Who Benefits from Payroll Tax Cuts? Market Power, Tax Incidence and Efficiency

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January 2024

Abstract

This paper studies firms' margins of response to a historically large payroll tax cut that affects a subset of Brazilian firms. Difference-in-differences estimates based on plausibly exogenous legal variation indicate that the payroll tax reduction causes an increase in employment, wages, and profits, while capital decreases. Responses are substantially more pronounced among small firms, and workers' earnings gains are concentrated at the top of the distribution. This evidence cannot be reconciled within a competitive framework. I estimate a model that allows for product and labor market power to explain these findings. Reduced-form estimates reveal that consumers pay 65% of payroll taxes, firm owners 23%, and workers 12%. These results establish not only that payroll tax cuts primarily benefit consumers, but also exacerbate within-firm earnings inequality.

*Department of Economics, lobel@berkeley.edu. I am indebted to Emmanuel Saez, Patrick Kline, Alan Auerbach and Benjamin Schoefer for their guidance and encouragement on this project. I also would like to thank Michael Amior, Nano Barahona, Michael Best, Bruno Barsanetti, Sydney Caldwell, Chris Campos, David Card, Carlos da Costa, Cláudio Ferraz, Fred Finan, François Gerard, Gustavo Gonzaga, Larry Katz, Etienne Lehmann, Adrien Matray, Edward Miguel, Conrad Miller, Enrico Moretti, Mathilde Muñoz, Jesse Rothstein, Juan Carlos Serrato, David Sraer, Stefanie Stantcheva, Jón Steinsson, João Thereze, Ricardo Perez-Truglia, Reed Walker, Chris Walters, Danny Yagan, Gabriel Zucman, and seminar participants at UC Berkeley, NBER Public Economics Meeting, NBER Business Taxation, Harvard Public Finance and Labor Workshop, NTA Annual Meeting, SOLE Conference, IIPF Annual Congress, and Cowles Conference for very helpful comments. This paper previously circulated with the title "The Unequal Incidence of Payroll Taxes with Imperfect Competition."

1 Introduction

Who benefits from payroll tax cuts? This question has emerged as one of the most important topics in the public discourse as payroll taxes account for 30% of total tax collection, and the adoption of payroll tax cut programs is becoming widespread (OECD 2019). Traditional Public Finance approaches this issue within a competitive framework, in which the answer arises from properties of aggregate labor demand and supply (Gruber 1997). This study challenges the traditional view, by providing evidence that product and labor market power are also central in shaping tax incidence.

This paper investigates the implications of payroll tax cuts in the context of Brazil, which implemented a payroll tax reform in 2012. Due to arbitrary sector-specific legal requirements, tax rates were reduced by 20 p.p. for a small subset of firms. The political process that determined eligibility often assigned remarkably similar sectors to different eligibility statuses, as illustrated by the cases of hotels and motels. Eligible and ineligible firms are not only similar in levels but most importantly, in pre-reform trends. This resemblance between groups provides a compelling basis for comparison, which I implement in a difference-in-differences specification. To evaluate this policy variation, I rely on novel anonymized administrative tax microdata, which enables the tracking of firms and workers over time, both before and after the reform.

I find that the tax cut caused a 12% employment increase three years after the implementation, a phenomenon even more pronounced among small firms. The competitive framework predicts that a firm-specific shock, which does not change workers' outside options, should not affect workers' earnings. However, I find that earnings increased by 3%. While these effects could potentially be influenced by compositional changes, the absence of any empirical evidence for such adjustments further substantiates the presence of labor market power. Interestingly, most gains are captured by individuals in the top percentiles of the earnings distribution, witnessing gains as high as 14%. This finding underscores that payroll tax cuts exacerbate within-firm earnings inequality.

Consistent with the unequal pass-through within firms, there are significant differences across occupations and races. Specifically, high-skilled workers benefit from a 6% pass-through, while low-skilled workers witness no gains from the same tax cut. I am not able to detect significant differences across gender. All of the earnings increase is concentrated among white workers. While racial disparities are a core concern in the social sciences, to the best of my knowledge this is the first study to empirically assess racial inequality in tax pass-through.

The lack of prior evidence stems from the fact that most tax authorities, the US among them, do not record race information.

Given that rich administrative microdata were previously unavailable to researchers, the payroll tax literature has focused on employment and wage responses. This study broadens the analysis by incorporating understudied margins of adjustment such as capital, profits, and revenue. Interpreting the capital response is not straightforward, since substitution and scale forces operate in opposite directions. Consistent with an optimal behavior of substituting toward cheaper inputs, I find that a decrease in labor costs leads to a 3% reduction in capital usage. Likewise, the revenue response is influenced by a quantity increase and a price decrease. I find a 5% revenue rise, which, combined with the scale response identified by the inputs choice, helps to quantify the extent of tax incidence passed onto prices. Profits - a key metric for gauging firms' willingness to pay for a tax reduction - surged by 30% in response to the reform. This empirical result is particularly meaningful, as numerous previous incidence papers do not observe accounting profits and instead rely on structural assumptions (e.g., Suárez Serrato and Zidar 2016a; Suarez Serrato and Zidar 2023).

The identifying assumption is that, conditional on fixed effects, eligibility is uncorrelated with time-varying unobserved determinants of outcomes. The first threat to this assumption relates to selection on eligibility, i.e., that Congress anticipated sector-specific trends when defining eligibility. To address this concern, I show not only that pre-trends are not statistically indistinguishable from zero in any of the outcomes, but also that eligibility is balanced in baseline levels. As an additional robustness test, I recover determinants of eligibility using a logit model and apply the associated propensity scores in a matching difference-in-differences procedure, which alternatively relies on the conditional independence assumption (CIA). Results from both methods are similar. The second threat to identification relates to the manipulation of sectoral choice. To address this concern, I first show in the data that firms rarely change sectors. Further, I confirm that the results remain the same even when the sample is restricted to firms that have never changed sectors.

Although employment increases after the reform, this effect could be driven by mere shifts from existing informal to formal jobs, both within and across firms. This margin of response is particularly relevant in the landscape of developing countries (Ulyssea 2018b; Haanwinckel and Soares 2021). Nevertheless, I conducted several tests indicating that informality does not play a major role in response to the payroll tax variation. In one of these tests, I leverage the panel structure of the data to show that the reform does not affect the share of formal new hires transitioning from non-employment or informality. This result is consistent with the fact that the informal sector in Brazil is predominantly characterized by self-employment and is prominently susceptible to fixed costs associated with licensing, legal liabilities, sanitary and security regulations (Maloney 2004).

To interpret the empirical findings, I develop a simple model in which firms have labor market power, as in Manning 2011; Card et al. 2018, and product market power as in Hamermesh 1996; Criscuolo et al. 2019. The interplay between these two competitive frictions, often modeled separately, sheds light on a key aspect: employment and wage pass-through are determined not just by the slope of the labor supply and product demand curves, but also hinge on behavioral responses that guide shifts of the marginal revenue product of labor and product supply. Consistent with the model, I find that both employment and earnings effects are more pronounced in small firms – the ones estimated to have less market power. This pattern, which standard monopsony models in a perfectly competitive product market fail to explain, resonates with a broad range of empirical findings in the context of industrial policies (Bronzini and Iachini 2014; Howell 2017; Zwick and Mahon 2017; Criscuolo et al. 2019).

