the operational filling process can lead to non responsiveness even in dominated tax regions.

The legislative decision process to define eligible sectors was political, and didn't seem to anticipate sector specific labor outcome levels, or trends. Sections X and Y are dedicated to provide details on the eligibility rules, and to test levels and trends

[12]This can be precisely observed in the micro tax data, but more broadly can also be seen in the sector level data publicly available on the Brazilian tax authority website (link).

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

[14]Law 13.161/2015

[15]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.

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

of eligible sectors. The pre-trends observed in the event studies (figures X and Y) together with the pre-reform balance reported in table 4 is reassuring. 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.

2.2 Data and Descriptive Statistics

I constructed two samples, one at the firm and other at the worker level, by combining tax and labor administrative data on the universe of formal firms operating in Brazil between 2008 and 2017. The firm identifier allows me to merge this data to the universe of union contracts signed in the country during the pre-reform period. To exploit regional variation on informality, I merge the data with the 2010 Census. The advantage of this data is that it allows me to track firms and workers over time, which constitutes an ideal laboratory to understand the effect of corporate tax policies on very granular measures of labor market outcomes. The disadvantage is that it doesn't provide the same level of detail on informal labor markets.

Tax Data. The firm level tax data comes from three forms, which are structured in the firm-year level. 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. First is the tax form, in which firms inform the tax authority about the total wage bill, i.e., the tax base for payroll taxation. This form is named *Guia de Recolhimento do Fundo de Garantia do Tempo de Serviço e Informações à Previdência Social* (GFIP). Firms have incentive to truthfully report this tax because workers do not participate in the pension system if employers don't report and collect taxes on the total wage bill. The second tax reform relies on the previous to compute the actual payroll tax 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, which is collected from the firm. The third form is specific to the reform studied in this paper. This form is named *Contribuição Previdenciária Sobre a Receita Bruta* (CPRB), and it is used to compute the revenue tax liability for firms that waive from the payroll tax. As only payroll tax waived firms are liable to CPRB, this tax form is also useful to construct dummies for treatment assignment. The base for CPRB is the net of exports gross revenue.

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[17], sector (at its most granular definition), firm size, wage bill, age and location. At the worker level, it contains variables regarding employment status, occupation, wage, race, gender, industry, municipality, as well as hiring and termination dates. Workers and firms are uniquely identified based on tax codes (PIS and CNPJ, respectively), which do not change over time. The main shortcoming in RAIS is the lack of information about informal and non-employed workers.

I use other sources of administrative data to complement this dataset. The 2010 Census provides information that allows me to compute formalization rates at each of the 5,300 Brazilian municipalities. Finally, from the Ministry of Labor (MTE), I obtained data on the universe of unionization contracts in the country. This data is structured in the firm x union x year level, it contains 1 million observations from 2008 to 2017, and allows me to study the earnings response to the tax policy according to the unionization status. Once this set of administrative data is merged, I construct two samples for the empirical analysis, one at the firm level, and the other at the worker level.

Firm Level Sample. To make the administrative data suitable to study the payroll tax reform in Brazil there are a few sample restrictions that are important to deal with specificities of the context. First, I exclude from the sample firms that have ever participated in the "Simples Nacional", which is a special tax tier not subjected to the payroll taxes studied in this paper. In the Brazilian corporate tax schedule there is a special tax tier named "Simples Nacional", which has never been subjected to the payroll taxes. The "Simples" is designed for small firms[18]. Firms in the "Simples" regime face a different tax tier which consolidates all tax liability in a single tax form with simplified and 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 tiers.

In terms of sector comprehensiveness, the sample encompasses 19 out of 21 one-

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

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

digit sectors[19] of the Brazilian economy. The construction sector is excluded because the treatment assignment to this sector was problematic. The tax bill allowed construction firms to be treated in only certain of its 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.

The sector of repair and sale of motor vehicles was 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. 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 evaluate the results on a balanced panel of firms (the ones that appear in all ten years of the sample) to relieve concerns with firms' attrition.

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 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 1,858,835 observations in the pre period (2008-2011). These firms are allocated in 19 one digit sectors that

[19] 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).

are broken down into 1,072 seven digit CNAE industries. Table 4 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 53.6 workers receiving an average monthly earning of $1,070 BRL (approximately $200 USD[20]). Each firm hired an average of 25.12 workers per year, and the labor force is 69% white. In terms of educational background, firms present an average share of 53% high school graduates. Detailed descriptive statistics for the worker level sample can be found in table 5.

3 Main Findings

The corporate tax cut causes a sharp expansion on employment, with limited effects on wages. In this section, I present details about the main results, including heterogeneity analysis across firm size and workers characteristics.

3.1 Empirical Strategy

The main empirical strategy is an event study instrumented by the sector eligibility. The design explores the staggered implementation of the program, together with the fact that there is a large share of firms never eligible or treated by the reform. The IV is important to adjust for two margins the imperfect take-up in eligible sectors; and also adjust for treatment in non-eligible sectors due to the product eligibility criteria. I fit similar models at the firm and worker level. Conditions for the LATE Theorem hold, thus IV estimates can be interpreted as average causal effects of tax cuts on employment and wages for compilers. At the firm level the estimated structural equation is,

$$Y_{jt} = \sum_{k=-4,\neq-1}^{3} \beta_k D_{jt}^k + X_{jt}'\gamma + \alpha_j + \xi_{s1(j),t} + \epsilon_{jt} \tag{1}$$

where, Y_{jt} is the outcome of interest; D_{jt} indicates that firm j is treated in year t; X_{jt} are set of controls (e.g., education, gender, race, age and its square); $\xi_{s1(j),t}$ is 1-digit sector interacted with year fixed effect; αj is the firm fixed effect; and k indexes the time relative to treatment.

For each time t relative to treatment, there is one respective first stage equation. Thus, in total there are K first stage equations given by,

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

$$D_{jt}^{k} = \sum_{l=-4,\neq -1}^{3} \pi_{kl} \times \mathbb{I}(t = e_{s(j)} + l) \times L_{s(j)} + \alpha_j + \xi_{s1(j),t} + X_{jt}'\delta_k + \eta_{jt},$$
$$\forall k \in [-4, -2] \cup [0, 3] \quad (2)$$

where, $e_{s(j)}$ is the event date, in which firm j's sector becomes eligible; $L_{s(j)}$ indicates if firm j's sector is eventually eligible; and the remaining coefficients are the same as described before. The standard errors are clustered at the 5 digit sector level. Appendix 9 provides more details on the empirical model, and outlines the reduced form equations.

The event study design provides two main advantages. First, it validates the identifying assumption by showing that the pre-reform coefficients of interest are not statistically different from zero. Second, it provides intuition about the dynamics of the treatment effect relative to the year before the event. I combine the event study set up and the 2SLS framework to estimate the average treatment effect on compilers. The pooled version of the difference-in-differences model is outlined in equations 3 and 4.

$$D_{jt} = \pi L_{s(j)t} + \alpha_j + \gamma_t + \xi_{s1(j),t} + X_{jt} + u_{jt} \quad (3)$$

where, D_{jt} indicates that firm j is 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, α_j is the firm fixed effect. Because eligibility is defined at the industry level, standard errors are clustered at the 5-digit industry level.

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 4,

$$Y_{jt} = \delta L_{s(j)t} + \alpha_j + \gamma_t + \xi_{s1(j),t} + X_{jt} + u_{jt} \quad (4)$$

Identification relies on the timing of eligibility being uncorrelated with the outcomes of interest, conditional on the fixed effects. The key identifying assumption is that firms' outcomes for eligible and non-eligible firms would have followed parallel trends in k>0, in the absence of the tax reform. I test this assumption in a set of checks summarized in section 3.3. One of the tests consists in showing that the pre-reform coefficients of interest are not statistically significant.

The firm level sample can impose challenges to evaluate the earnings effect. One might be concerned that at the firm level, the average earnings can be affected by compositional changes in the labor force. To address this concern, I take advantage of the granularity of the micro data, to estimate a similar model at the worker level.

The first challenge to leverage a worker level analysis is to define treatment assignment. Since workers are mobiles across eligible firms and sectors, it is not obvious how to assign them to treatment. 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 their pre-reform employer, and then evaluate individuals' outcomes regardless of the firms that they end up working for. Thus, $L_{s(i,t_0)}$ is equal to one if firm j's pre-reform sector eventually becomes eligible. Similarly, to the firm level specification, the pooled difference-in-differences model at the worker level is given by,

$$D_{it} = \pi L_{s(j_0)t} + \theta_i + \alpha_j(i,t) + \xi_{s1(i,t_0),t} + X_{it} + u_{it} \tag{5}$$

$$Y_{it} = \delta L_{s(j_0)t} + \theta_i + \alpha_j(i,t) + \xi_{s1(i,t_0),t} + X_{it} + u_{it} \tag{6}$$

, where i indexes workers, Y_{it} is workers' labor market outcome in year t; θ_i is the worker fixed effect; $\alpha_j(i,t)$ is the firm fixed effect; and the remaining variables and fixed effects are analogous to definitions in equations 3 and 4. Similarly to the firm level analysis, I also fit the event study model to the worker level sample. The structural and first stage equations are presented below,

$$Y_{it} = \sum_{k=-4,\neq -1}^{3} \beta_k D^k_{j(i,t_0)t} + X'_{it}\gamma + \theta_i + \alpha_{j(i,t)} + \xi_{s1(i,t_0),t} + \epsilon_{it} \tag{7}$$

$$\begin{aligned} D^k_{j(i,t_0)t} = &\sum_{l=-4,\neq -1}^{3} \pi_{kl} \times \mathbb{I}(t = e_{s(i,t_0)} + l) \times L_{s(i,t_0)} \\ &+ \theta_i + \alpha_{j(i,t)} + \xi_{s1(i,t_0),t} + X'_{it}\delta_k + \eta_{it}, \forall k \in [-4,-2] \cup [0,3] \end{aligned} \tag{8}$$

where, $D^k_{j(i,t_0)t} = 1$, if $t = e_{j(i,t_0)} + k$; $e_{j(i,t_0)}$ is the year when the pre-reform firm enters treatment; $e_{s(i,t_0)}$ is the year when the pre-reform sector becomes eligible; and the remaining variables and fixed effects are the same as defined before. Standard errors are clustered at the 5-digit industry level.

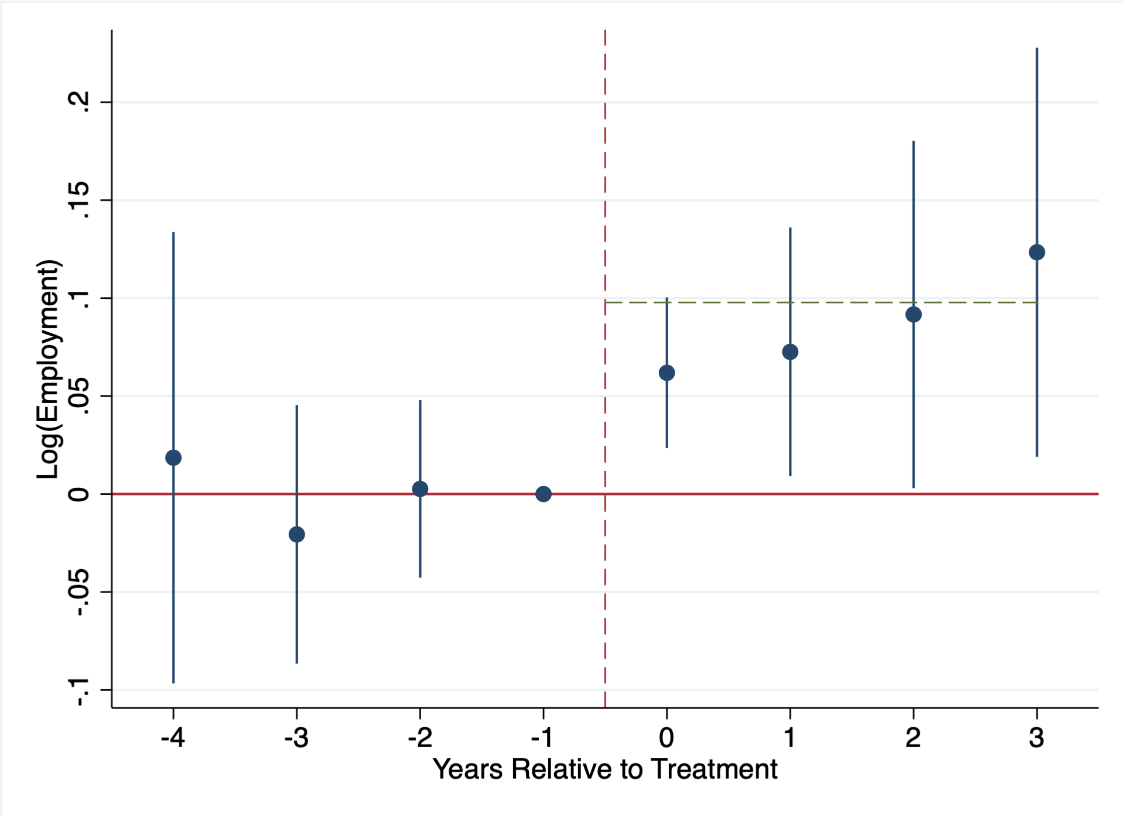
3.2 Results

I start presenting the positive and statistically significant results at the firm level, which are driven by smaller firms. Then I move to the worker level results to show that the pass through to earnings due to the tax cut comes only in the longer run, and doesn't vary across workers characteristics.

3.2.1 Firm-level

I fit equations 3 and 4 using the firm level data, to find a 10% employment increase, i.e., participation in the payroll tax program causes a 10% employment increase (SE = 0.013) in treated firms relative to control. Table 6 presents the estimates, which corresponds to an elasticity of employment with respect to labor cost of -0.68. The employment effect is driven by large firms, and it doesn't affect the between occupation sorting of workers. There is a statistically significant effect on the average earnings at top percentiles of the within firm wage distribution. In terms of dynamics, figure 2 reports estimates from equations 2 and 1, which shows that as the reform kicks in, there is an immediate employment response that is sustained and slightly increased over time. The dashed horizontal line in the upper right part of the figure reports the local average treatment effect on compliers of 10% estimated based on equations 3 and 4.

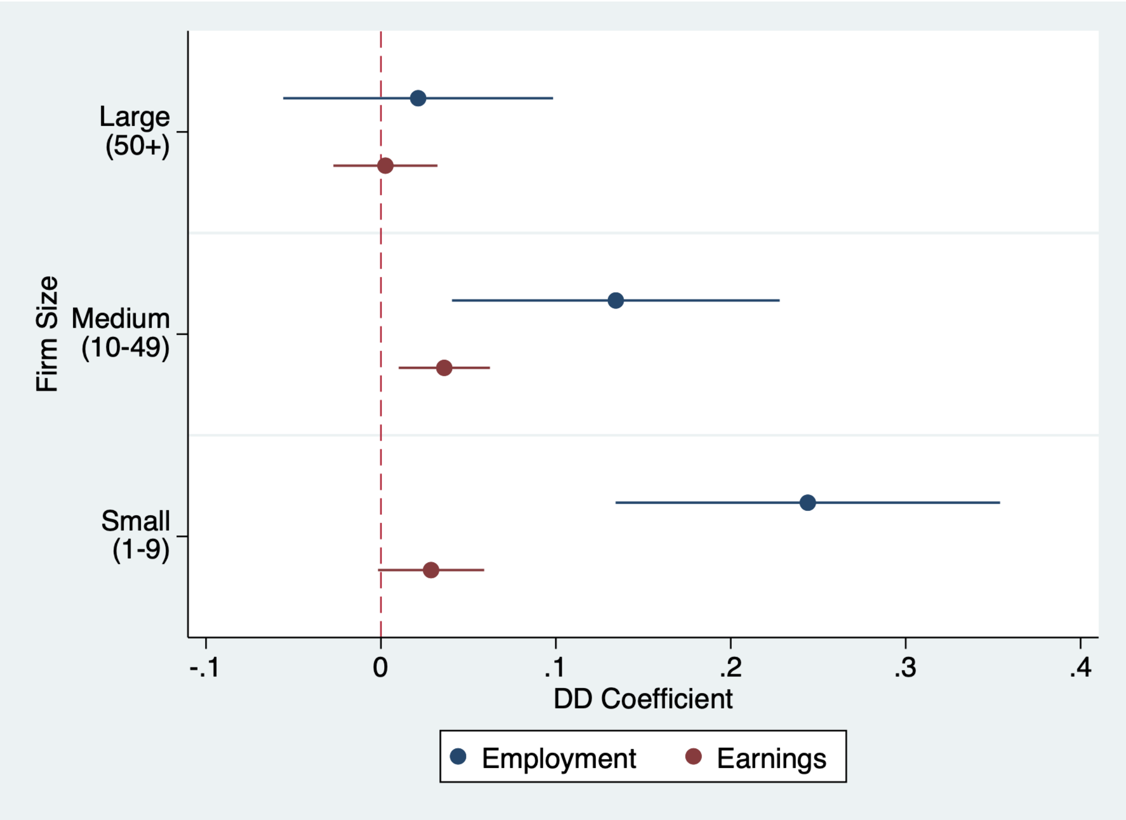
Figure 2: Employment: Event Study Estimates



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. Standard errors are clustered at the 5-digit sector level.

Next, I fit equations 3 and 4 within three firm size categories (small, medium and large). These categories are defined in the pre-reform period, i.e., prior to 2012. Firms are classified as small if they had less than nine employees, medium if they had between 10 and 49 workers, and large if they had more than 50 workers. Figure 3 reports the results on the size heterogeneity analysis. The blue markers show that the employment effect monotonically decreases with the firm size groups, and the employment increase is statistically different between small and large firms. The red markers show that the firm level earnings effect are small, barely distinguishable from zero, and are not statistically different between size categories. However, there is a statistically significant difference across firm level average earnings in upper versus lower within firm percentiles.

Figure 3: Firm Level: Heterogeneity Analysis



Note: This figure presents the event study estimates for the firm level estimates, for three firm size groups (small, medium and large firms). Size categories are defined in the pre-reform period. The blue marks plot the employment difference-in-differences coefficient, while the red markers plot the firm level earnings effect. Standard errors are clustered at the 5-digit sector level.

Elasticities. To compute the employment elasticity with respect to the labor cost, first we need to estimate the labor cost variation induced by the reform.[21] In the context of the Brazilian reform, the cost of labor is defined as the wage bill $\times$ (1 + payroll tax rate). Figure 11 plots firm level distribution of labor cost among treated and control firms, in the post period. Average labor cost for control firms is 131%, whereas for treated firms is 112%, which is consistent with the statutory rates. To estimate the labor cost variation, I rely on the IV outlined by equations 9 and 10. Equation 9 estimates the first stage, which adjusts for the imperfect compliance,

[21]Even though there is a small revenue tax implemented in this reform, payroll taxes are the one directly affecting the labor cost. There might be other margins affecting sensitivity of employment, but certainly the labor cost is a margin of interest.

and equation 10 estimates the labor cost variation on eligible firms.

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

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

where, τ_{jt} is the payroll tax rate paid by firm j in year t; all other variables and fixed effects are identical to equations 3 and 4. As usual, the IV coefficient of interest is given by the ratio $\beta_{IV} = \frac{\delta}{\pi}$. Table 8 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 131% to 112% of the total wage bill, i.e., $d\ln(1+\tau)$ = -0.145. It is reassuring that the IV estimates aligns with the statutory tax cut. It serves as a sanity check to confirm that the IV is properly adjusting for the imperfect compliance. Column (2) reports a 8.5% (SE = 0.001) decline in the labor cost for eligible firms due to the tax reform. The impact on eligible firms is naturally smaller because some eligible firms don't face the payroll tax cut.

Thus, the elasticity of employment with respect to the labor cost (1 + payroll tax rate) is equal to -0.68[22]. 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 elasticity to all workers. 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.

The payroll tax reform implies a reduction on labor cost, which is an exogenous expansion of the labor demand curve. I use the wage response due to the exogenous variation on labor demand to compute the elasticity of labor demand and labor supply with respect to wages[23]. I find $\epsilon_D = 0.54$, and $\epsilon_S = 6.67$. It is important to highlight that the labor supply elasticity relates to the elasticity faced by the firm, or by the treated unit (sector). This elasticity is different from the market labor supply elasticity, which should be much smaller. The rationale is that when wages are shocked at the sector level, the mobility across sectors allow workers to be more

[22] 9.78% divided by the payroll tax variation ($d\ln(1+\tau)$ = -0.145

[23] The algebra for the computation of the elasticities are detailed in the appendix 9

responsive compared to a market wide shock.

The labor supply elasticity at the sector level ($\epsilon_S = 6.67$) is high, but not too far off from other recent studies. For instance, Azar, Berry, and Marinescu 2019 found that firms face a labor supply elasticity of 5.8, and Dube, Giuliano, and Leonard 2019 found elasticity of 4.6. The labor supply elasticity found for the Brazilian market implies that firms can reduce wages by roughly 15% below the marginal product of labor.

Within Firm Earnings Distribution. To evaluate the distributional consequences of the earnings effect, I fit the event study models in equations 3 and 4 for a new set of outcome variables: average earnings per percentiles of the within firm distribution. Even though this analysis is leveraged at the firm level (therefore, subjected to compositional changes), it is a valid result to evaluate the effect of the tax reform on the within firm earnings inequality. Table 7 displays the aggregate estimates from equations 3 and 4. Column (1) shows that average earnings on treated firms increase by 1.84% (SE = 0.0048) 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 percentile of the within firm earnings distribution. As we move from the top to the bottom percentiles the average 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 4.86% (SE = 0.0076), 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 2.89% (SE = 0.0063) 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 2 and 1. The results are presented in figures 12, 14 and 15, 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.

Occupation. The granularity of the labor data allow me to compute the firm level employment per occupation group[24]. I split employees into two occupation groups: leaders and operational workers. Leaders are directors, managers and qualified technical positions, while operational workers occupy the remaining positions. In figure 16 it can be noticed that the employment of leaders gradually increases as the reform phases in. The employment effect of leaders three years after the tax cut *(t=3)* is statistically greater compared to the year of implementation *(t=0)*. This dynamic is not observed for the employment of operational workers, who face uniform response over time. In terms of firm level earnings per occupation, figure 17 shows that the log of average earnings for operational workers haven't been affected by the reform, while the average earnings for leaders present a gradual increase over time. Next section offers suggestive evidence that the disparate earnings effect across occupations cannot be rationalized by the minimum wage.

Next, I turn to study whether the firm level employment effect is driven by within or between occupations. One might wonder, if the firm expansion due to the corporate tax cut is driven by more employment of the same type of workers, or instead the firm employs from an upscale occupation position, to improve management over operational employees? To leverage this analysis I exploit the granularity of CBO occupation data, which contains 2,300 occupation codes. I ranked these occupations based on the pre-reform average earnings, and group them in percentiles according to the earnings ranking. Therefore, I can assign an index to each firm year based on the average occupation percentile that they employ from. Column (3) of table 6 shows that there is a sharp zero effect of the reform on firms' average occupation percentile. This fact favors the narrative that the tax reform affects employment within occupation, rather than between occupations.

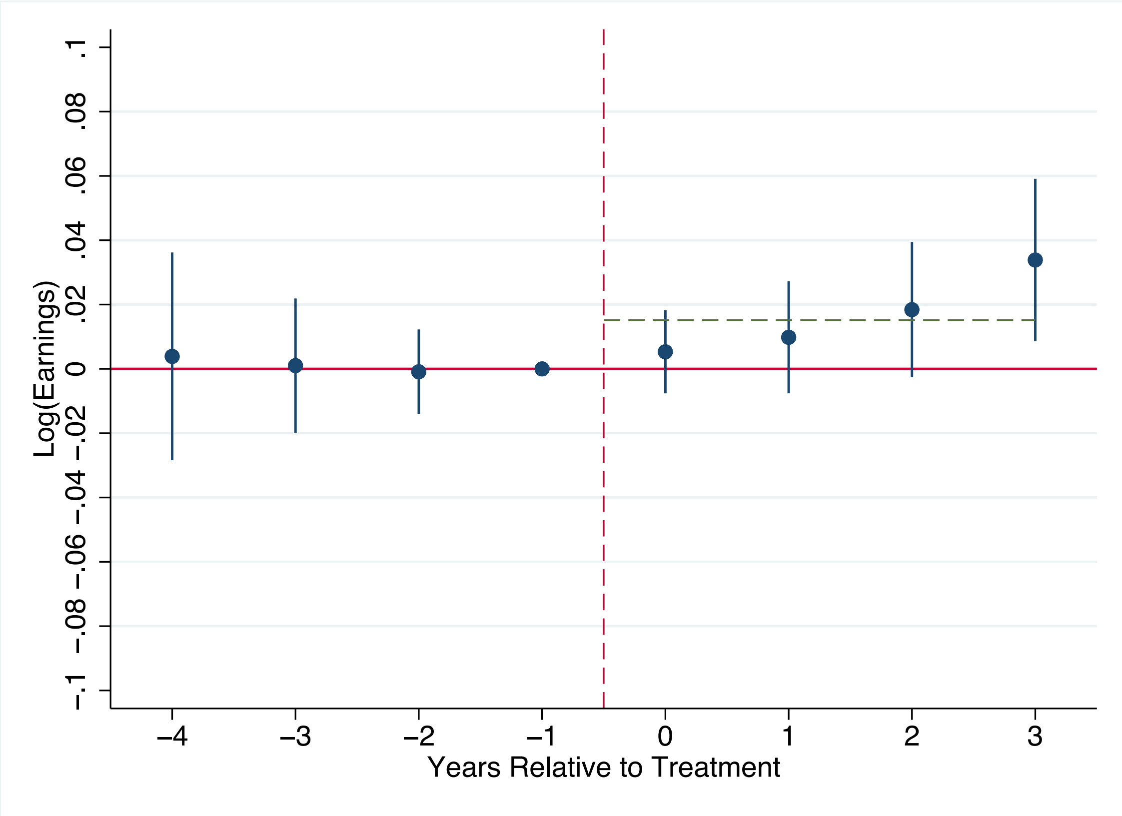
3.2.2 Worker-level

To evaluate the pass-through of the tax benefit to incumbent workers, I fit equations 5 and 6 in the worker level sample. Even though, the gross earnings paid by the firm sharply drops after the reform (figure 6), I find that the net earnings (net of payroll taxes) received by employees presents only a modest increase of 1.8%, which is indistinguishable from zero at standard confidence levels. However, the

[24] I rely on the CBO (Classificação Brasileira de Ocupação) for the occupation codes.

results from equations 7 and 8 show that in the long run there is a 4% positive and statistically significant pass-through to net earnings. As depicted in figure 4, takes time for the earnings effect to show up and it only becomes significant three years after the tax cut. I rule out potential explanations that the small earnings effect is driven by minimum wage constraints, and I don't find a heterogeneous earnings response across workers' characteristics.

Figure 4: Worker Level: Net Earnings Effect

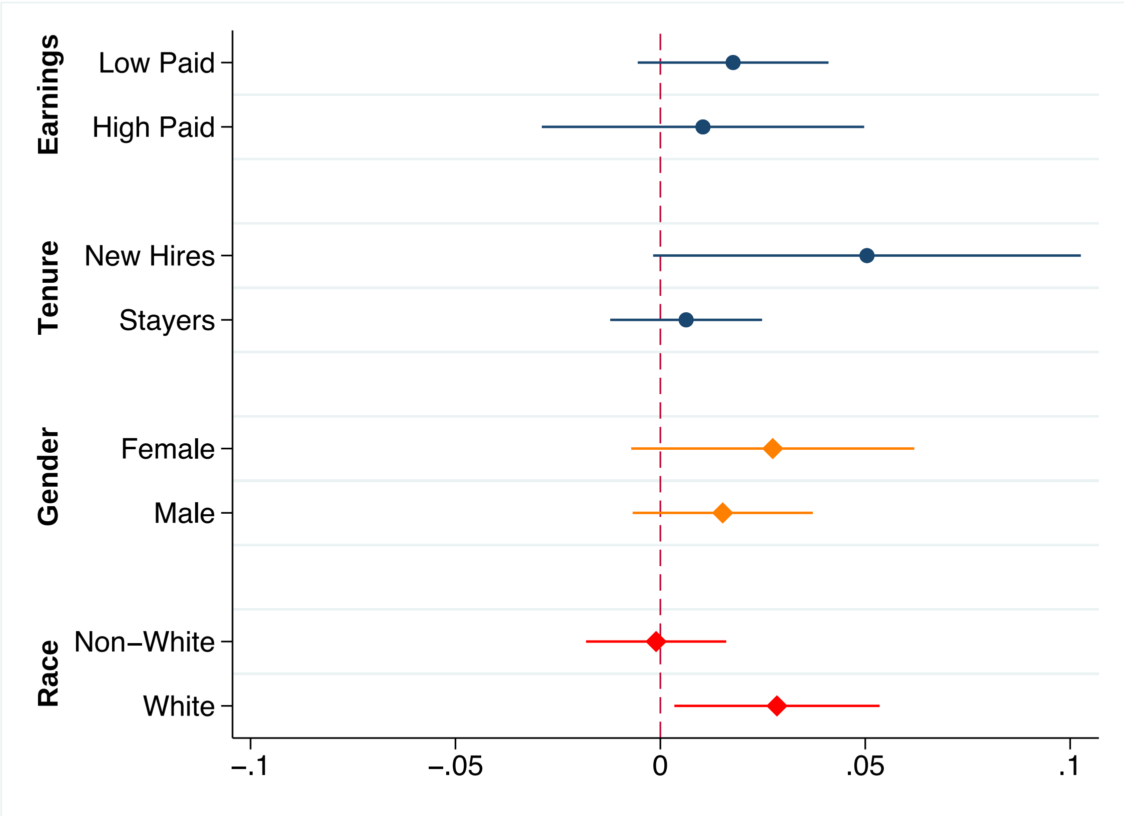


Note: This figure presents the event study estimates for average earnings (net of payroll taxes) for workers that were employed for at least three years in the same firm during the pre-reform period. I normalize the results with respect to one year prior to the treatment event. The analysis spans four years prior to the payroll tax cut program and three years after. The dashed horizontal line in the upper right part of the figure reports the local average treatment effect on compilers of 1.8% estimated based on equations 3 and 4. Standard errors are clustered at the 5-digit sector level.

Heterogeneity. The granular worker level data allows me to evaluate if the earnings response to the tax cut varies according to workers' characteristics. I show in figure 5 that treatment effect is not statistically different between none of the margins of heterogeneity studied. However, there are two margins to be highlighted.

First, there is an economic divergence between the pass-through to new hires versus stayers. The caveat to this result is that in my sample of incumbent workers, the new hires are workers employed during the pre-reform period, so this measure is not accounting for the earnings effect to new hires that were previously non-employed. Second, I want to highlight the deterioration of the racial pay gap. Even though the differential earnings effect is not statistically significant, one can notice that there is a significant positive pass-through to white workers' earnings, while non-white employees face a sharp zero pass-through.

Figure 5: Worker Level: Summary of Heterogeneities on Earnings Effect

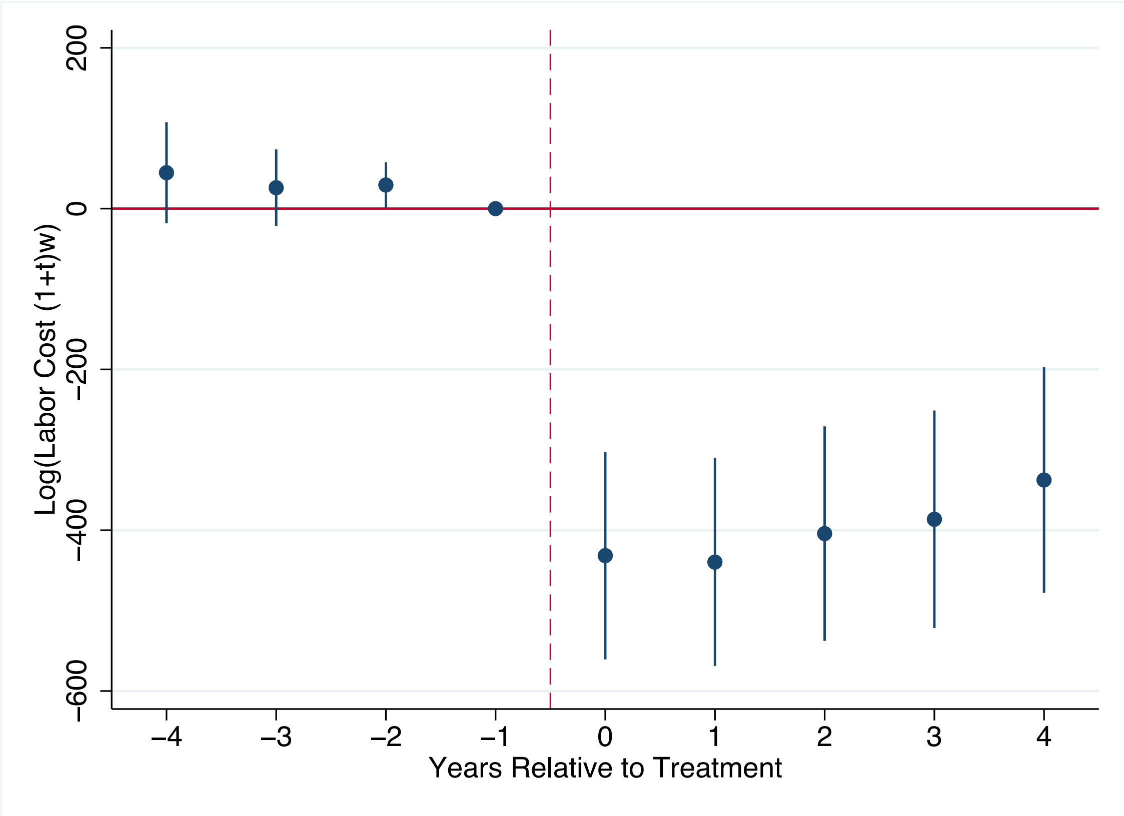


Note: This figure presents the pooled difference-in-differences coefficient for the earnings effect at the worker level, across many characteristics of interest, such as, income, tenure, gender and race.

Gross Earnings. In Brazil, firms are responsible to collect the payroll taxes, thus the difference between gross and net earnings is the gap between what employers pay and how much employees receive. Even though workers didn't observe substantial net earnings gains due to the reform (figure 4), it is important to note that firms did face a large and sharp decrease in the gross earnings paid to workers (figure

6). To compute the gross earnings, I use firm level annual tax and payroll data to obtain measures of firms' payroll tax rates per year. I apply these rates to workers net earnings to obtain the annual gross earnings of all workers in the sample. The pre-reform average gross earnings is $2,000 BRL and it drops $400 BRL (20p.p) immediately after the reform.

Figure 6: Worker Level: Gross Earnings Effect



Note: This figure presents the event study estimates for average gross earnings paid workers that were employed for at least three years in the same firm during the pre-reform period. The labor cost is computed using firm level tax data, and worker level earnings data. I apply the firm payroll tax rate in year t, to all employees in that firm in year t. I normalize the results with respect to one year prior to the treatment event. The analysis spans four years prior to the payroll tax cut program and three years after. The plot shows an average decrease of $400 on the gross earnings, which has an approximate average of $2,000 during the pre-reform period. Standard errors are clustered at the 5-digit sector level.

Minimum Wage. One might wonder if the small earnings effect presented in figure 4 is driven by minimum wage constraint. The idea underlying this argument is that as labor demand expands, the new wage for minimum wage workers can still be under the minimum wage constraint. Thus for workers binding on the minimum wage, there won't be any observable earnings effect. Figures 22 and 23 suggest that this is not the case, as there is no statistical difference between earnings below and above the minimum wage barrier. To leverage this analysis, I classify workers into the minimum wage categories based on the modal pre-reform minimum wage status. On average 20% of workers in the sample are constrained by the minimum wage. This percentage grows to 30% in large firms, decreases to 10% in medium firms, and 5% in small firms[25] (figures 18 and 19).