The model delivers invertible mapping between relevant parameters and reduced form estimates. I estimate the labor supply elasticity faced by the firm (ϵ = 4.15), capital-labor elasticity of substitution (σ_{KL} = 1.72), and output demand elasticity (η = 1.43). The labor supply elasticity implies a wage markdown of 0.81, suggesting that Brazilian firms capture 19% of the marginal revenue product of labor. This value aligns closely with estimates from other countries (Card et al. 2018). The capital-labor elasticity of substitution is similar to Karabarbounis and Neiman 2014. Lastly, the output demand elasticity reveals the presence of product market power, with an estimated markup of 0.41, which seats toward the upper range of prior estimates, but still between the values found in Harasztosi and Lindner 2019 and Curtis et al. 2021.

In terms of mechanisms, firms increase their scale by 6%, which accounts for two-thirds of the employment response. The remaining one-third stems from capital-labor substitution. Aligned with the stronger employment, revenue and earnings-per-worker response depicted empirically, the scale effect is more pronounced for small firms, rising to 24%. This greater expansion is primarily a consequence of their limited market power, which can enable them to grow more without exerting excessive pressure on prices. A full-incidence analysis indicates that consumers pay 65% of payroll taxes, workers 12%, and firm owners 23%. To measure the deadweight loss of payroll taxation, the model connects reduced-form responses to changes in economic surplus and the net fiscal cost. On the margin, an additional dollar in tax cuts leads to a $0.66 in efficiency gains. This relates to a marginal value of public funds (MVPF) of 1.66, which reflects the high distortionary costs of taxation in developing countries. This estimate falls in the upper range of the 0.5-2 interval reviewed by Hendren and Sprung-Keyser 2020.

Literature and Contributions. The paper's main contribution is to thoroughly assess firms' margins of response to payroll taxation. The study provides theoretical insights into the role of market power in shaping tax incidence and efficiency. To the best of my knowledge, this is the first paper to incorporate consumers in payroll tax incidence analysis. Although the incidence to consumers is novel to Public Finance, my estimate aligns closely with the minimum wage literature (Harasztosi and Lindner 2019).

This paper contributes to four strands of literature. First, it builds on a large body of work that finds mixed effects of payroll tax cuts on employment and wages (Gruber 1997; Saez et al. 2019; Kugler et al. 2017; Cruces et al. 2010; Kugler and Kugler 2009; Saez et al. 2012). This study can reconcile the debate by adding a key element: market power. Ongoing work (Biro et al. 2022) also accounts for the role of labor (not product) market power in the tax incidence analysis. However, they analyze an age-specific lump-sum policy, while I study a firm-specific tax rate variation, which allows me to directly measure labor and product market power and alleviates pay equity confounding concerns (Dube et al. 2019; Breza et al. 2018).

The Brazilian payroll tax variation of 20 p.p. is unprecedented. In the US, for example, research that leverages payroll tax variation relies on changes of less than 1 p.p (Guo 2023).[1] To advance cutting-edge research that leverages employee-level data to offer a nuanced understanding of how corporate taxes affect various groups of workers (Ohrn 2023; Carbonnier et al. 2022; Risch 2024), I pair the sizable shock with rich employer-employee matched tax microdata. I find that responses to the tax cut vary not only among different types of workers but also across firms.

Second, my empirical findings document clear evidence that Brazilian firms retain labor market power, which is in line with a burgeoning strand of frontier research (Card et al. 2018; Berger et al. 2022; Jäger and Heining 2022; Kline et

[1] Studies on the Brazilian payroll tax reform (Dallava 2014, Baumgartner et al. 2022, Scherer 2015) rely on aggregated sector data, and do not analyze business outcomes. Anonymized firm-level tax data allows for observation of the two margins of imperfect compliance: eligible firms that do not take-up, and those that are treated in outside sectors due to the product criteria.

al. 2019; Garin and Silvério 2019; Benmelech et al. 2022; Burdett and Mortensen 1998). I build on this body of work by quantifying the channels through which imperfect competition shapes firms' responses to industrial policies, which in turn impacts the incidence and efficiency of government subsidies. Differently from Berger et al. 2022, this paper integrates labor and product market power, taking the model directly to heterogeneous firm-level empirical responses. As argued by Manning 2021, few papers aim to directly estimate the labor supply curve faced by the firm, mostly because it is challenging for researchers to disentangle market from firm-level shocks. The frontier has adopted two alternatives: a model-based, and an experimental-based approach (Dal Bo et al. 2013; Dube et al. 2020; Belot et al. 2019). I contribute to this strand by providing well-identified quasi-experimental evidence, leveraging the uniqueness of the Brazilian reform.

Third, this study also advances the literature by estimating elasticities of substitution between capital and labor (Karabarbounis and Neiman 2014; Raval 2019; Chirinko et al. 2011; Caballero et al. 1995; Oberfield and Raval 2021). In a meta-analysis, Gechert et al. 2022 criticize prior work because of the use of cross-country variation and omission of the first-order condition for capital. Papers that have addressed these concerns, as I do, using local variation and optimality conditions for both inputs (Harasztosi and Lindner 2019; Curtis et al. 2021) have suffered from not accounting for labor market power.

Finally, an important industrial policy literature studies government subsidies for R&D (Bronzini and Iachini 2014; Howell 2017); equipment (Zwick and Mahon 2017); and investment (Criscuolo et al. 2019). This body of work has found that subsidies are more effective for boosting employment in small businesses. This paper is the first to document this pattern for payroll tax changes. In addition, it posits that market power can be a key ingredient in rationalizing the mechanism behind the notable responses of small firms in this literature.

The rest of the paper is organized as follows. Section 2 presents institutional background and data. Section 3 presents the empirical analysis, including data-driven evidence of market power. Section 4 develops the model. Section 5 identifies and estimates the model. Section 6 estimates the incidence and excess burden associated with the Brazilian payroll tax system. Section 7 concludes.

2 Institutional Background and Data

This section describes the institutional background of the payroll tax system in Brazil and provides details on the payroll tax reform implemented in 2012. The section then describes the main datasets used to measure the effects of payroll tax variation on various outcomes, for different types of workers and firms.

2.1 Brazilian Payroll Tax System and the 2012 Reform

Similar to most OECD countries, Brazilian payroll taxes are designed to fund social security programs, such as retirement pensions and unemployment insurance. Tax rates are also similar to those of other OECD countries (see Figure H.1 for cross-country comparison). In contrast to other tax reforms studied in the past, the Brazilian payroll tax cut program offers unique advantages from an empirical perspective. First, it alleviates pay equity concerns as the policy is targeted at the firm, rather than worker-level. Second, the Brazilian reform offered a large first-stage, evidenced by a 20 p.p. payroll tax reduction. Third, only a few firms were affected, minimizing general equilibrium effects and SUTVA violations. Fourth, the reform lasted for many years, allowing for short- and long-term decomposition.