More work is in progress to understand the reasons underlying the positive earnings effect for workers constrained by the minimum wage. One possibility is that the high informality levels, and the low minimum wage in Brazil makes it hard for the "equilibrium" wage[26] to be very far down from the minimum. This fact combined with the large shift in labor demand induced by the reform makes it unlikely that "equilibrium" wages would remain under the minimum wage.

3.3 Threats to Identification

The identifying assumption on the main difference-in-differences specification is that eligibility to the tax benefit is uncorrelated with the outcomes of interest, conditional on fixed effects. The main threat to the validity of this assumption is if the Government has anticipated sector specific trends when eligibility rules were defined. Another concern is that firms strategically select into sectors once the reform is announced. In this section, I provide multiple tests to address both of these concerns. I show that eligibility choices were a result of a political process that was not optimizing to anticipate sector specific trends. I also show that sector change is a difficult margin of manipulation and firms are not operating in this margin.

Regarding the concern with sector specific trends, I start by following the most standard and formal way to address this threat, which is testing the pre-trends. Second, on top of similar trends, I show that firms (and workers) are balanced in levels across eligibility groups. Third, I show that results are robust to alternative estimation methods, such as matching difference-in-differences. Fourth, analyzing sectors featured in the tax bill, I provide intuitive evidence that their eligibility status was not driven by correlation of sector specific trends to outcomes of interest. Regarding strategic selection into sectors, I first show that the results are robust to pre-reform sector assignment. Second, I show robustness to a sample that restricts to firms that have never changed sectors. Third, I show that only a few firms

[25] Small firms defined as the ones with 1-19 employees, medium with 20-99, and large with more than 100 employees in the pre-period.

[26] The wage that would have been observed in the absence of the minimum.

have actually changed sectors, and there is not a trend of switching towards eligible sectors.

3.3.1 Selection on Eligibility

I rely on the event study design to show that the pre-reform coefficients of interest are not statistically significant, for all of the outcomes of interest. This means that treated and control groups were following similar trends when the reform was enacted. To address the balance in levels, I show in figures 20 and 21 that workers and firms' characteristics are not correlated with eligibility.[27] The baseline model shows that is unlikely that characteristics are able to explain eligibility choices. The one characteristic that is more concerning regarding balance is gender, which I control for in the main specifications. Also, the results of interest are estimated on a two way fixed effect model, therefore I use this model to test balance and I find sharp zero difference across eligibility groups (figures 20 and 21).

On top of the statistical test, I provide more anecdotal evidence that the political process that determined eligibility was not seeking to anticipate sector specific trends. In table 3, I share a non-exhaustive list of similar sectors that are plausibly following the same trends, but present different eligibility status. For instance, the sector of hotels were eligible, but motels were not. The industry of open television is eligible, but cable television is not. The list goes on, and more examples can be found on table 3.

After all, if the reader is still not convinced that sector eligibility was not correlated with sector trends, I show that the results are qualitatively similar under alternative empirical strategies that rely on alternative identification assumption. I repeat the analysis using a matching difference-in-differences empirical strategy, in which I match each eventually treated firm to one never treated firm. Notice that the group of eligible sectors differ from treated firms because of the imperfect compliance discussed in section 2.1, thus the matching difference-in-differences strategy does not assume anything about the political process that defines eligibility. There are other threats[28] to the matching difference-in-differences, but since the results are qualitatively similar in both strategies, this is reassuring to the reader who was still not convinced about the validity of the IV design. In sum, the IV relies on

[27] I do so by estimating the baseline OLS model: $L_{s(j)t} = X_{jt} + u_{jt}$, and the TWFE model: $L_{s(j)t} = X_{jt} + \alpha_j + \gamma_t + \xi_{s1(j),t} + u_{jt}$, where $Ls(j)t$ is a dummy to indicate if the firm (worker) is eligible in year t; X_{jt} are characteristics of the firm (worker); and the fixed effects are the same used in all empirical specifications presented before.

[28] I run a placebo test and show pre-reform balance to address these threats to the matching difference-in-differences design. The next paragraph details the tests.

the assumption that eligibility is uncorrelated with sector trends, and I show that we can obtain similar results using an alternative method that doesn't rely on this assumption.

The matching algorithm goes as follows. First, I match firms that belong to the same deciles on employment, wages and hires during the pre-reform years. A propensity score is fitted and applied to break eventual ties. The main concern with this approach is that firms can be similar in levels, but different in trends during the pre-reform period. I eliminate this concern by showing that the pre-trends are indistinguishable from zero. I ran a few other 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 zero wage effects, providing evidence that the results are not driven by any inconsistency in the matching algorithm. In another test, I show that treated and control firms are balanced in levels of pre-reform characteristics.

3.3.2 Manipulation on Sectoral Choice

Another threat to the design is if firms could strategically change sectors after the reform was announced. In that case, firms with expectations for employment growth could have self selected into treatment, and therefore the results of the paper could not have been interpreted as caused by the policy. I show in this section that sector manipulation is a difficult margin of manipulation[29], and firms are not actively using it. I show in the data that there are only a small number of firms changing sectors, and even among those, there are not a trend of switching towards eligible sectors.

To reassure that firms that have changed sectors are not driving the results, I run a few extra robustness checks. First, I assign firms to eligibility groups based on their pre-reform sectors. This way, I don't allow firms to strategically enter eligibility by sector manipulation. If the estimated results on eligible firms were sensitive to this manipulation, the results should disappear on this robustness check. However, I show that the results remain qualitatively the same. Similarly, I restrict the sample to firms that have never changed sectors, and the results don't change. All these tests taken together, indicate that sector manipulation is not an active margin of

[29]Firms in the regular tax tiers (object of this study) face a long bureaucratic process to change sectors. They would first have to change their operating agreement, which requires proof that they are operating in a new industry. Then they need to request new operational licenses in multiple administration offices such as the city hall, state, federal tax authorities, and others. Failing in one of these steps can imply tax compliance fines.

response, which reinforces the causal interpretation of the results.

4 Institutional Settings of Developing Economies

This section is dedicated to study heterogeneities that are mostly prevalent in developing economies. The primary goal of this analysis is to clarify that the results are not driven by particular institutional settings of developing economies. I take advantage of the fact that Brazil is a large and diverse developing economy with some local labor markets that reassemble developed countries. I exploit this variation to disentangle the effects of a corporate tax reform in settings with different degrees of exposure to institutions typical of developing economies, such as informality and unionization.

4.1 Informality

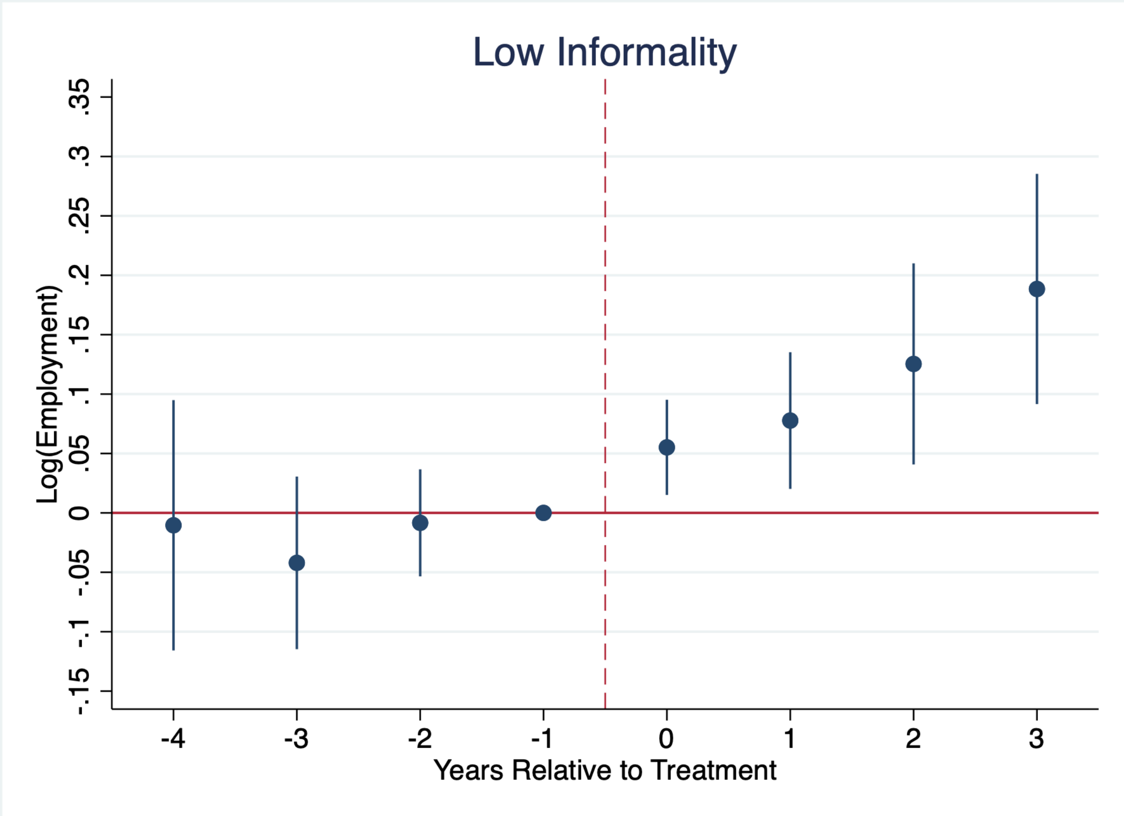
As 45% of the Brazilian labor market is shadowed in the informal economy (PNAD, 2012)[30], one might be concerned that the employment result is mechanically driven by formalization of existing workers, rather than an additional rise in employment caused by the reform. Next, I provide evidence that this is not the case. I take advantage of the fact that two years prior to the payroll tax reform, the Brazilian Census Bureau implemented a national Census survey with rich regional informality data. There are 5,300 municipalities in Brazil which are distributed in a wide range of informality, see figure 24. At the lower end, municipalities present a formalization rate lower than 20% which are rates observed in developing countries. At the upper tail, there are regions with more than 80% formalization rate which are standards of developed economies.

I split regions in two groups according to the position in the pre-reform median of the formalization rate. I leverage the analysis of labor market implications of the tax reform in both groups of regions. If the main employment response to the tax cut (figure 2) was driven by the mere formalization of informal workers, we should expect to see larger employment effects on high informality regions. I find precisely the opposite, i.e., low informality regions are the ones driving the employment effect, which is suggestive that the results are driven by additional employment rather than formalization. Figure 7 and 8 plot the event studies for both groups of regions. Since previous result suggested that small firms are driving

[30] Pesquisa Nacional por Amostra de Domicílios (PNAD) is a household survey administered by the Brazilian Census Bureau (IBGE).

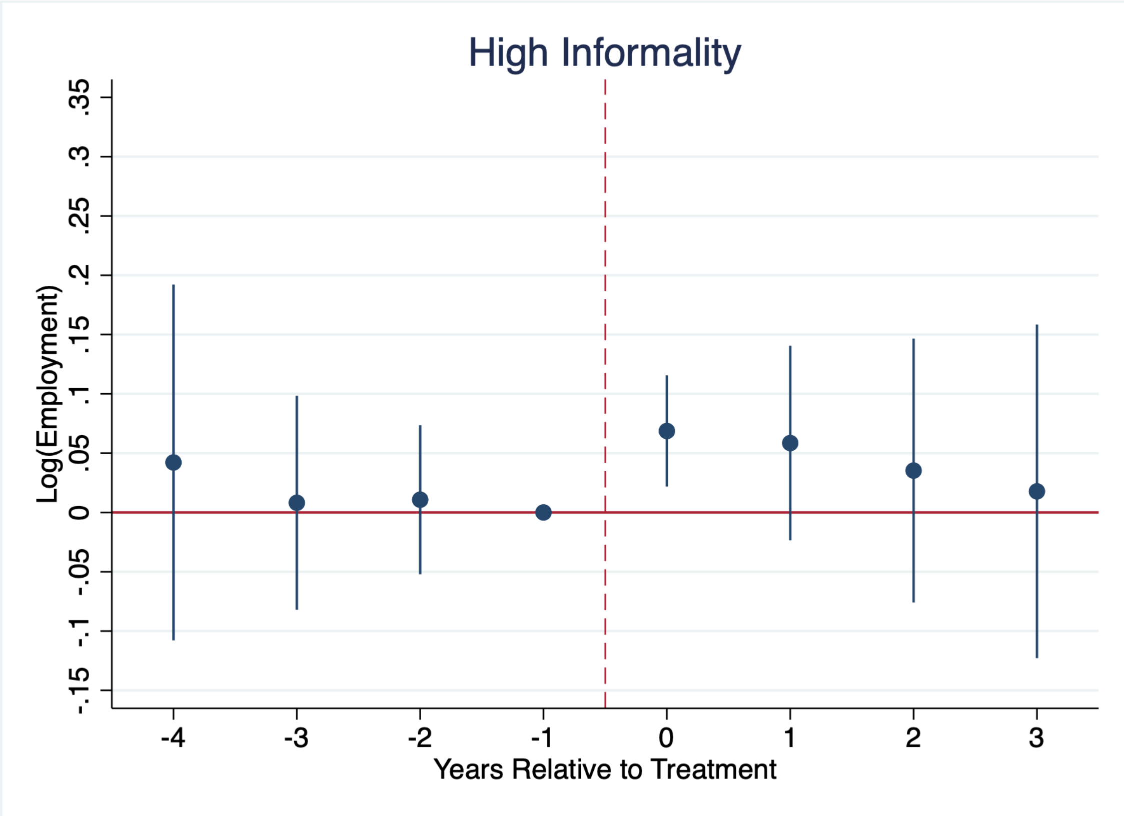
the employment effect (figure 3), I also want to show that the firm size distribution is somewhat evenly distributed across pre-reform firm size, see figure 25.

Figure 7: Firm Level: Employment Effect



Note: This figure plots the event study presented in equations 2 and 1 estimated for firms in informality municipalities. Standard errors are clustered at the 5-digit sector level.

Figure 8: Firm Level: Employment Effect



Note: This figure plots the event study presented in equations 2 and 1 estimated for firms in high informality municipalities. Standard errors are clustered at the 5-digit sector level.

4.2 Unions

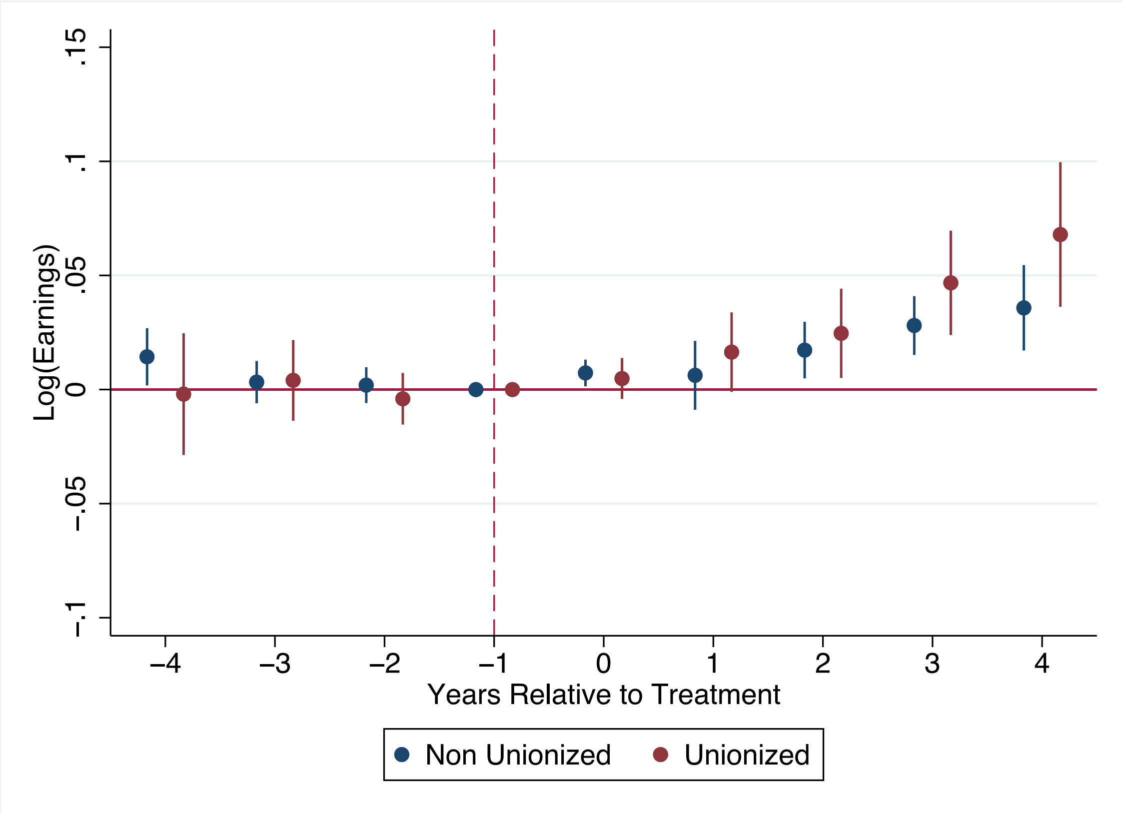
Another institutional setting prevalent in developing countries are the labor unions. The analysis of labor market implications of the tax reform for unionized versus non-unionized workers is helpful to shed light on the underlying mechanisms to the empirical findings. One mechanism of interest is that the tax cut is split between employers and employees according to a bargain process. The literature lacks evidence of the effects of tax policy interacted with unionization. I find suggestive evidence that tax cuts to unionized firms lead to more pass-through to earnings due to increase in employees' bargain on the wage setting process.