Institutional Setting. The Brazilian payroll tax schedule has three components, and all are collected from firms. The main component is a 20% flat tax over the total wage bill, which is affected by the reform. Second, there is an accident risk insurance component that varies between 1% and 3%. The last layer is an 8% to 11% tax on wages, which is employee-specific and can vary among workers in the same firm. These tax components are deposited in a social security fund that pools resources from all workers in the country. This means that the public social security system does not provide individual savings accounts in which resources could be traceable and mapped to workers' specific benefits.

Policy Motivation. The official goal announced for the tax reform was to increase the competitiveness of Brazilian firms. The Government at the time had the tradition of engaging in industrial policies that subsidized specific corporations and sectors. To uncover the Government's rationale for favoring certain firms over others, I conducted extensive empirical investigations. I tested (and rejected) the hypothesis that becoming eligible for tax benefits was associated with more contributions to political campaigns. Section 3.5 leverages an additional analysis that relies on propensity scores to predict eligibility. Overall, the suggestive evidence indicates that the process of defining eligibility was a complex political decision, which did not seem to anticipate sector-specific trends. It is important to underscore that the research design does not assume random eligibility assignment. Instead, it posits that in the absence of the tax reform, eligible and ineligible sectors would have followed a similar trajectory. Section 3.5 presents a set of tests that provide details on the eligibility rules and test trends and balance across the eligibility status.

Eligibility. The policy established sector- and product-specific eligibility criteria for the payroll tax exemption. Product eligibility was defined based on Mercosur Common Nomenclature (NCM). Most of the product-eligible firms are in the manufacturing industry, but treatment due to NCM criteria is not restricted to the manufacturing sector. Indeed, all sectors in the Brazilian economy contain firms treated based on NCM product criteria.[2] Treatment due to the NCM eligibility criterion only allows for a partial payroll tax waiver, according to the share of eligible products in the firms' gross income.

Within broadly defined industries, the reform did not grant eligibility to all sectors. For example, the media industry is eligible for the open television sector, but it is not for cable television. This finely detailed level of eligibility assignment across similar sectors provides a compelling basis for comparison, which I implement in a difference-in-differences framework. It also mitigates confounding concerns from concurrent policies, such as those under the umbrella of "Plano Brasil Maior", which did not target the same sectors at such a granular level. In the empirical analysis, I add industry-year fixed effects to leverage variation within broadly defined industries, which further alleviates concerns related to other industry-specific shocks.

Timing. The first tax bill outlining the policies and eligible sectors was passed in December 2011 and implemented a few months later, April 2012. The reform was initially outlined in an executive bill that skipped prior discussion in Congress. This type of payroll tax cut had never been implemented previously in Brazil, so the policy was not expected by employers and employees. The policy is still valid today, and there is no expectation of being eliminated soon. Several other tax bills added more sectors to the reform in 2013 and 2014.[3]

Tax Variation. On December 14, 2011 Congress enacted the payroll tax cut reform that waived the main component of payroll taxation for a small share of sectors and products. Treated firms faced a uniform decrease in payroll tax rates, from 30 p.p. to 10 p.p. of the total wage bill, without any cap for high-income earners. To provide slight compensation to the government budget in the face of this large drop in tax collection, targeted firms were forced to pay a small 1% to 2.5% tax on the gross revenue. Importantly, the reform did not affect individuals'

[2]This can be precisely observed in the anonymized micro tax data.

[3]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.

perception of the solvency of their retirement plans, because the Federal Treasury committed to cover any deficits caused to the social security system.

Within-Sector Variation. In several of the granular eligible sectors, several firms were not affected by the reform. We need to start by remembering that 45% of firms in Brazil are informal (Ulyssea 2018a) and do not pay payroll taxes. Also, firms in the "Simples" tax regime are not subject to payroll taxes, and therefore not affected by the reform even if they are in eligible sectors.[4] Finally, among firms that satisfy all of the eligibility requirements, a substantial share of those do not take-up the benefit. Section 2.2 focuses on understanding imperfect take-up behavior.

Overall, less than 2% of formal firms in the country are impacted by the reform. Even within granularly defined local labor markets, less than 3% of firms are affected. To highlight the modest macro relevance, Table A.1 shows that at the peak of its implementation in 2014, the payroll tax cut program covered only a relatively small share of Brazilian sectors (9%), firms (1.7%), and workers (6%).[5] Section 3.2 provides several spillover tests that support the view that the reform should be seen as a firm- rather than a market-level shock.

2.2 Data and Descriptive Statistics

By combining tax and labor administrative data on the universe of formal firms operating in Brazil between 2008 and 2017, I constructed two samples. One at the firm- and the other at the worker-level. The final dataset is anonymized and arranged in a panel structure. Below, I describe each data source.

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 Labor. This dataset is compiled annually and contain information on all formal job spells in the country. It uniquely identifies workers and firms based on tax codes (PIS and CNPJ, respectively), which do not change over time. The data include firms' characteristics such as sector, age and location. It also covers detailed workers' information, such as occupation, earnings, race, gender, industry, and municipality, as well as hiring and termination dates. The main shortcoming is the lack of information on informal and non-employed

[4]This alternative tax system was created in 1996 and had two main goals: to simplify tax rules and reduce the tax burden on small corporations.

[5]The fact that "Simples" firms are not eligible and there is imperfect take-up in eligible sectors contributes to the share of firms being smaller than the share of sectors. The fact that larger firms are more likely to take-up contributes to the share of workers being larger than the share of treated firms.

workers. To access information on the informal sector, I rely on the 2010 Census, which is administered by the Brazilian Census Bureau (IBGE). The Census measures formalization rates in each of the 5,300 Brazilian municipalities.

Anonymized Tax Records. To conduct a comprehensive analysis of the tax reform, this study relied on detailed anonymized data from the Brazilian federal tax authority (RFB). These data include information on the universe of corporate tax returns, including payroll and revenue taxes, gross revenue, capital, and profits.[6] The data structure is a panel at firm-by-year level, ranging from 2008 to 2017. A firm is defined based on the 8-digit tax code, known as "CNPJ", which aggregates all establishments by firms. This is the relevant unit of analysis because tax planning across establishments tends to be consolidated at firm-level. In any case, 95% of firms are single establishment and 99% of firms are single sector.

Firm Sample. To appropriately study the payroll tax reform in Brazil using administrative data, I imposed a few sample restrictions. I focus on firms that throughout the analysis have never participated in the *Simples Nacional*, which is a special tax tier not subject to payroll taxes. This restriction is crucial, because firms switching in and out of the *Simples* regime would exhibit gaps in their observed payroll tax data.

The sample provides a broad representation of the Brazilian economy and covers 19 out of 21 industries in the Brazilian economy. The construction industry is not included because the reform applied to construction firms on a site-specific basis, rather than at firm-level. Without access to detailed construction site-level data, I cannot accurately determine the proportion of treated sites within a firm, the number of workers employed at specific sites, or assess the precise effect of the policy on construction payroll tax liability. Also, construction was at the epicenter of the "Car Wash" operation, a massive corruption scandal uncovered during the decade this study examines. Investigations revealed that economic transactions within the construction industry were heavily influenced by illicit business arrangements, which led to the bankruptcy of major construction players.