To obtain data on the worker unionization status, I rely on administrative data from the Minister of Labor (MTE) on the universe of union contracts signed in Brazil prior to the reform (2008-2011)[31]. I assign workers to the unionized group based on the modal pre-reform unionization status, which is flagged by a dummy variable

[31] This dataset is detailed in section 2.2

that informs each year if the employer has an active contract signed with a labor union. I fit the models outlined in equations 7, 8, 9 and 10 to the sample of unionized and non-unionized workers. I find suggestive evidence that unionized workers face larger pass-through due to the corporate tax cut. Even though the estimates are not statistically different between groups, table 1 shows that the earnings effect for non-unionized workers are equal to 1% (indistinguishable from zero), and unionized workers experience a statistically positive point estimate more than two times larger (2.2%) and statistically significant at 5%. In terms of dynamics, figure 9 shows that the pass-through gap between unionized and non-unionized groups grows over time.

Figure 9: Worker Level: Earnings Effect Across Unionization Status



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 four prior to entering the payroll tax cut program and three years after. Employees are classified into unionization categories based on the modal pre-reform unionization status. The blue markers plot the local average treatment effect for non-unionized workers, while the red markers are the effects for unionized workers. Standard errors are clustered at the 5-digit sector level.

Table 1: Earnings Effect per Unionization Status

	Non- Unionized (worker level)	Unionized (worker level)
Currently Treated	0.0107	0.0223*
	(0.00643)	(0.0108)
Observations	80,476,582	31,280,068
Firm FE	Yes	Yes
Sector x Year FE	Yes	Yes
Worker FE	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table presents the results for unionized and non-unionized workers. Unionization is determined based on the modal pre-reform unionization status. The worker unionization dummy is defined based on the modal pre reform unionization status. The table shows that the pass-through to earnings is larger in unionized compared to non-unionized workers. The local average treatment effect is not distinguishable from zero for the non-unionized workers, however it is significant at a 5% level for unionized workers. Standard errors are clustered at the 5-digit sector level.

5 Discussion of Models

In progress

6 Unintended Consequences: Tax Evasion

Up to this point, it has been shown how the Government can affect employment and earnings through corporate tax reforms. Beyond the employer-employee relationship, the tax reform also affects the tension between employers and the Government itself. In the case of the Brazilian corporate tax reform, as the payroll taxes were waived there was an implementation of a small revenue tax[32], which created incentives for revenue under reporting. The tax evasion response is an unintended consequence of the reform, but is consistent with findings in previous literature that studied perturbations on the revenue tax schedule (Lobel, Scot, and Zúniga 2020; Bachas and Soto 2021; Londoño-Vélez and Ávila-Mahecha 2019).

To evaluate the revenue impact of the reform, I rely on firm level revenue data[33]

[32] Approximately 1.5% of the gross revenue.

[33] Due to challenges on accessing revenue data on the universe of firms, the data is limited to the balanced panel of firms in the period 2008-2017. The data is structured in the firm x year level, as the main sample. The effects on labor market outcomes don't change when restricted to the sample with revenue information.

detailed on table 9. I use this data to estimate the event study presented in equations 1 and 2. Even though this same specification presents an average employment expansion, figure 26 shows a revenue decrease of 5% (table 2). I interpret this finding as suggestive evidence of firms engaging in tax evasion strategies and revenue under-reporting.

Tax evasion models consider the firms' choice of revenue under reporting as the result of an optimal decision that depends on the cost of mis-reporting. Tipically this cost is convex in the distance of the true and the reported revenues (Lobel, Scot, and Zúniga 2020). Under the lenses of these models, firms that have never under reported revenues have lower marginal cost of evading. The Brazilian two tier tax schedule[34] provides variation on firms exposure to revenue tax avoidance that allows me to test the prediction of these models. Firms taxed on a revenue based tax tier have presumably explored all the revenue mis-reporting opportunities, as they choose evasion optimally.[35] I test the tax evasion predictions, fitting the difference-in-differences model in equations 9 and 10 to analyze the revenue impact on firms taxed based on a profit based tier versus revenue based tax tier[36].

Table 2 reports that the negative revenue response is driven by revenue based firms, which is the exact prediction of the model. Column (1) shows that the average revenue effect is -5%. Column (2) reports a sharp zero revenue response to firms in the revenue based tax tier. Finally, column (3) reports a -8% revenue response for firms taxed under the profit based tax tiers. This suggests that when the new revenue tax is included to every treated firm (regardless of their tax tiers), the firms in the profit based tax tiers drive the average evasion response. To further analyze the underlying evasion incentives, I break down the analysis per tax tier and firm size.

A few things are worth noting in the results presented in Figure 27. First, there is no evidence of evasion responses on small and medium firms. Among those firms, the revenue effects are indistinguishable across tax tiers. Second, within large firms, there is statistical difference in the revenue effect across tax tiers. This is suggestive that large firms in the profit based tax tier are driving the tax evasion findings. These firms are large not only in employment (more than 50 workers at the baseline), but also in revenue (above $3MM USD annual revenue to qualify for the profit based tier).

[34]The regular Brazilian tax schedule offers two tiers that are either profit or revenue based. Eligibility to these tiers depend on a revenue threshold (currently at $14MM BRL, or $3MM USD per year).

[35]For example, it is harder to under-report revenues that contain a third party report.

[36]For the purposes of splitting firms into tax tiers, I fixed each firm to the tier that they were in the pre-reform year.

Table 2: Firm Level: Revenue Effect per Tax Tier

	Revenue (all tiers)	Revenue (rev based (< 14MM))	Revenue (profit based (> 14MM))
Currently Treated	-0.0495**	-0.00157	-0.0791***
	(0.0165)	(0.0274)	(0.0213)
Observations	823,775	550,791	272,980
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 difference-in-differences coefficients estimated using equations 9 and 10 on the balanced panel of firms. The outcomes of interest are log of revenues evaluated at each tax tier category. The regular Brazilian tax schedule offers two tiers that are either profit or revenue based. Eligibility to these tiers depend on a revenue threshold (currently at \$14MM BRL, or \$3MM USD per year).

7 Conclusion

There is a long standing question in the literature about the labor market implications of a payroll tax reform. The community of scholars and policy makers have faced numerous challenges to answer this question. First, the estimates of payroll tax policies targeted at specific workers can be confounded by pay equity norms. Second, estimates from tax reforms implemented across the board (i.e., to the universe of workers), are vulnerable to lurking variables due to other macro shocks. Finally, most of the payroll tax reforms in the past were temporary policies, which doesn't allow the researcher to disentangle short and long term effects.

I exploit a Brazilian payroll tax reform targeted at the sector and product levels. The policy has been in place for more than ten years and allows me to overcome all the challenges faced by previous work. Difference-in-differences estimates instrumented by eligibility find a large employment effect at the firm level. The employment effects are driven by small firms, but not rationalized by formalization of existing workers. The pass-through to earnings is small, with substantial timing variation. It is zero in the short run, and gradually increases up to a 4% effect three years after the tax cut.

Merging employer-employee data with firm level tax data and the universe of collective bargaining agreements (CBAs) in Brazil, I provide suggestive evidence that the pass-through to earnings is augmented for unionized workers. Even though the pass-through to unionized and non-unionized workers is statistically significant, unionized workers experience a significant effect of 2.2%, whereas the pass-through to non-unionized workers is only 1% and not significant. I leverage the exogenous variation on labor cost to estimate a labor demand with respect to wages of -0.54, and labor supply of 6.67. In a context of monopsony in the labor market, this elasticity implies that firms benefit from market power to mark down wages by 15%.

8 Appendix

8.1 Figures

Figure 10: Tax Forms Information

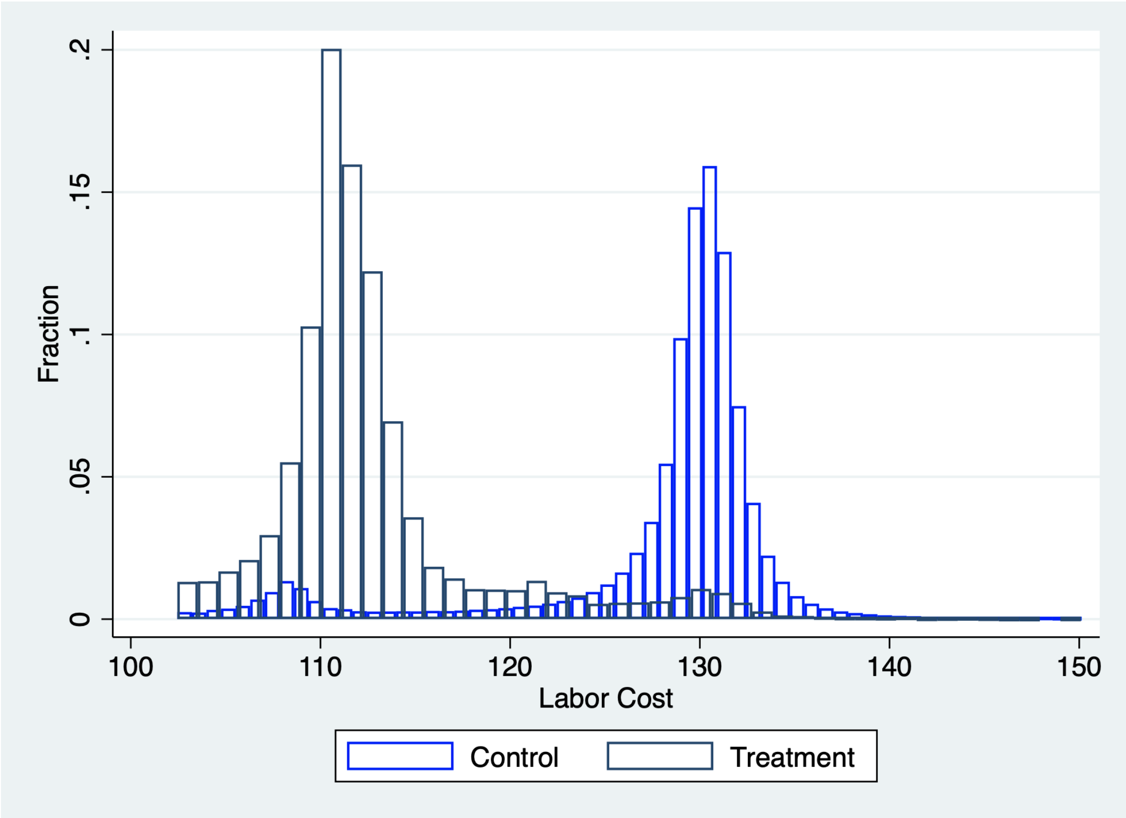
Nº	Campo	Descrição	Tipo	Tam	Dec	Obrig
01	REG	Texto fixo contendo "0145".	C	004*	-	S
02	COD_INC_TRIB	Código indicador da incidência tributária no período: 1 – Contribuição Previdenciária apurada no período, exclusivamente com base na Receita Bruta; 2 – Contribuição Previdenciária apurada no período, com 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

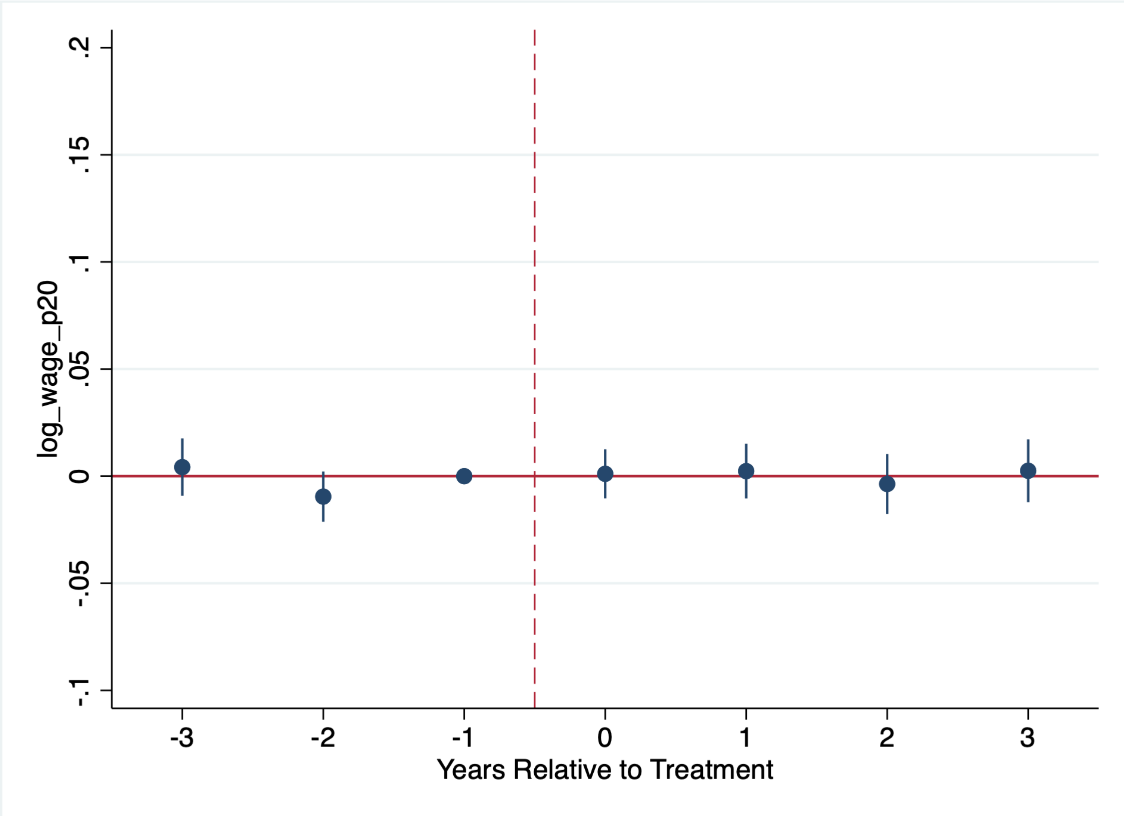
Note: This figure shows instructions for eligible firms to request the payroll tax benefit. It describes detailed information to be provided in Tax Administration software, in order to substitute part of the payroll tax by revenue taxes.

Figure 11: Histogram on Treatment Intensity



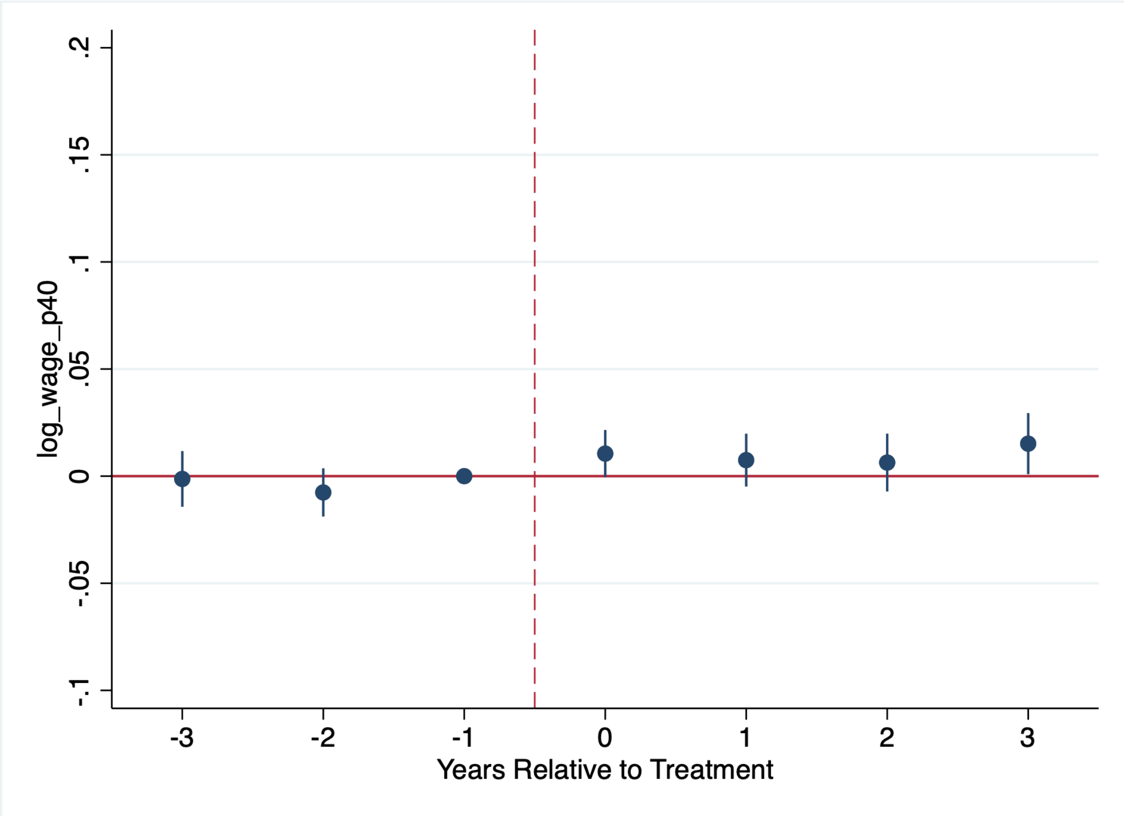
Note: This histogram compares the distribution of labor cost (defined as wages plus payroll tax) between treated and control firms. The average labor cost during treatment is 112%, whereas 131% out of treatment. The distribution for the two groups are centered on the average, but present dispersion.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

Figure 12: Wages (Percentile 20): Event Study Estimates



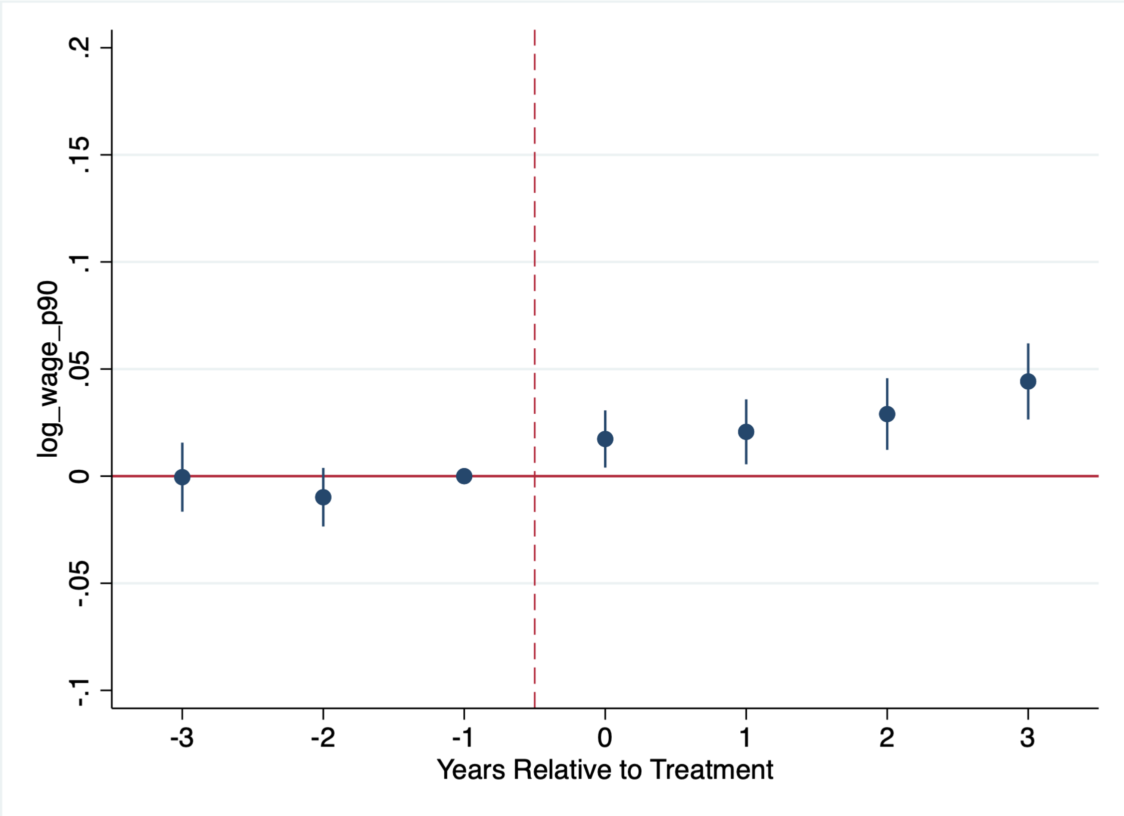
Note: This figure presents the event study estimates for wages at the percentile 20 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans three years prior to entering the payroll tax cut program and three years after. Standard errors are clustered at the 5-digit sector level.

Figure 13: Wages (Percentile 40): Event Study Estimates



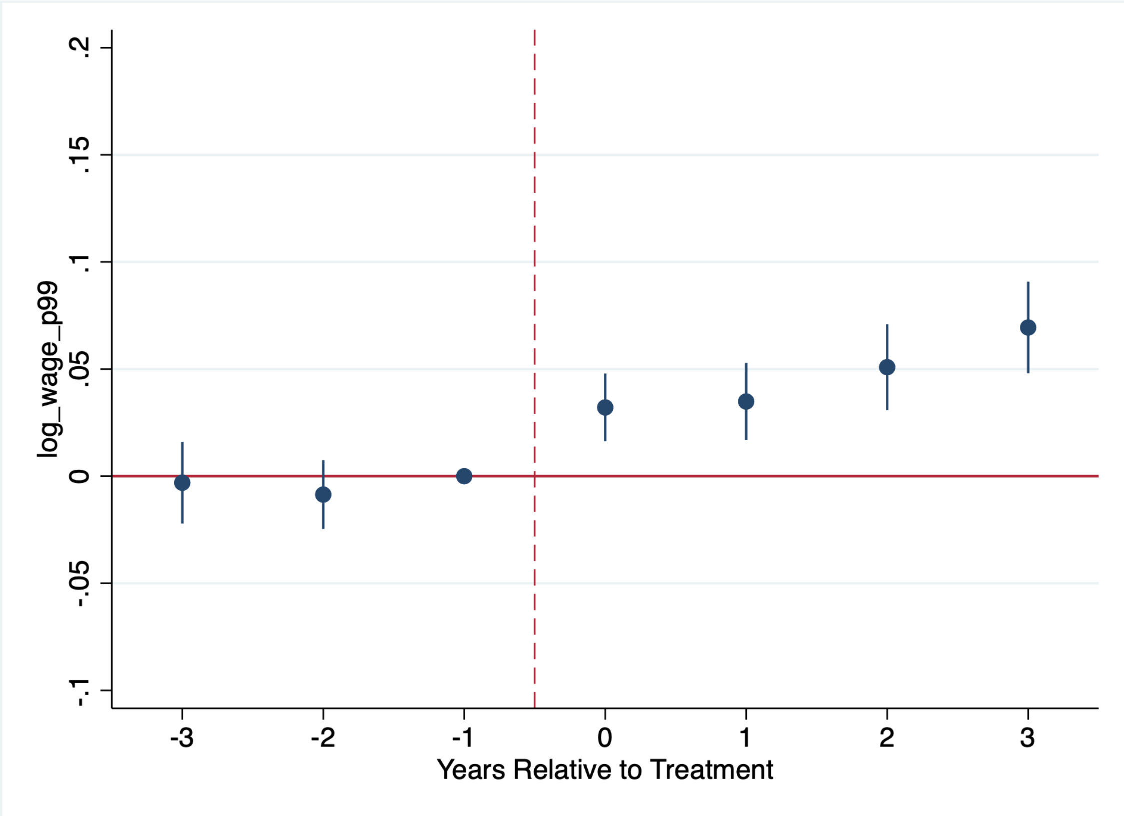
Note: This figure presents the event study estimates for wages at the percentile 40 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans three years prior to entering the payroll tax cut program and three years after. Standard errors are clustered at the 5-digit sector level.

Figure 14: Wages (Percentile 90): Event Study Estimates



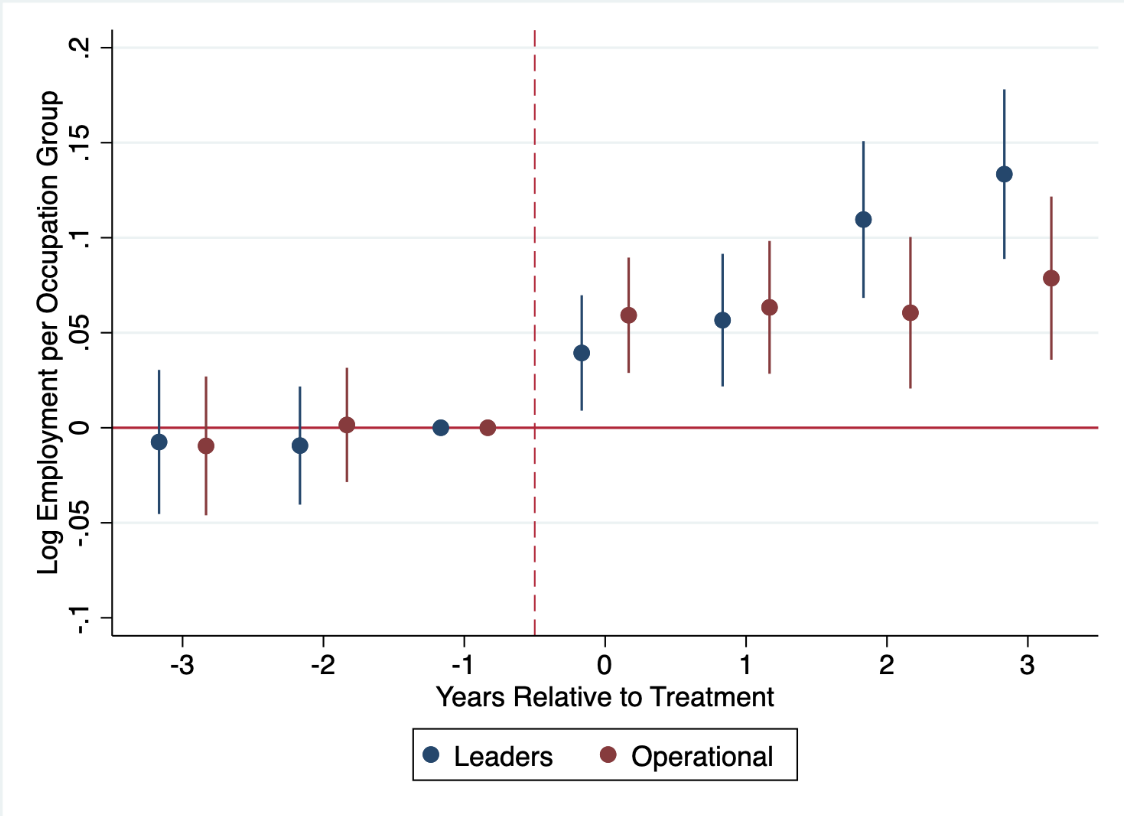
Note: This figure presents the event study estimates for wages at the percentile 90 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans three years prior to entering the payroll tax cut program and three years after. Standard errors are clustered at the 5-digit sector level.

Figure 15: Wages (Percentile 99): Event Study Estimates



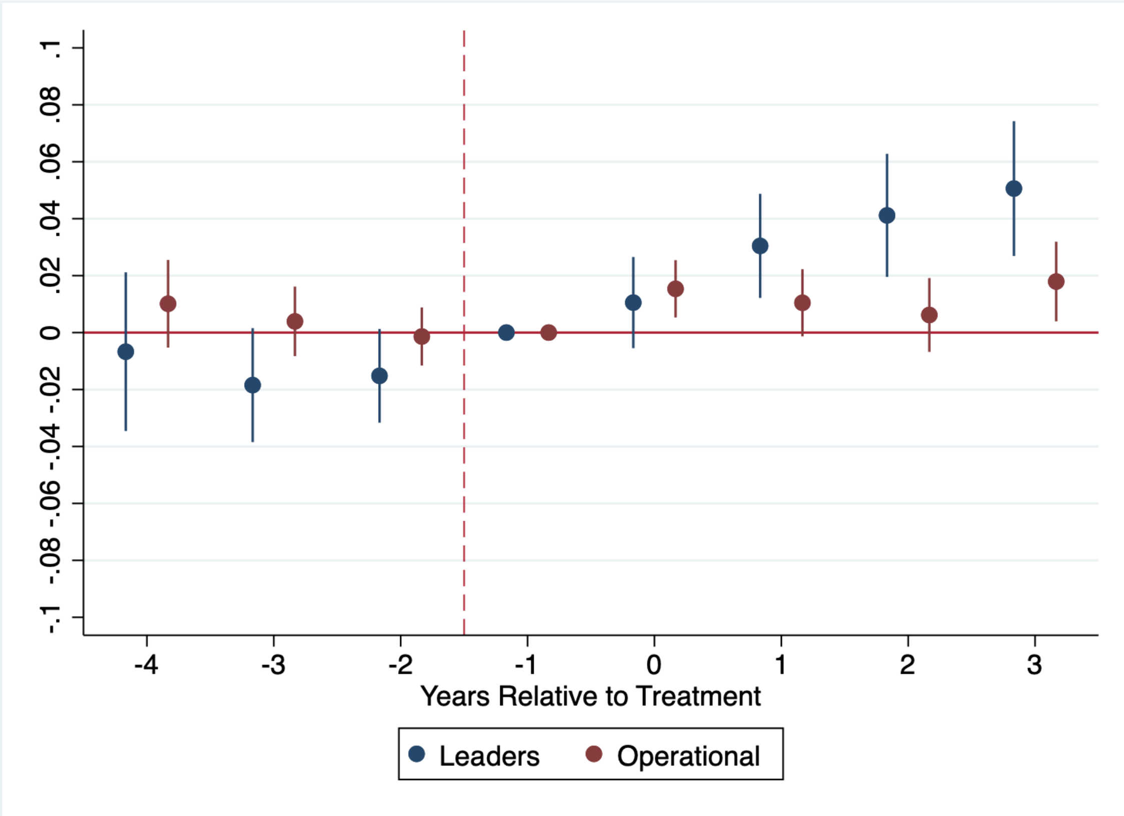
Note: This figure presents the event study estimates for wages at the percentile 99 of the within firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize the results with respect to one year prior to the event. The analysis spans three years prior to entering the payroll tax cut program and three years after. Standard errors are clustered at the 5-digit sector level.

Figure 16: Firm Level: Employment per Occupation



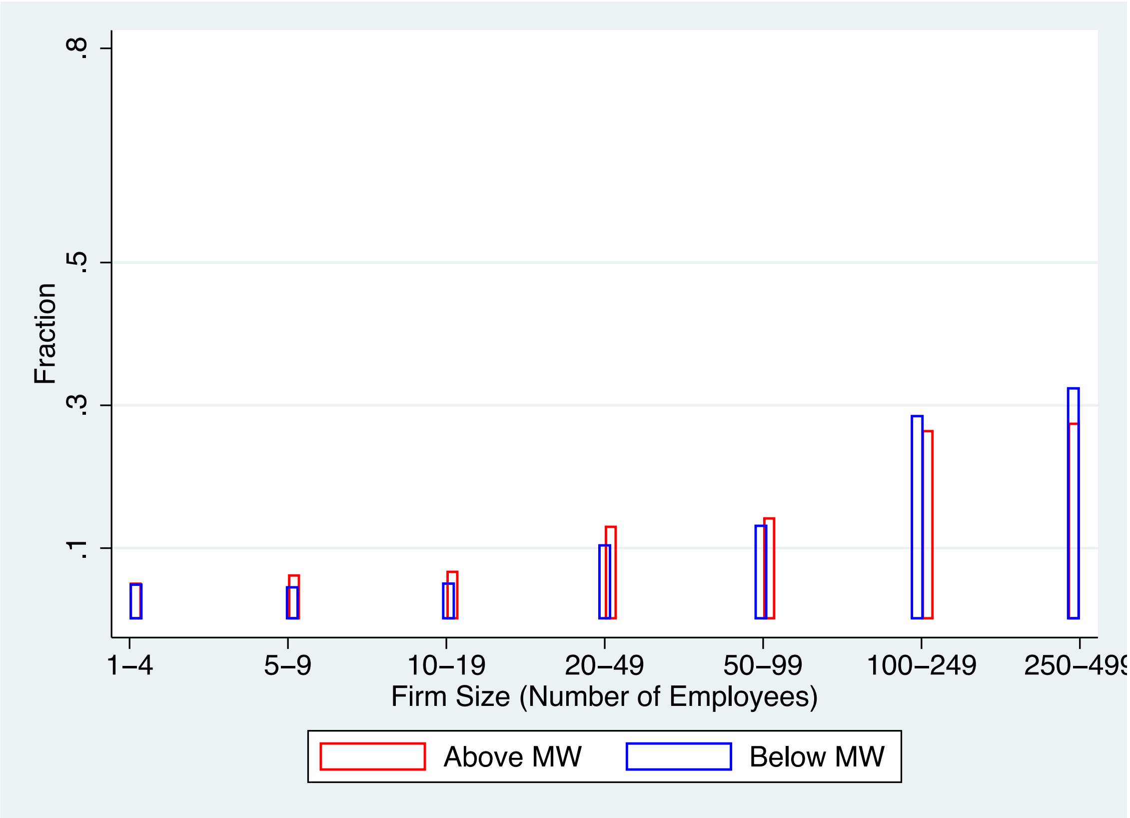
Note: This figure presents the event study estimates for the log of employment per occupation group. Leaders are directors, managers and qualified technical positions according to the CBO classification. The average employment effect are similar, however the employment response for leaders occurs more gradually. Standard errors are clustered at the 5-digit sector level.

Figure 17: Firm Level: Earnings per Occupation



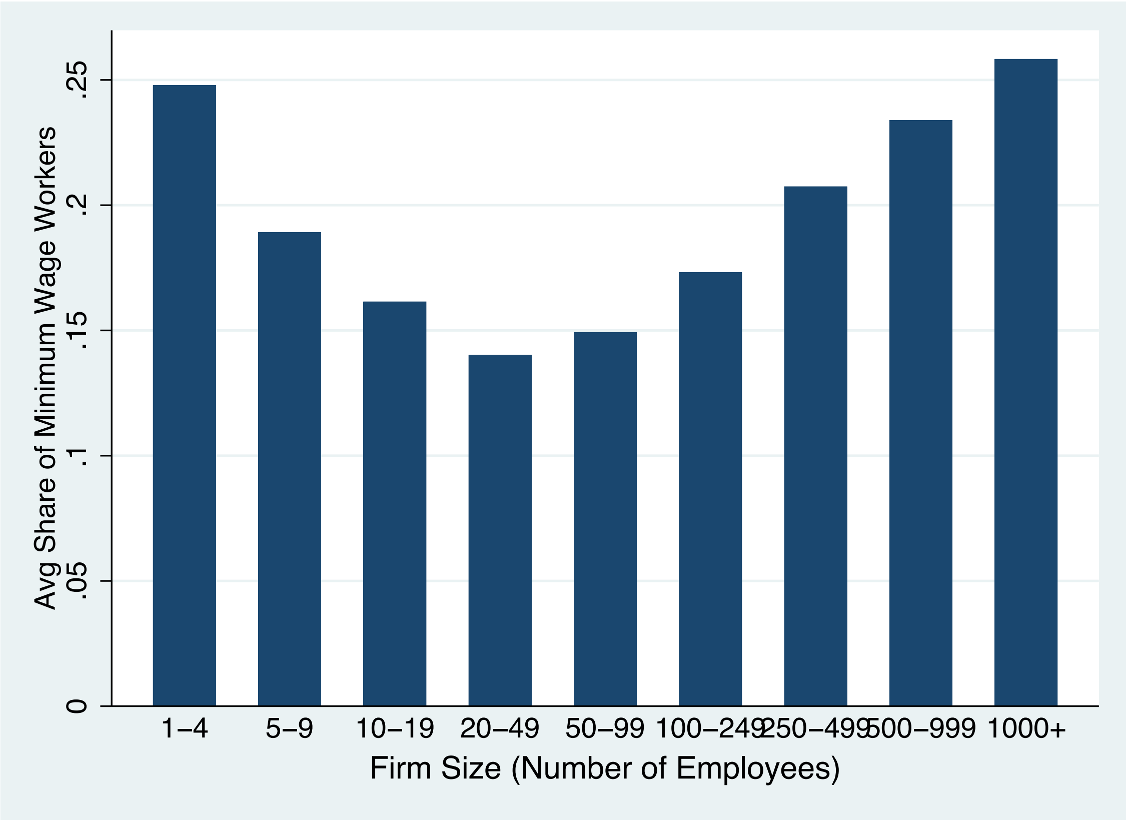
Note: This figure presents the event study estimates for the log of earnings per occupation group. Leaders are directors, managers and qualified technical positions according to the CBO classification. While leaders experience gradual pass-through to earnings due to the reform, operational workers don't observe significant earnings increase. Standard errors are clustered at the 5-digit sector level.