The retail industry is not included in the sample because I am not able to control for changes in the value-added tax system (VAT) known as ICMS. This tax is predominantly concentrated in the retail industry, in which over 85% of the tax

[6] Due to confidentiality constraints, these data was not shared with the researcher. The anonymized tax data were handled solely by the tax authority on official computers, and all results have been reviewed to preserve full confidentiality.

collected stems from VAT (Naritomi 2019). While payroll taxes are administered at the federal level in Brazil, states are responsible for VAT. During the period of analysis, states engaged in multiple VAT tax reforms. These include sector and product-specific exemptions and rate changes, as well as variations in withholding policies and auditing programs. The main sample is not winsorized or balanced, but the results are robust to these procedures (see Appendix G).

Worker-level Sample. To maintain consistency between the firm- and worker-level analysis, I apply the same restrictions previously discussed to ensure an equivalent set of employers in both data sets. I follow the displacement literature (Jacobson et al. 1993; Lachowska et al. 2020) and impose a tenure restriction in order to focus on workers who have been employed for at least three years in the pre-reform period. In this sample, workers are assigned to treatment based on their pre-reform employer, regardless of the firms they end up working for.

Take-up. There is a substantial share of eligible firms that do not take-up the benefit. This phenomenon is generalized across all cohorts of eligibility from the beginning of the program. It may be puzzling that numerous eligible firms are not taking advantage of the generous government subsidies. To interpret this observational fact, it is important to bear in mind that the increase in revenue tax would surpass the payroll tax decrease for only 1% of eligible firms. Thus, the substantial imperfect take-up cannot be rationalized through the lens of a perfect tax optimization choice.

A few facts help to rationalize the imperfect take-up. First, enrollment in the program was not automatic, as in the Swedish case studied by Saez et al. 2019. In Brazil, firms have to self-report eligibility on government-provided software to enable tax exemptions, based on separate tax forms. Second, the tax bills did not impose punishment for non-compliers, possibly because, from a legal point of view, eligibility was seen as beneficial to firms. Based on the Brazilian tax code, it is implausible that prosecutors would sue firms that do not opt into a supposedly beneficial tax system.

Even though enrollment implied a net tax cut, empirical findings in other countries (Kleven and Waseem 2013; Janet et al. 2006; Zwick 2021; Moffitt 2007) suggest that the operational process can lead to non-responsiveness, even in dominated tax regions.[7] Take-up is monotonically increasing with firm size. This

[7]This is related to an extensive body of work dedicated to understanding rational attention (Hoopes et al. 2015) and other frictions that can rationalize low participation rates in public programs (Currie et al. 2001; Heckman and Smith 2004). Similarly, several papers study the role of tax salience (Chetty et al. 2009; Chetty et al. 2013; Finkelstein 2009)

pattern is consistent with the fact that larger firms are more likely to have accounting support, be aware of tax benefits, and be able to pay for filling costs.

Payroll Tax Cuts. Figure 1 compares payroll and revenue tax rates for firms that were treated at some point in time versus those that never received the tax benefits. The group of never-treated firms includes, for example, eligible firms that did not take-up the benefit. Revenue taxes are divided by the total wage bill, so all tax rates are comparable. Reassuringly, tax rates from the raw data in Figure 1 align well with statutory rates. The figure reports unprecedented payroll tax reductions. For context, studies that leverage payroll tax variation in the US rely on changes of less than 1 p.p (Guo 2023). Also, it is important to note that the payroll tax drop is considerably greater than the revenue tax increase, which reinforces the interpretation of an overall tax cut as opposed to a tax substitution.[8]

3 Empirical Analysis

The payroll tax cut causes a sharp expansion in employment, with small but significant effects on long-term pre-tax wages. In this section, I present details on the main results, including heterogeneity analysis across firm size and workers' characteristics.

3.1 Identification Strategy

The main empirical strategy is a fuzzy event study instrumented by sector eligibility. The design explores the staggered implementation of the program and the fact that the vast majority of firms are never eligible or treated.[9] The IV is necessary to adjust for two margins of imperfect compliance: imperfect take-up in eligible sectors and take-up in ineligible sectors due to NCM product eligibility criteria. I estimate the following structural equation:

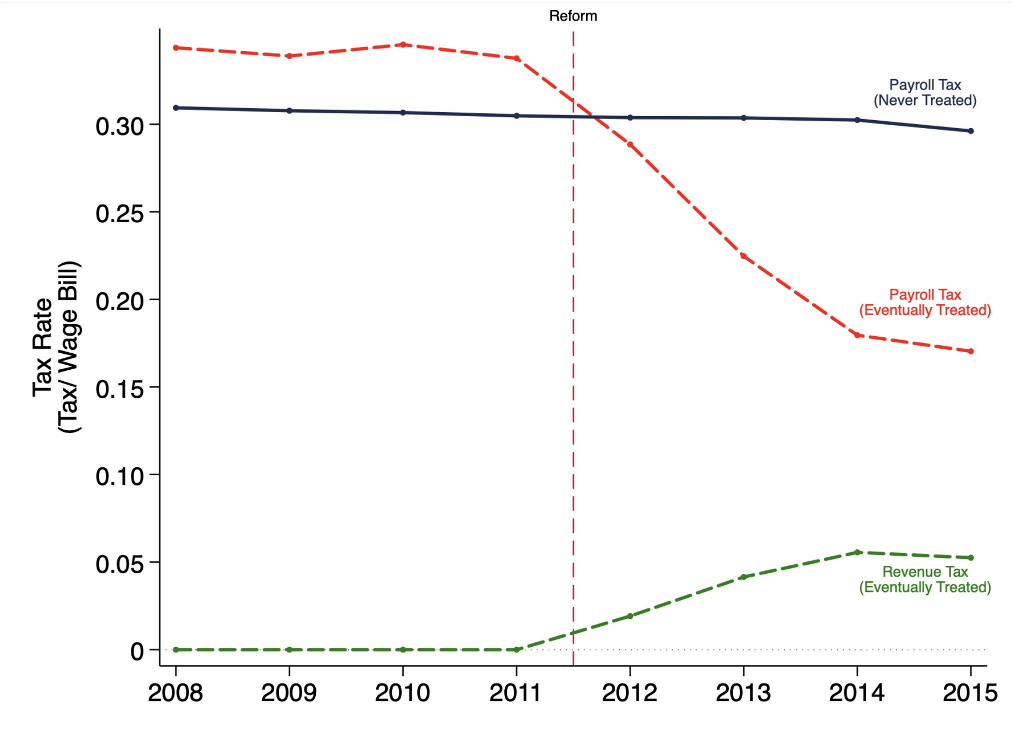
$$Y_{jt} = \sum_{k=-4,\neq -1}^{3} \beta_k D_{jt}^k + X_{jt}'\gamma + \alpha_j + \xi_{I(j),t} + \epsilon_{jt} \tag{1}$$

where, $\xi_{I(j),t}$ is industry (broader than sector) interacted with year fixed effect, α_j is the firm fixed effect, and k indexes the time relative to treatment, X_{jt} are a set of controls for workforce composition (e.g., education, gender, race, age,

[8]The graph reports averages, but even when we look at outliers in the labor share, only in 1% of cases would it not be advantageous taking up the benefit.