Figure 18: Distribution of Minimum Wage Workers Across Firm Size



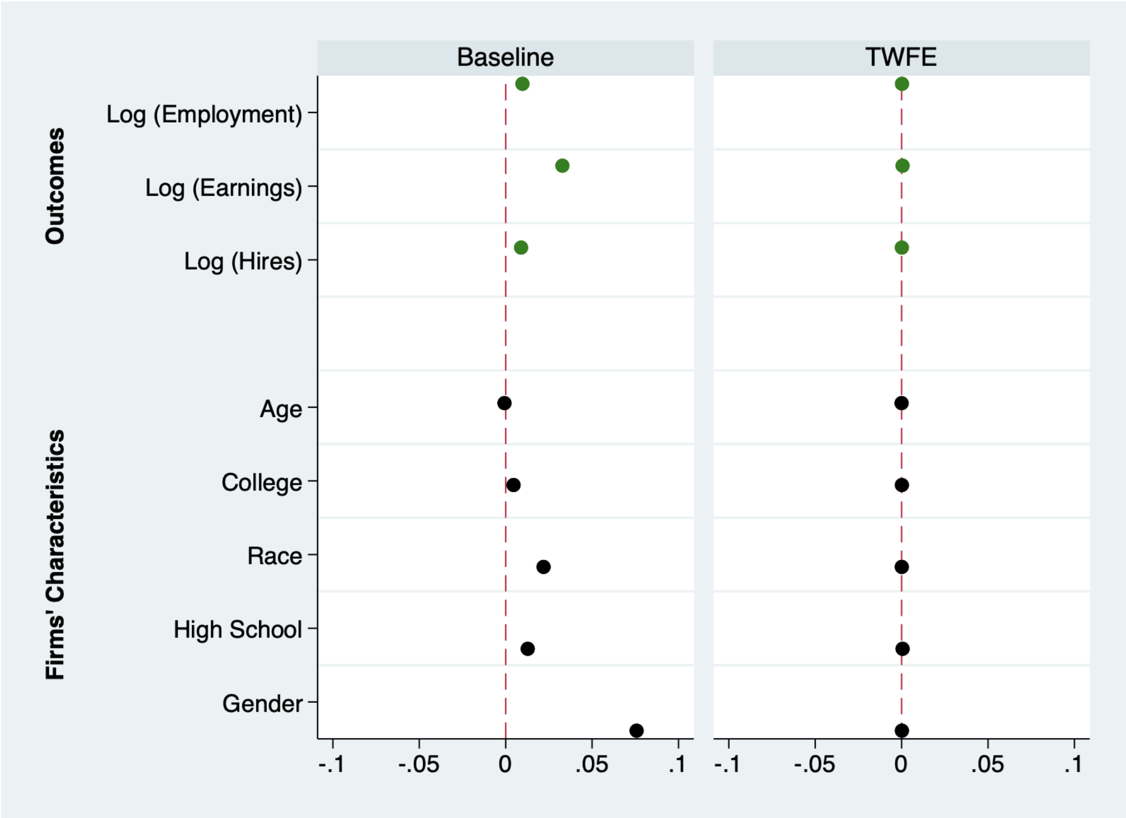
Note: This figure presents the distribution of minimum and non-minimum wage workers across the firm size distribution. Workers are categorized based on the modal pre-reform minimum wage status. Firms are categorized in size buckets according to their pre-reform size. The red bars plot the fraction of non-minimum wage workers in each size bin relative to the total of non-minimum wage workers. The blue bars are analogous to minimum wage workers. Standard errors are clustered at the 5-digit sector level.

Figure 19: Wages: Event Study Estimates



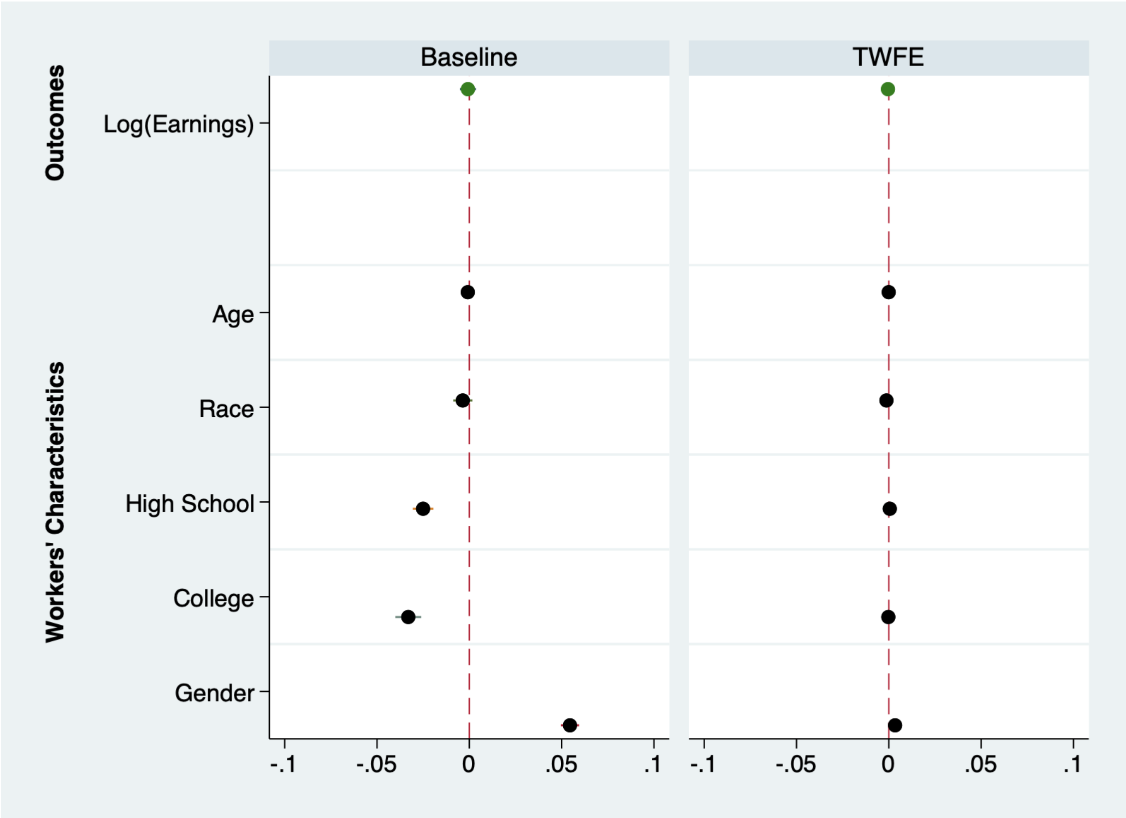
Note: This figure presents the share of minimum wage workers per firm relative to the total firm employment, in the pre-reform period. Workers are categorized based on the modal pre-reform minimum wage status. Standard errors are clustered at the 5-digit sector level.

Figure 20: Balance Test: Firm Level



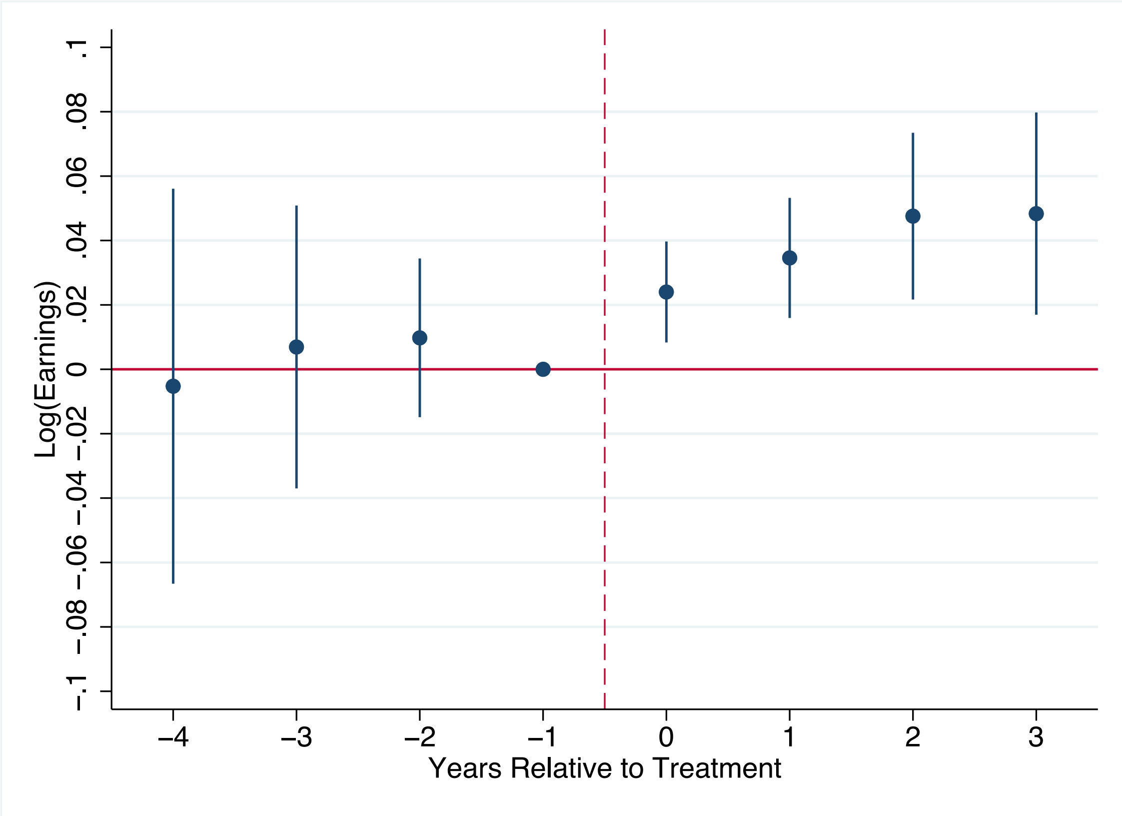
Note: This figure presents the coefficients for the $L_{s(j)t} = X_{jt} + u_{jt}$, and the TWFE model: $L_{s(j)t} = X_{jt} + \alpha_j + \gamma_t + \xi_{s1(j),t} + u_{jt}$, where $Ls(j)t$ is a dummy to indicate if the firm is eligible in year t; X_{jt} are characteristics of the firm; and the fixed effects are the same used in all empirical specifications presented before. These models are fitted to the firm level sample, where each observation is a firm x year. The confidence intervals are plotted, but they are negligible in this dataset, which is close to the universe of firms in Brazil over the ten year period of analysis.

Figure 21: Balance Test: Worker Level



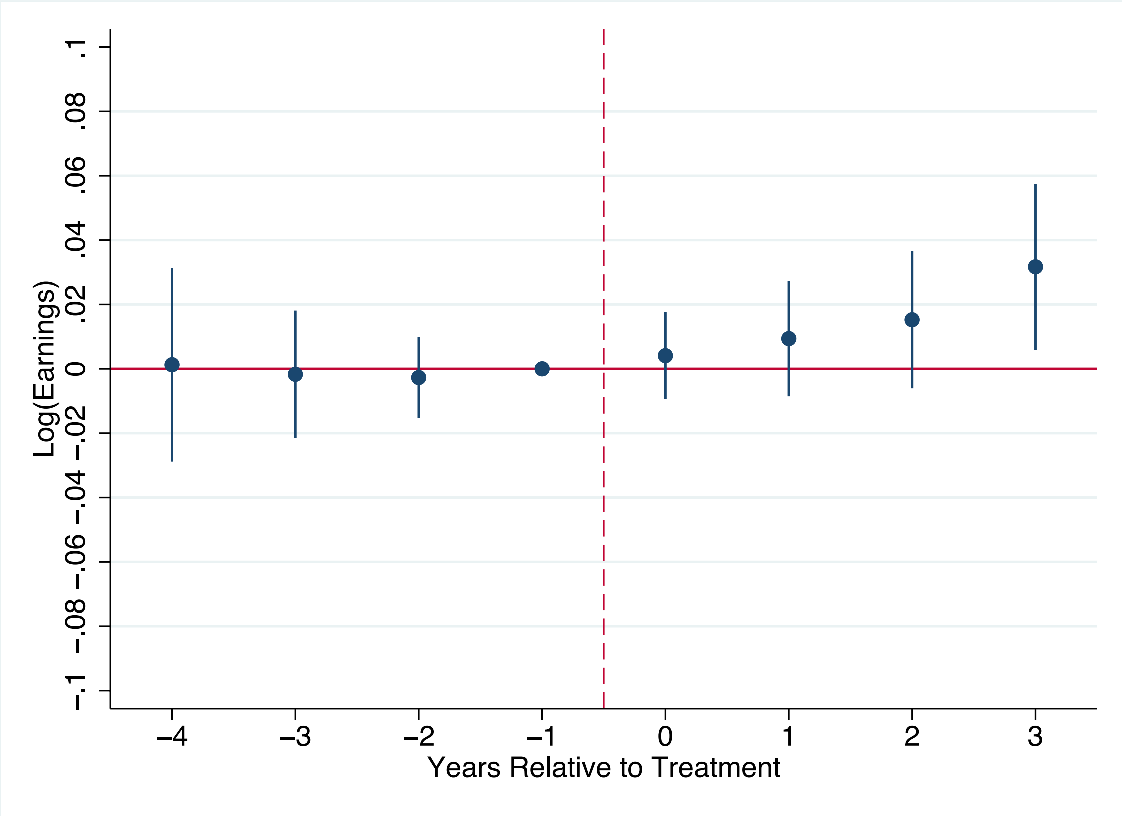
Note: This figure presents the coefficients for the $L_{s(j)t} = X_{jt} + u_{jt}$, and the TWFE model: $L_{s(j)t} = X_{jt} + \alpha_j + \gamma_t + \xi_{s1(j),t} + u_{jt}$, where $Ls(j)t$ is a dummy to indicate if the firm is eligible in year t; X_{jt} are characteristics of the worker; and the fixed effects are the same used in all empirical specifications presented before. These models are fitted to the worker level sample, where each observation is a worker x year. The confidence intervals are plotted, but they are negligible in this dataset, which is close to the universe of firms in Brazil over the ten year period of analysis.

Figure 22: Wages: Event Study Estimates



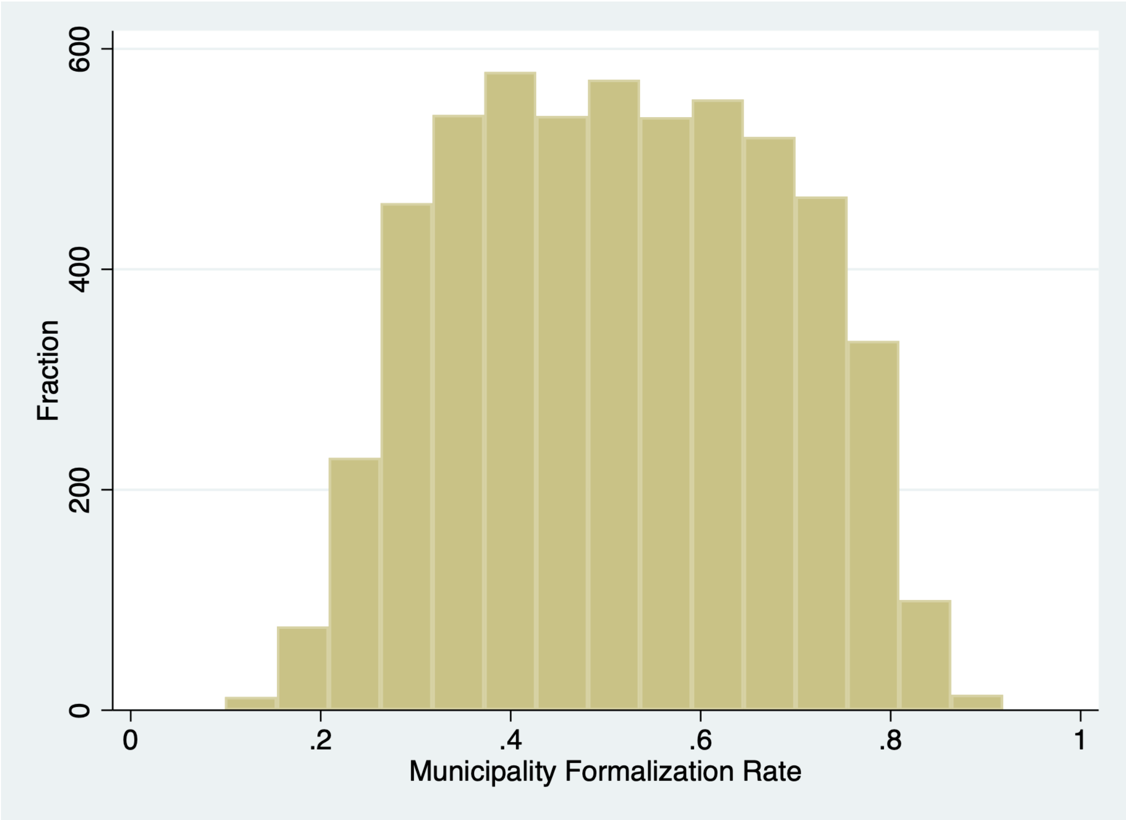
Note: This figure presents the event study estimates for average earnings effect for workers that were employed for at least three years in the same firm during the pre-reform period. I normalize the results with respect to one year prior to the treatment event. The analysis spans four prior to entering the payroll tax cut program and three years after. The dashed horizontal line in the upper right part of the figure reports the local average treatment effect on compliers of 1.8% estimated based on equations 3 and 4. Standard errors are clustered at the 5-digit sector level.