[9]The fact that the vast majority of firms are never treated mitigates concerns related to the staggered rollout in two-way fixed effects models (De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021).

Figure 1: Tax Variation



Note: This figure presents the evolution of tax rates for eventually treated vs never treated ones. The blue line depicts that payroll tax rates for never treated firms are stable over time. 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.

and its square), the results remain robust even when the set of controls X_{jt} are removed from the specification. 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

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

where, $e_{s(j)}$ is the event date, in which firm j's sector becomes eligible, $L_{s(j)}$ indicates whether firm j's sector is eventually eligible, and the remaining coefficients are the same as described before. Because eligibility is defined at the sector level (mostly at 7-digit), I conservatively cluster at 5-digit industry-by-state level (Bertrand et al. 2004 ; Cameron and Miller 2015). Appendix B provides more details on the empirical model, underlying assumptions, and reduced-form equations.

I also estimate an IV difference-in-differences model, in which all periods after the policy implementation are pooled into a single post-period indicator. The first stage and structural equations are outlined in equations (3) and (4), respectively:

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

where, D_{jt} indicates that firm j is treated in year t, $L_{s(j)t}$ indicates that firm j's sector became eligible before period t, and the remaining coefficients are the same as before. The first-stage coefficient π increases as the take-up rate on treated sectors increases, and deflates as more treatments occur in non-treated sectors due to the NCM criteria. The associated reduced form is expressed in equation (4):

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

Validity of Design. Identification relies on the assumption that conditional on fixed effects, eligibility is uncorrelated with time-varying unobserved determinants of employment and wage growth. This implies that in the absence of the reform, outcomes for eligible and ineligible firms would follow similar trends. There are two main threats to this design. The first is the potential for Congress to anticipate sector-specific trends when determining eligibility rules. The second threat stems from the possibility that firms might strategically move to eligible sectors after the reform is announced. Section 3.5 provides several tests that mitigate these concerns.

3.2 Firm vs Market-level Shock

Theoretical predictions regarding the effects of a tax change hinge on whether the shock impacts the entire market or is specific to particular firms. The fact that a very small share (1.5%) of formal firms and workers benefited from the tax cut is indicative (but not conclusive) that the reform should be seen as a firm-specific variation. To further examine this, I follow literature that has considered job-switching patterns to define local labor markets (Felix 2021). This analysis shows that 67% of Brazilian job switchers stay in the same occupation and region rather than the same industry. That said, I define the local labor market in occupation x region cells.[10] To evaluate spillovers within the local labor market, I leverage several tests.

First, I provide purely descriptive evidence that even at the local labor market level, the share of treated firms is small. Within eligible sectors, there are unaffected firms that are either in the informal sector or in ineligible tax tier (*Simples*) or decided not to take-up the benefit. Table H.1 walks through this logic and shows that conditional on having an eligible sector in the local labor market (LLM), less than 3% of firms in the LLM are affected.

Second, I run a spillover test using firms from the *Simples* tax regime (ineligible tax tier). These firms are ineligible for the payroll tax benefit, but they can operate in eligible industries. If the reform were to create a market-level shock, we should expect to see a negative employment effect in these firms compared with other *Simples* firms in ineligible sectors. Figure A.1 shows that this is not the case. It reports a small and not statistically different than zero spillover effect. It also shows that *Simples* firms in eligible vs ineligible sectors follow similar trends in the pre-reform period. In contrast, it shows that among *non-Simples* firms, there are substantial effects of being in an eligible sector.

Finally, if there were a spillover effect we would expect to see relatively more wage pass-through in intensively treated local labor markets.[11] The reasoning is that spillovers affect workers' outside options, and therefore generate greater wage hikes. Figure H.2 shows that the wage effect for workers in high versus low

[10] This definition uses the 2-digit occupation code from CBO, and the micro-region defined by the Brazilian Census Bureau (IBGE).

[11] I define treatment intensity by the share of treated firms, but the results are qualitatively the same if I define intensively treated markets based on the average or total amount of subsidy.

intensively treated markets is not statistically different from each other. This evidence suggests that the driving force underlying the workers' earnings increase is not the market spillover, supporting the view that the Brazilian payroll tax reform should be interpreted as a firm-specific instead of a market-level shock.

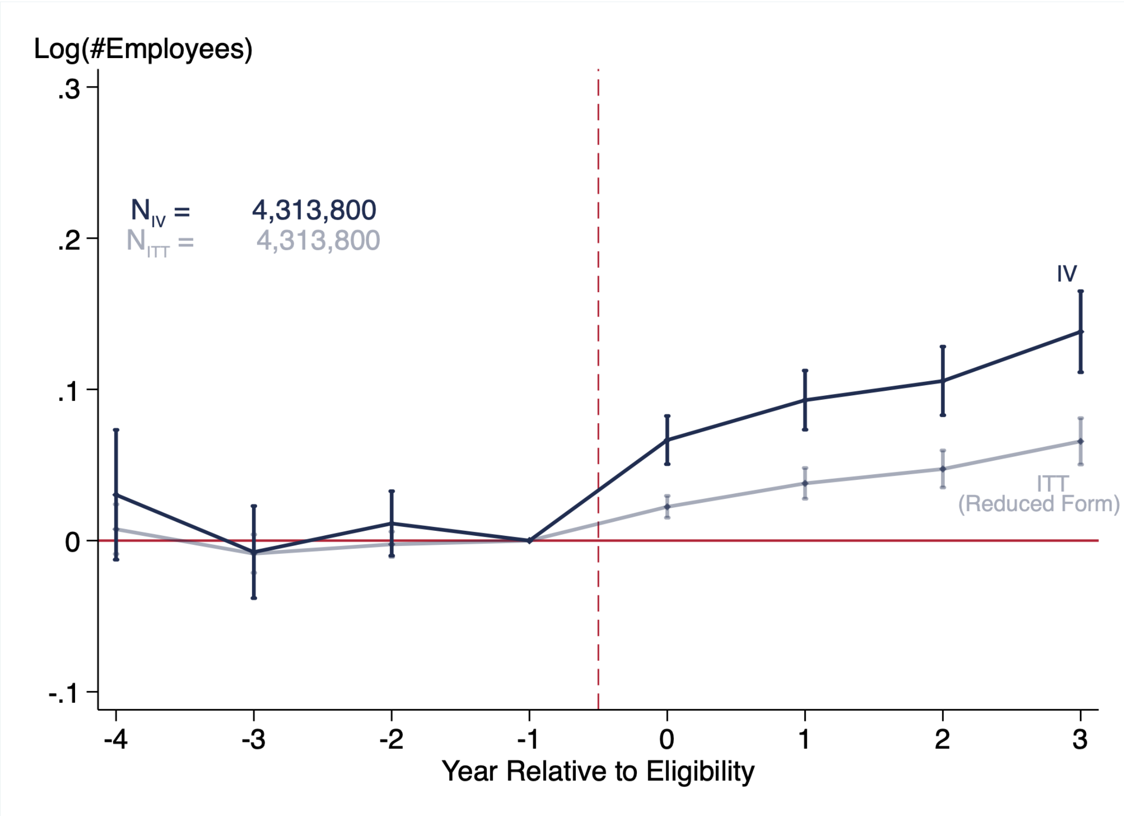
3.3 Firms' Margins of Response

In this subsection, I report the causal effects of the payroll tax reform on a comprehensive set of firm-level outcomes. The findings indicate that after a firm-specific payroll tax cut, employment and earnings-per-worker rise, which is not consistent with perfectly competitive labor markets. In alignment with a standard monopsony framework, the reduced-form estimates reveal an increase in revenue, and profits, whereas capital decreases after the tax cut.