Figure 23: Wages: Event Study Estimates



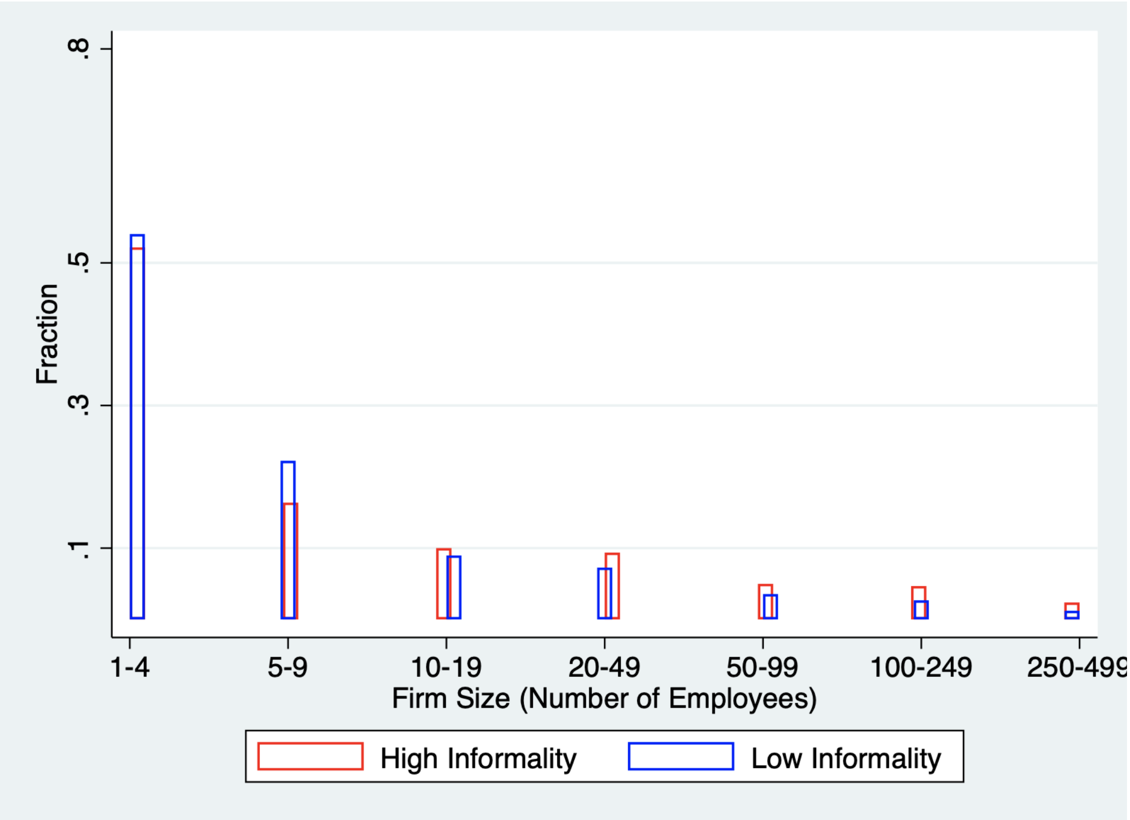
Note: This figure presents the event study estimates for average earnings effect for workers that were employed for at least three years in the same firm during the pre-reform period. I normalize the results with respect to one year prior to the treatment event. The analysis spans four prior to entering the payroll tax cut program and three years after. The dashed horizontal line in the upper right part of the figure reports the local average treatment effect on compliers of 1.8% estimated based on equations 3 and 4. Standard errors are clustered at the 5-digit sector level.

Figure 24: Formalization Rates per Municipality



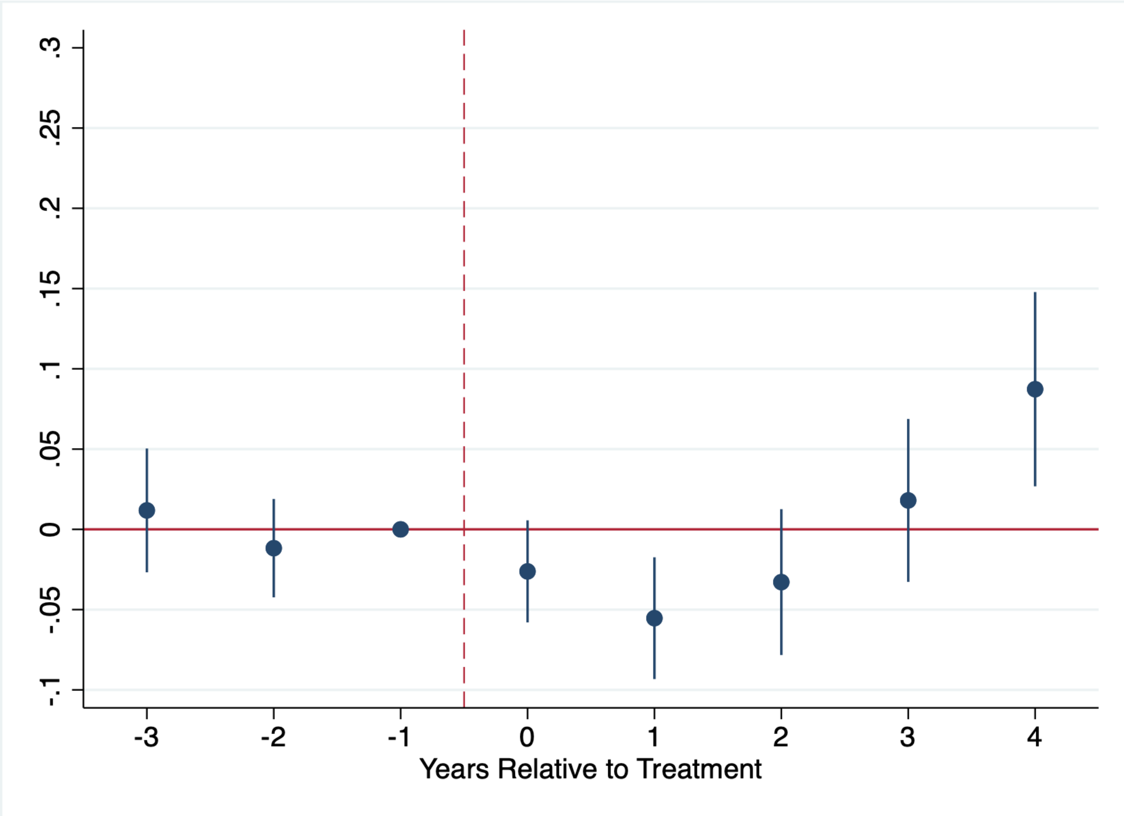
Note: This figure presents the distribution of formalization rates per municipalities in Brazil, according to the 2010 Census. There are 5,300 municipalities with heterogeneous informality rates.

Figure 25: Firm Size Distribution Across Informality Groups



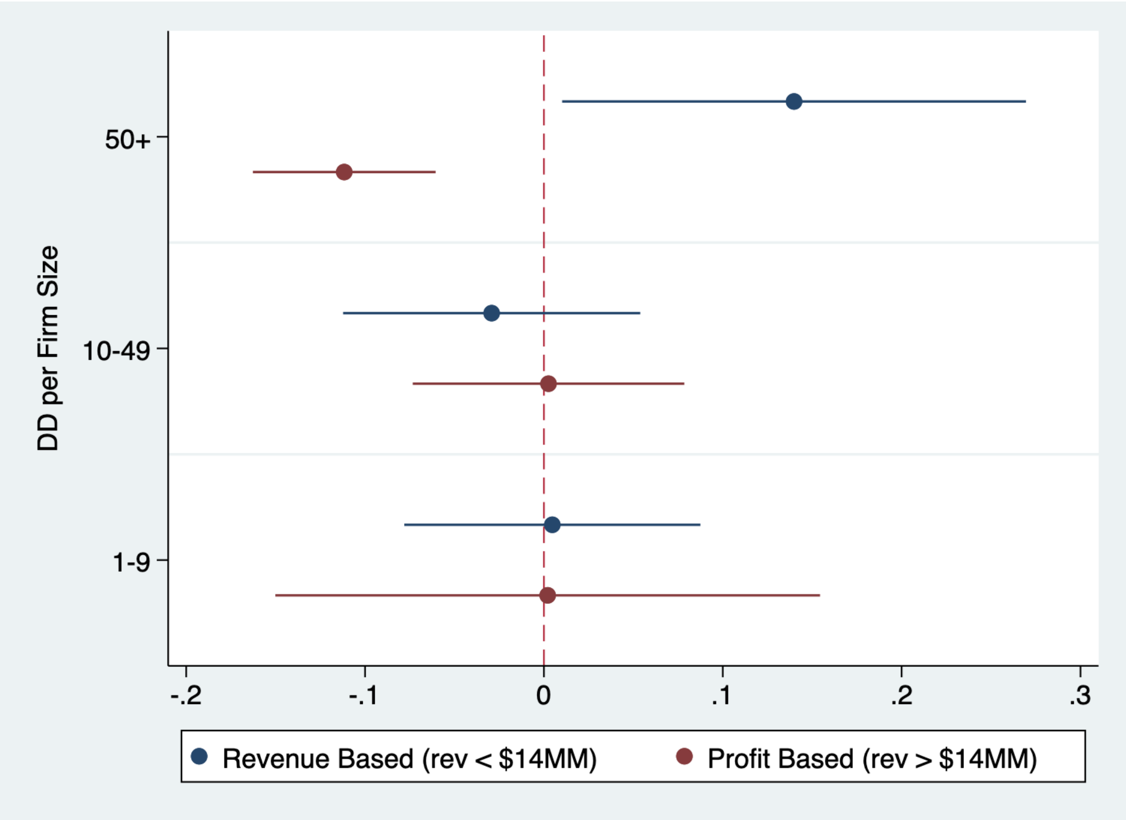
Note: This figure plots the histogram of firm size distribution for firms in low versus high informality areas. The firm size and the low formalization rates are computed in pre-reform years. The firm size category of 5-9 employees is slightly larger in the low informality areas. Other than that the distributions are similar to each other.

Figure 26: Firm Level: Revenue Effect



Note: This figure presents the event study estimates for log of gross revenues. I normalize the results with respect to one year prior to the treatment event. The analysis spans three years prior to entering the payroll tax cut program and four years after. Revenue is reported to the tax authority once a year. The estimates in the plot come from equations 1 and 2 fitted to the sample of balanced firms. Standard errors are clustered at the 5-digit sector level.

Figure 27: Firm Level: Revenue Effect per Firm Size and Tax Tier



Note: This figure presents the difference-in-differences coefficients estimated using equations 9 and 10 on the balanced panel of firms. The outcomes of interest are log of revenues evaluated at each tax tier category and firm size bins. The size categories are determined pre-reform. The regular Brazilian tax schedule offers two tiers that are either profit or revenue based. Eligibility to these tiers depend on a revenue threshold (currently at $14MM BRL, or $3MM USD per year). The blue markers present the coefficient for revenue based firms, and the red markers present the coefficients for firms under the profit based regime. Standard errors are clustered at the 5-digit sector level.

8.2 Tables

Table 3: Eligible vs Non-Eligible Sectors

Eligible	Not Eligible
Hotels	Motels
Open television	Cable television
Public bus transportation	School bus and taxi
Eletronic games manufacturing	Toys and other recreative games manufacturing
Internet portals and content providers	News agencies
Trains	Touristic trains
Newspaper, magazine and book printing	Other periodic printing
Maintenance aircraft and vessels	Maintenance aircraft and other transportation modes

Note: This table presents a list of sectors that are displayed in the tax bills as eligible to the payroll tax cut, and compares it with another list of similar sectors that are not included in the tax reform. This is an anedoctal evidence that the Governement was not anticipating sector trends when determining eligibility.

Table 4: Descriptive Statistics: Firm Level Sample

	(1) Non-Eligible (pre)	(2) Eligible (pre)	(3) Avg (pre)
Descriptive Statistics			
Employment	53.46	55.87	53.60
	(1,019.68)	(330.08)	(991.85)
Earnings	1,055.61	1,317.61	1,070.63
	(1,121.57)	(1,255.82)	(1,131.33)
Hiring	24.97	27.44	25.12
	(366.91)	(169.26)	(358.12)
Tax Rate	28.46	28.99	28.49
	(8.70)	(9.11)	(8.72)
Gender	0.55	0.77	0.56
	(0.40)	(0.29)	(0.40)
Employees Age	37.20	35.91	37.12
	(8.91)	(7.69)	(8.85)
Firm Age	23.16	20.37	22.99
	(10.46)	(10.10)	(10.46)
High School +	0.53	0.59	0.53
	(0.41)	(0.37)	(0.41)
Share White	0.68	0.74	0.69
	(0.37)	(0.32)	(0.36)
N	1,747,045	111,790	1,858,835

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 5: Descriptive Statistics: Worker Level Sample

	(1) Non-Eligible (pre)	(2) Eligible (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 pre-reform period (2008 to 2011). This sample is a 1% random draw from the universe of firms. 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 6: Pooled Difference-in-Differences

	(1) Log (Employment) Firm Level	(2) Log (Earnings) Worker Level	(3) Avg Occup Pctile Firm Level	(4) Log (Employment) Firm Level	(5) Log (Earnings) Worker Level
Currently Treated	0.0888	0.0179	0.00000427		
	(0.0468)	(0.0111)	(0.0000324)		
Eligible x Post				0.0421	0.0103
				(0.0225)	(0.00597)
Constant				1.189***	6.242***
				(0.106)	(0.0842)
Observations	4,208,016	111,641,068	4,205,793	4,208,016	111,641,068
Firm FE	Yes	Yes	Yes	Yes	Yes
Sector (1 digit) x Year FE	Yes	Yes	Yes	Yes	Yes
Worker_FE	No	Yes	No	No	Yes

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

Note: This table presents IV and reduced form estimates. Due to imperfect compliance, there are less treated firms within the eligible group, which explains the smaller effects in the reduced form. The first column and third columns are at the firm level, while the second and fourth are at the worker level. The instrument is the interaction between two indicators: one that flags sector eligibility and the other that indicates if the time is post eligibility. Standard errors are clustered at the 5-digit sector level.

Table 7: 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	0.0184***	0.0486***	0.0289***	0.0136**	0.00481
	(0.00477)	(0.00765)	(0.00628)	(0.00496)	(0.00498)
Observations	4,211,566	4,211,566	4,211,566	4,211,566	4,211,566
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. Standard errors are clustered at the 5-digit sector level.

Table 8: First Stage: Cost of Labor (1 + payroll tax rates)

	(1) Log(Labor Cost) (1 + Payroll Tax)	(2) Log(Labor Cost) (1 + Payroll Tax)
Currently Treated	-0.143***	
	(0.00123)	
Post x Eligible		-0.0850***
		(0.00101)
Age	-0.000503***	-0.000607***
	(0.0000528)	(0.0000550)
Gender Composition	-0.00216*	-0.00447***
	(0.000898)	(0.000927)
High School	-0.000979	-0.00145*
	(0.000652)	(0.000684)
College	-0.00456***	-0.00616***
	(0.00111)	(0.00116)
White	-0.00524***	-0.00731***
	(0.000821)	(0.000880)
Observations	2,252,356	2,252,356
IV	Yes	Reduced
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 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 payroll tax changes in eligible firms due to the reform. Standard errors are clustered at the 5-digit sector level.

Table 9: Descriptive Information on the Gross Revenue Data

Year	Balanced Sample	Revenue Data	Tax Tier (rev based)	Tax Tier (profit based)
2008	252,103	86,760	58,630	28,130
2009	252,103	86,153	58,550	27,603
2010	252,103	85,840	58,332	27,508
2011	252,103	85,872	58,488	27,384
2012	252,103	85,322	58,200	27,122
2013	252,103	84,693	57,537	27,156
2014	252,103	81,854	56,001	25,853
2015	252,103	78,705	52,634	26,071
2016	252,103	77,995	51,147	26,848
2017	252,103	71,823	44,720	27,103

Note: This table presents descriptive statistics on the sample size of the firm level revenue information. The revenue data is obtained at a firm level, but only for firms that belong to the balance sample, i.e., exist from 2008 to 2017. Out of those, some of them are exempted by law from reporting revenues. Examples of exempted agents are associations, clubs, churches, condominiums. Firms that report revenue are divided among two tax tiers, which can be revenue or profit based.

9 Appendix

A Firm Entry and Exit

To evaluate whether the payroll tax cut generated responses on firms' entry and exit margin, I compare eligible versus non eligible sectors on the percentage variation of three outcomes: (i) number of firms; (ii) number of entrants; (iii) number of exits. I conduct this analysis on the sector level because the industry aggregates the number of firms in a measure that is consistent to the main eligibility criteria. I explore the eligibility variation in a standard difference-in-differences approach. To adjust for product eligible firms, I exclude from the sample non-eligible sectors that contain more than 20% of firms being eventually treated. In this context, an active firm in year t is the one that employs at least one worker in December, 31^{st} of year t. Entrant firms in year t are the active ones that were not active in t-1. Existing firms are active in t, but not in t+1. To reduce noise from less meaningful variation, I restrict the analysis to sectors that had at least two entrant and two exiting firms in each year. Regressions are weighted based on the pre-reform sector median firm size. The fitted model is given by,

$$\Delta Y_{jt} = \delta L_{s(j)t} + \xi_{s1(j),t} + X_j + u_{jt}$$

where $Y_{jt} = \frac{Y_{j,t}-Y_{j,t-1}}{0.5(Y_{j,t}+Y_{j,t-1})}$ is the percentage variation on the outcome of interest; $L_{s(j)t}$ is an indicator equal to one if year t is after sector j enters eligibility; $\xi_{s1(j),t}$ are broad sector times year fixed effect to absorb sector specific time trends; and X_j are controls such as sector average age, race, education, gender and its squares.

Table 10 summarizes the results. In summary, none of the estimates are statistically different from zero, based on standard levels of confidence. However, according to columns (1) and (2), the point estimates are slightly positive for the variation on the number of active and entrant firms. Column (3) presents negative point estimates on the existing margin. These non significant results suggest that we cannot reject the hypothesis that the tax reform had no effect on entrepreneurs' decisions of opening and shutting down businesses. This result is important to understand the impact of tax policy on entrepreneurship decisions, but also to clarify that the main results of the paper on employment and earnings were not driven by the firm entry response. The fact that the employment estimates on the balanced and unbalanced panels were similar, anticipated that there was not much action on the firm entry margin. Figures 28, 29 and 30 provide event study estimates that allow the reader to visually test the pre-trend assumption, and access the magnitude of the standard errors.

Table 10: Firm Entry and Exit

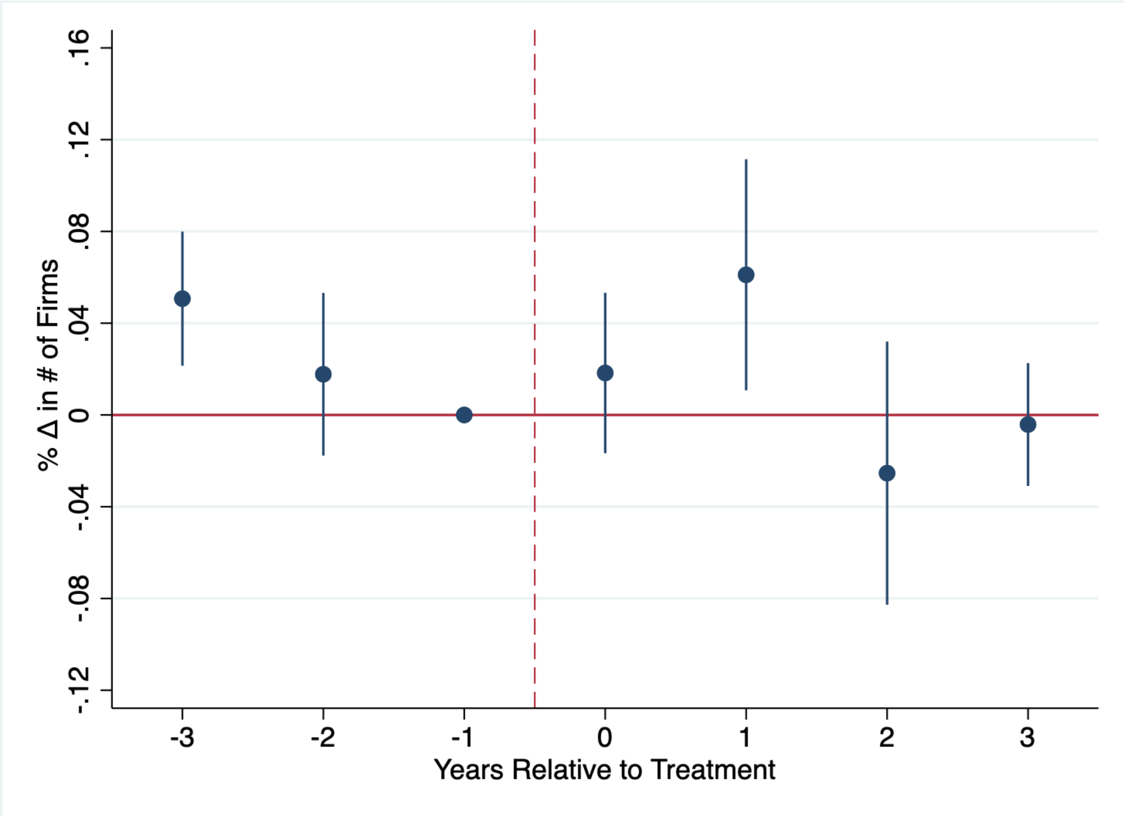
	(1) # of Firms (%Δ)	(2) # of Entrants (%Δ)	(3) # of Exits (%Δ)
Eligible x Post	0.0131	0.0337	-0.00855
	(0.0120)	(0.0659)	(0.0556)
Constant	0.381*	0.807	1.508
	(0.189)	(1.298)	(1.093)
Observations	2,856	2,856	2,856
Sector (1 digit) x Year FE	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

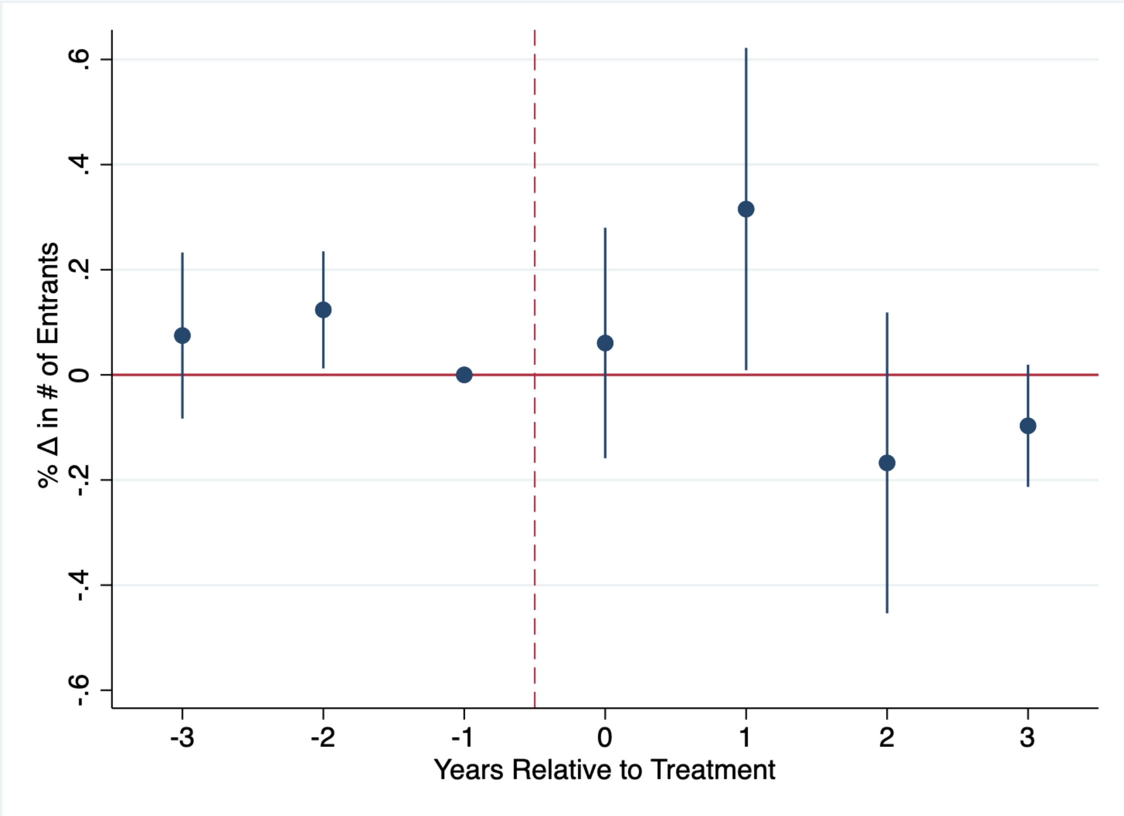
Note: This table reports the difference-in-differences coefficients to study the firm entry and exit responses. None of the results are statistically significant at standard levels of confidence. Column (1) presents a small positive effect on the number of firms. Column (2) suggests a 3.37% variation on the number of entrants due to the reform, and column (3) shows a slightly negative effect on firm exit. All regressions are weighted by the pre-reform sector median firm size. Robust standard errors are provided.

Figure 28: Active Firms: Event Study Estimates



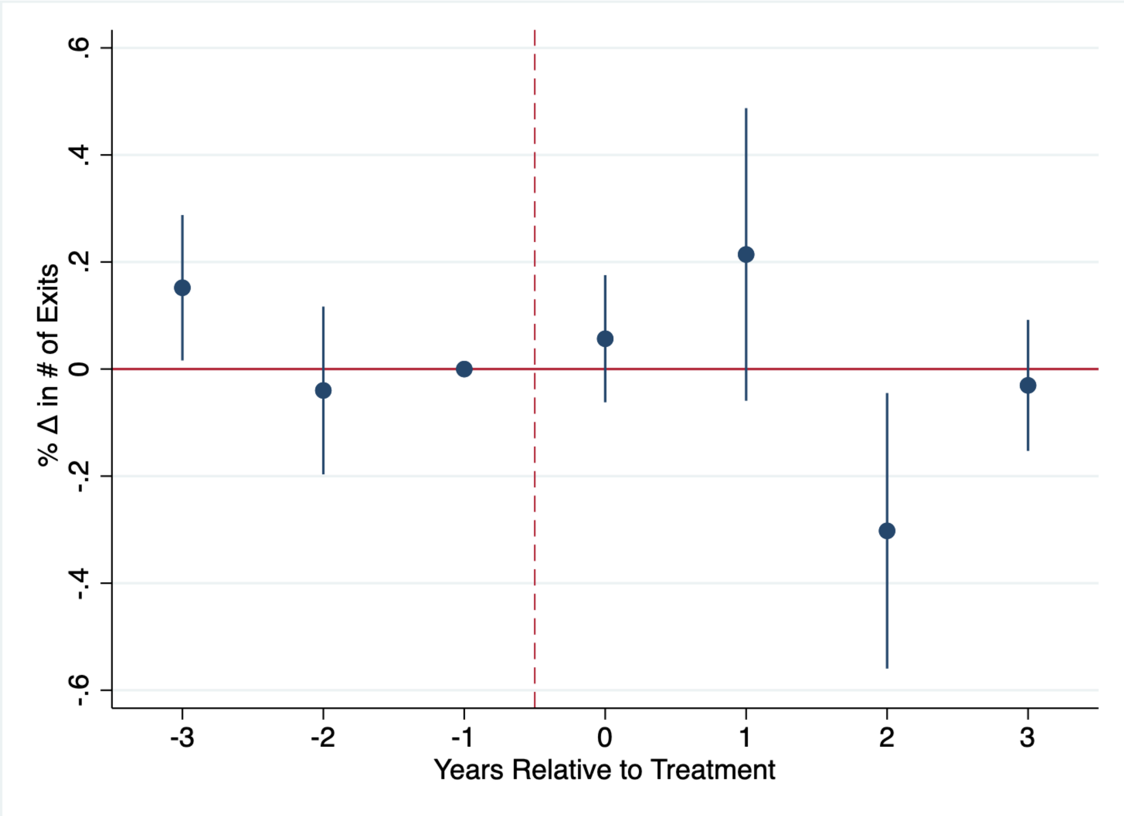
Note: This figure presents the event study estimates for active firms. The outcome is the percentage variation on the number of active firms. The event is the year in which the sector becomes eligible. I normalize the results with respect to one year prior to the event. The analysis spans three years prior to entering the payroll tax cut program and three years after. There is no statistically significant effect on the margin of active firms. The regression is weighted by the pre-reform sector median firm size, and relies on robust standard errors.

Figure 29: Entrant Firms: Event Study Estimates



Note: This figure presents the event study estimates for entrant firms. The outcome is the percentage variation on the number of entrant firms. The event is the year in which the sector becomes eligible. 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. There is no statistically significant effect on the margin of active firms. The regression is weighted by the pre-reform sector median firm size, and relies on robust standard errors.

Figure 30: Exit Firms: Event Study Estimates



Note: This figure presents the event study estimates for exit firms. The outcome is the percentage variation on the number of exit firms. The event is the year in which the sector becomes eligible. 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. There is no statistically significant effect on the margin of active firms. The regression is weighted by the pre-reform sector median firm size, and relies on robust standard errors.

B More Details on the Empirical Model

Given the set of k first stage equations, the reader might not be able to see immediately the reduce form equation. Starting with the firm level design, we obtain the reduced form by substituting all first stage equations into the second stage,

$$Y_{jt} = \sum_{k=-4,\neq-1}^{3} \beta_k \left[\sum_{l=-4,\neq-1}^{3} \pi_{kl} \times \mathbb{I}(t = e_{s(j)} + l) \times L_{s(j)} + \alpha_j + \xi_{s1(j),t} + X'_{jt}\delta_k + \eta_{jt} \right] + X'_{jt}\gamma + \alpha_j + \xi_{s1(j),t} + \epsilon_{jt}$$

where, $D^k_{jt} = 1$, if $t = e_j + k$; e_j is the year when firm j enters treatment; $L_{s(j)}$ indicates if firm j's sector is eventually eligible; $e_{s(j)}$ is the date when firm j's sector becomes eligible; X_{jt} set of controls such as education, race, age and its square; $\xi_{s1(j),t}$ is 1-digit sector x year fixed effect; α_j is the firm fixed effect; η_{jt} and ϵjt are residuals. Standard errors are clustered at the 5 digit industry level. Reorganizing terms,

$$Y_{jt} = \sum_{l=-4,\neq-1}^{3} \left[\sum_{k=-4,\neq-1}^{3} \beta_k \pi_{kl} \times \mathbb{I}(t = e_{s(j)} + l) \times L_{s(j)} \right] + X'_{jt} \left[\gamma + \sum_{k=-4,\neq-1}^{3} \beta_k \delta_k \right] + \\ + (\alpha_j + \xi_{s1(j),t}) \left[1 + \sum_{k=-4,\neq-1}^{3} \beta_k \delta_k \right] + \left[\epsilon_{jt} + \sum_{k=-4,\neq-1}^{3} \beta_k \eta_{jt} \right]$$

Thus, the reduced form coefficient is,

$$\rho_l = \sum_{k=-4,\neq-1}^{3} \beta_k \pi_{kl}$$

Notice that if K=L and diagonal is such that $\pi_{kl} = 0$ (when k $\neq$ l), then $\rho_l = \beta_l \pi_{ll}$, and $\beta_l = \frac{\rho_l}{\pi_{ll}}$. However, if K<L then the system $\rho_l = \sum_{k=-4,\neq-1}^{3} \beta_k$ for l=1,...., L is a system of L equations in K<L unknowns and generally cannot be solved. The off diagonal coefficients estimated in equations 2 and 8 are small and not statistically different than zero, which makes the interpretation of the reduced form coefficients equal to the one dimensional case, i.e., $\rho_l = \beta_l \pi_{ll}$. At the worker level, the algebra to obtain the reduced form coefficient is analogous to the firm level computations presented in this appendix.

C Algebra for Elasticities Computation

Departing from equilibrium in the labor market after the introduction of a small change in the payroll tax wdt

$$L_d d(w(1+dt)) = L_s(w)$$

Differentiating both sides,

$$L_d'(w(1+dt))\left(\frac{dw}{dt} + +\frac{dw}{dt}dt + w\right) = L_s'(w)\frac{dw}{dt}$$

$$\frac{dw}{dt}\left(L_d'(w(1+dt))(1+dt) - L_s'(w)\right) = -wL_d'(w(1+dt))$$

$$\frac{dw}{dt}\frac{1}{w} = \frac{-L_d'(w(1+dt))}{L_d'(w(1+dt))(1+dt) - L_s'(w)} = \frac{-\epsilon_D}{\epsilon_D + \epsilon_S}$$

Thus,

$$\frac{d\log w}{dt} = \frac{-\epsilon_D}{\epsilon_D + \epsilon_S} \tag{11}$$

where, $\epsilon_D = -w\frac{L_d'}{L_d}$ and $\epsilon_S = \frac{w}{L_s}L_s'$

Equation 11 indicates that after a payroll tax cut, net wages increase the more that demand is more elastic relative to supply. In this model the tax incidence is independent of the tax collector (firm vs worker). Similarly, one can define the gross earnings as $W = w(1+dt)$, and the market equilibrium after the tax reform is,

$$L_d(W) = L_s(W - Wdt)$$

Differentiating both sides and following similar algebra,

$$\frac{dW}{dt}\frac{1}{W} = \frac{d\log W}{dt} = \frac{-\epsilon_S}{\epsilon_D - \epsilon_S} \tag{12}$$

Equation 12 indicates that after the payroll tax cut, gross wages decrease the more that demand is less elastic relative to supply. Finally, the fall on employment due to the tax reform is given by,

$$\frac{-dL(w)}{dt} = -L_s'(w)\frac{dw}{dt} = -\frac{L}{w}\epsilon_S\frac{dw}{dt} = L\epsilon_S\frac{\epsilon_D}{\epsilon_D + \epsilon_S}$$

where, L is total employment and the last equality relies on $\frac{dw}{dt}\frac{1}{w} = \frac{-\epsilon_D}{\epsilon_D+\epsilon_S}$. Thus,

$$\frac{d\log L}{dt} = \frac{\epsilon_S \epsilon_D}{\epsilon_S + \epsilon_D} \tag{13}$$

From the empirical analysis we obtain the left hand side of equations 11, 12 and 13. Therefore, I will compute the two elasticities using equations 11 and 13. I plug the estimated elasticities on the right hand side of equation 12, as a final verification step. I find $\epsilon_D = 0.54$, and $\epsilon_S = 6.67$. It is important to highlight that the labor supply elasticity relates to the elasticity faced by the firm, or by the treated unit (sector). This elasticity is different from the market labor supply elasticity, which should be much smaller. The rationale is that when wages are shocked at the sector level, the mobility across sectors allow workers to be more responsive compared to a market wide shock.

The labor supply elasticity at the sector level ($\epsilon_S = 6.67$) is high, but not too far off from other recent studies. For instance, Azar, Berry, and Marinescu 2019 found that firms face a labor supply elasticity of 5.8, and Dube, Giuliano, and Leonard 2019 found elasticity of 4.6. The labor supply elasticity found for the Brazilian market implies that firms can reduce wages by roughly 15% below the marginal product of labor.

Robustness Checks

[TO BE INCLUDED]

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