Employment. I begin by analyzing the effects of payroll tax reductions on employment. Figure 2 reports estimates from Equation (1), when the outcome is log employment. Several key findings emerge. First, prior to the reform, there is no statistical difference between employment trends of eligible and non-eligible firms, supporting the validity of the identification assumption. Second, there is an immediate and statistically significant increase in employment following the payroll tax cuts, which is sustained throughout the post-reform period. To address potential concerns related to adjustment costs in interpreting these results (Chetty et al. 2011), I rely on the long-difference (LD) coefficient at t=3 as my preferred point estimate. Column 2 of Table 1 reveals that the reform causes an employment increase of 12% (s.e. 0.031). These results remain qualitatively similar even within the balanced sample of firms (Appendix G), which suggests that the dynamics of firm entry and exit are not governing the employment effect.

The frontier in Public Finance has shown renewed interest in well-identified employment and earnings effects to payroll tax cuts because these responses serve as basis for rationalizing incidence between workers and firm owners (Saez et al. 2019). However, these two margins alone do not paint a complete picture. To advance our knowledge, we aim to understand whether the employment increase is coming from scale or substitution responses. These alternative channels influence prices and output differently, leading to contrasting incidence implications. If employment increases because firms are producing more (scale effect), then the additional supply of goods ultimately benefits consumers. On the other hand, if employment increases because firms are substituting capital with labor while maintaining constant production, consumers face no gains, but there is still additional demand for labor directing some benefits to workers. The challenge of empirically distinguishing these two forces lies in the difficulty of observing capital information at the firm-level. One of the strengths of the tax data is that it allows me to observe firm-level input choices, such as capital stock over time.

Figure 2: Event Study Estimates on Employment



Note: This figure presents event study estimates for employment. The event is the year in which the firm enters treatment for the first time. I normalize results with respect to one year prior to the event. The analysis spans four years prior to entering the payroll tax cut program and three years after. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Capital. The payroll tax reform presents an unambiguous incentive for firms to expand employment from the perspectives of both scale and substitution. Nevertheless, the usage of capital is subject to two counteracting forces. On one hand, reduced labor costs stimulate production, and thus positively affect capital demand. On the other hand, lower labor costs generate incentives to substitute capital for labor. Relying on firm-level anonymized balance sheet information from tax records, column (3) of Table 1 shows that the net effect of scale and substitution responses led to a 3% decrease in capital three years after the payroll tax cut. This result suggests that capital-labor substitution is an important margin of adjustment to payroll taxes. In section 5, I leverage the capital response not only to quantify scale and substitution effects but also to estimate the capital-labor elasticity of substitution.

Usually, capital data is observed only for large manufacturing firms (Zwick and Mahon 2017), an additional strength of this tax data is that even smaller business have to report capital information[12], allowing us to measure the capital-labor adjustments across a broader spectrum. All firms in the "Real Profit" (RP) tax tier are obligated to file balance sheet information in Brazil.[13] Notably, within this tax tier, treatment effects on labor market outcomes closely mirror those estimated for all formal firms.

Earnings-per-worker. Our analysis shows that the payroll tax cut increases labor demand due to its impact on employment. Whether this additional firm-specific demand leads to earnings-per-worker to increase depends on the competitive structure of the labor market. In a perfectly competitive market, firms act as price takers and face a horizontal labor supply curve. Consequently, a firm-specific shock that shifts labor demand should not affect wages. This interpretation hinges on the view that the Brazilian payroll tax reform should be viewed as an idiosyncratic, rather than a market-level, shock. Section 3.2 offer multiple tests that support this view.

Column (4) of Table 1 indicates that the tax cut resulted in a 3% increase in earnings per worker. This finding aligns with the notion that Brazilian firms, on average, do not operate under perfect competition. The detailed microdata, integrating worker and firm-level information, allows us to examine this pattern across the entire within-firm distribution. Figure 3 shows a lack of pre-trends and a gradual increase in effects over time. The inequality in wage pass-through across the within-firm distribution is particularly striking. Three years post-tax cut, the effect is almost negligible at the bottom, but it rises to nearly 14% at the firm's 99th percentile. The monotonic pass-through pattern resembles that seen with firm-specific shocks from patent allowances (as in Kline et al. 2019).

These results shed light on an important consequence of payroll tax cuts: it exacerbates within-firm wage inequality. As the government reduces payroll tax rates, wages for those who already had higher earnings increase relatively more. A possible explanation for this finding could be rent extraction. The substantial earnings effect observed at the top of the within-firm income distribution may

[12]Such as equipments, machinery, vehicles, buildings, computers, and other items that are broadly used.

[13]"Real Profit" is a tax tier for firms with annual gross revenue above USD 15 million.

stem from firm owners, or the owner's family, who are potentially employed at the firm and capable of capturing more rents. In this scenario, we would expect to see more pronounced within-firm earnings disparities among smaller firms, where owners are more likely to hold top managerial positions. By contrast, Figure A.3 indicates that the disparity in earnings pass-through across the income distribution is similar among small and large firms, a phenomenon that does not support the rent extraction view.

The within-firm inequality in earnings pass-through is consistent with the notion that firms face steeper labor supply curves for high-skill workers, requiring them to accept more wage gains when expanding employment. Conversely, low-skill workers may be seen as interchangeable and easily hired at market wages during firm expansion. This interpretation is further supported by the relatively smaller number of employers hiring high-skill workers, as indicated by the higher local labor market concentration at the high-skill employment level (Table H.2).

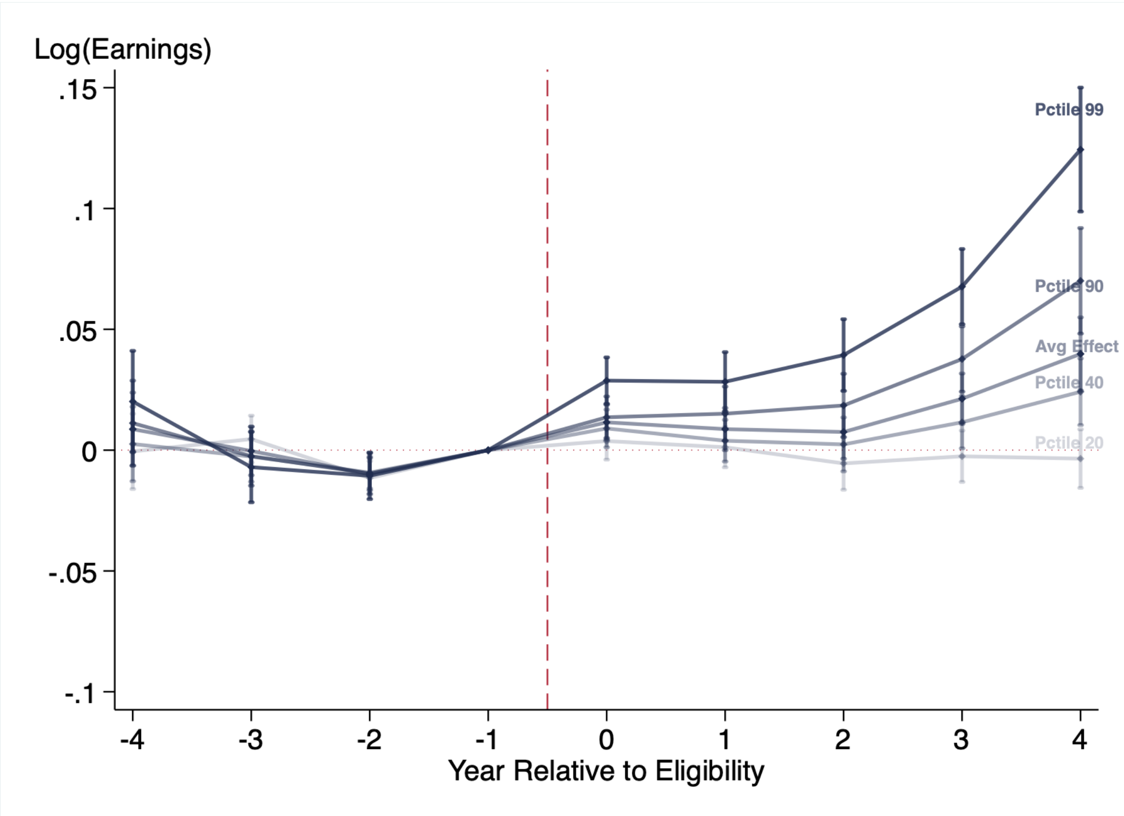
Profits. Relying solely on employment responses requires the use of structural assumptions to map input choices to profit outcomes - an approach often employed in the tax incidence literature (e.g., Suárez Serrato and Zidar 2016a; Suarez Serrato and Zidar 2023). The richness of the data used in this study enables direct observation of the tax cut captured by firm owners in the form of accounting profits. Considering that profits can be negative or zero, I chose not to use logarithmic transformations for this specific outcome. Instead, I conduct the analysis in levels, dividing the point estimates by the average profit in the years before the reform.[14]

Column (6) of Table 1 shows that the reform resulted in a 30% increase in profits, indicating that firm owners are able to capture part of the tax cuts as increased profits. These results remain robust when using the inverse hyperbolic sine transformation. Figure *A*.2 further supports the parallel trends assumption, demonstrating that the pre-reform coefficients are not statistically significant, thereby validating our empirical design.

Firm-level Heterogeneity. The comprehensive data enables an evaluation of firm size heterogeneity across all margins of response. Column 1 of Table 1 starts by demonstrating that heterogeneous responses are not mechanically driven by differences in the first stage. The reform impacts both small and large firms

[14] Analyzing data in levels is more sensitive to outliers, prompting me to apply a standard winsorization procedure at the 5% and 95% levels to reduce this sensitivity (Yagan 2015; Kline et al. 2019).

Figure 3: Earnings Effect Within Firm Wage Distribution



Note: This figure presents event study estimates for wages at different percentiles of the within-firm wage distribution. The event is the year in which the firm enters treatment for the first time. I normalize results with respect to one year prior to the event. Standard errors are conservatively clustered at 5-digit industry-by-state level.

Table 1: Firms' Margins of Adjustment

	(1) Log Labor Cost $(1+\tau)$	(2) Log Employment	(3) Log Capital	(4) Log Earnings	(5) Log Revenue	(6) Ebit (β/μ)
Baseline	-.1247***	.1279***	-.0337	.0309***	.0495	.3037*
	(.0059)	(.0311)	(.0496)	(.0094)	(.0371)	(.1633)
Small Firms *(Below median)*	-.1275***	.3461***	.0085	.0602**	.1864**	.5632
	(.0224)	(.0587)	(.1133)	(.0252)	(.0875)	(.797)
Large Firms *(Above median)*	-.1253***	.0969***	-.0312	.0228***	.0223	.2988*
	(.0047)	(.0352)	(.0549)	(.0086)	(.0391)	(.1559)
Controls	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Sector x Year FE	✓	✓	✓	✓	✓	✓
N	449,679	450,387	345,217	450,387	374,774	265,889

Note: This table reports long-difference coefficients from t=3 in the IV specification. Each column reports different margins of adjustment, such as labor cost, employment, capital, earnings, gross revenue, and profits. Results are presented for the baseline sample and separately per firm size, which is defined with respect to the median in the pre-reform period. Standard errors are reported in parentheses.

equally in terms of labor cost variations due to the payroll tax rate reduction. As discussed in Section 5, the empirically observed responses may indicate that small firms possess less market power in both product and labor markets, corroborating previous research that identified a monotonic relationship between firm size and labor market power (Yeh et al. 2022). Consistent with the notion that small firms wield less market power, they exhibit a greater ability to expand employment without significantly influencing prices in the product market or wages in the labor market.

Table 1 reveals that employment increases by 35% (s.e. 0.05) for small firms, compared to only 10% (s.e. 0.03) for large firms. Regarding earnings-per-worker, market power may operate through two conflicting channels. On one hand, greater labor market power boosts the effects on earnings-per-worker and reduces employment effects due to a steeper labor supply curve. On the other hand, more product market power restricts both employment and earnings-per-worker responses due to less scale response, reflected in a more modest shift in product supply and labor demand curves. Section 4 formalizes this intuition. Column (4) of Table 1 reports that earnings-per-worker respond more prominently in firms estimated to have less market power, supporting the interpretation that price-setting power curtails the scale response and, consequently, the earnings pass-through of tax policy.

Our measure of market power is based on empirical responses stemming from firm-level variation in payroll tax rates. An alternative measure involves relying on labor market concentration, observable through comprehensive labor data. This alternative approach, however, has several limitations. First, as Manning 2021 notes, there is controversy regarding the assumption that market concentration is an appropriate proxy for market power. For example, according to Burdett and Mortensen 1998's framework, increased competition facilitates worker mobility from low- to high-wage firms, potentially raising market concentration. Second, the concept of market concentration depends heavily on a subjective definition of market boundaries.

Despite these concerns regarding the use of market concentration to base the heterogeneity analysis, I further leverage the detailed nature of the labor data to define granular local labor markets[15], and examine responses within these markets. Figure H.5 illustrates that, in line with the firm size heterogeneity analysis, both employment and earnings-per-worker effects are more pronounced in firms with lower market concentration. Overall, these analyses support the conclusion that firms with less market power are more labor responsive to payroll tax cuts.

Column (3) shows that capital decreases more prominently in firms estimated to have greater market power. This pattern is consistent with the explanation that market power may constrain scale responses. When firms expand operations following a tax cut, they increase demand for both labor and capital. The limited scale response observed by larger firms coupled with capital-labor substitution is in line with the 3.1% (s.e. 0.05) capital decrease. In contrast, small firms may be able to offset the substitution effect with a scale response. This hypothesis aligns with the findings in column (5), where less powerful firms not only scale up more substantially but also do not drive up prices, resulting in stronger revenue responses to the tax cut.

3.3.1 Informality

Given the identified causal employment response to the tax cut, it is worth considering whether the increase in employment is due to the formalization of existing employees or the addition of new ones. I present several pieces of evidence that informality is not driving the results.

Transition. The panel structure of the data allows me to track previous employment spells for workers who held formal jobs in the past. Essentially, the data

[15] Local labor markets are defined at the commuting zone x 2-digits occupation level, based on job switcher patterns (Felix 2021).

enable me to determine for each new hire whether they transitioned from non-employment/informality or another formal job. If the positive employment effect were due to hiring existing informal workers, we would expect to see a sharp increase in the proportion of new hires transitioning from non-employment or informality in treated firms after the reform. However, as Figure H.3 indicates, the proportion of new hires coming from non-employment and informality remains constant over time and across treatment status, which suggests that formalization is not a significant margin of response.

Regional Variation. Another approach to the informality question is to leverage regional variation in informality rates. Brazil's large and diverse developing economy has local labor markets that range from those resembling developed economies to those similar to African countries. Two years before the payroll tax reform, the Brazilian Census Bureau conducted a national Census survey that provided rich regional informality data at the municipality-level. As Figure H.4 shows, there is a wide range of informality rates across Brazil's 5,300 municipalities.

I exploit this variation to distinguish the effects of a payroll tax reform in settings with different degrees of exposure to informality. I divide regions into two groups: those below and above the median in terms of formalization rate. If the main employment response to the tax cut was driven by the mere formalization of informal workers, it would be reasonable to expect larger employment effects in regions with high informality. However, my findings indicate the opposite (Table A.3). One might still be concerned that the labor cost variation induced by the policy in low- and high-informality areas can be different. I show that the first stage is uniform across informality status, which reinforces that formalization is not driving the results.

Workers' Education and Capital Response. As Ulyssea 2018b notes, informal employment is concentrated among firms with lower average education. The labor data provide information on workers' educational level, which enables me to compute average education per firm. I show that responses are concentrated in firms with higher shares of qualified workers i.e., firms less likely to hire informally. This serves as additional evidence that the employment effect is not driven by informality (Table A.3). Finally, if the observed employment effect resulted merely from informality, it would represent a nominal shift with no substantial economic consequence. Yet, as highlighted in Section 3.3, the reform prompts a substitution from capital to labor, which suggests that employment responses are real.

Discussion. The empirical evidence indicating that informality is not a major factor in employment responses is consistent with several factors highlighted in prior research. First, informality in Brazil is primarily driven by self-employment rather than informal employment in a formal firm (PNAD, 2012). This implies that informal workers are more similar to entrepreneurs than employees, and their formalization decision is more sensitive to other fixed costs such as licenses to operate, costs related to opening and maintaining a firm, other corporate taxes, legal liabilities, sanitary and security regulations (Maloney 2004). Second, even though there is a reduction in labor cost, the worker's decision to formalize extends beyond a simple cost-benefit analysis (see Perry 2007 for discussion). Compared to other worker-level policies like unemployment insurance (UI), which enables workers to fully capture gains by fleeing to informal markets, payroll taxes present a different scenario. As discussed in Section 6, only a small share of payroll taxes are paid by employees. This reduced burden lessens workers' incentives to change informality decisions based on payroll tax variation.

3.3.2 Liquidity Constraints and Composition

The observed heterogeneity in firm size aligns with previous studies (Bronzini and Iachini 2014; Howell 2017; Zwick and Mahon 2017; Criscuolo et al. 2019; Saez et al. 2019), which have shown that tax subsidies often elicit stronger employment responses from smaller firms across various contexts. One view is that the payroll tax cut serves to alleviate the financial constraints of the firm, subsequently leading to an increase in employment. To explore this hypothesis, I leverage the rich data to investigate the liquidity channel but found no evidence suggesting that liquidity constraints significantly drive the firm-level heterogeneity observed in employment responses. In the firm-level anonymized balance sheet data, I observe both short-term assets (e.g. cash) and short-term liabilities (e.g. short-term bills). I use the ratio of them to proxy for financial constraint. The firms were divided into groups based on whether they fell below or above the median liquidity constraint prior to the reform. The employment effects observed for both groups were strikingly similar, as shown in Table A.4.

It is true that firms within the "Real Profit" regime, for which we have access to balance sheet information, are less likely to be financially constrained. However, even when focusing solely on this subset of firms we observe a similar pattern to the full labor sample: firms below the median are substantially more responsive than those above the median (Table 1). These findings indicate that liquidity constraints may not be the primary factors driving the heterogeneity in employment responses at the firm level.

Composition. Interpretation of the earnings results in terms of pass-through could be compromised if, as a result of the policy, firms change the composition of their labor force. I conduct extensive investigation that provides several pieces of evidence that the employment responses do not induce changes in the labor force composition at the firm-level. First, I leverage the panel structure of the data to fit equations 3 and 4 using workforce characteristics as an outcome. Table 2 demonstrates that the tax reform does not significantly impact the composition of employed workers across various dimensions. The only exception is gender, where the reform induces a marginal but statistically significant effect of 1 p.p in the share of male workers, which is an economically irrelevant effect given the baseline share of 60% (column 3). Columns (1) and (2) show that the effects on the share of workers with high school and college degrees are indistinguishable from zero. Columns (4) and (5) present evidence that the reform did not affect the share of employed white workers or the average employees' age.

Columns (6) and (7) follow the composition analysis of Kline et al. 2019. Column (6) investigates whether the reform influences firms to hire workers from different parts of the earnings distribution and finds no effect on the quality of new hires proxied by their pre-hiring earnings. Column (7) reports the impact on a quality index, computed using a Mincer regression of log earnings on a quartic in age fully interacted with gender and race, estimated annually with firm fixed effects as additional controls. Taken all together, Table 2 suggests no evidence of skill upgrading in response to the reform.

Second, I explore whether the tax cut affects the types of occupations firms employ. I exploit the detailed CBO occupational codes[16], which contain 2,300 occupations. After ranking occupations based on pre-reform earnings and grouping them into percentiles, I determine each firm's average occupation percentile. Table A.5 reveals a sharp zero effect of the reform on firms' average occupation percentile. This empirical fact implies that the tax reform expands employment within, rather than between, occupations. This underscores the fact that there is no major shift in worker composition or technology-induced labor demand. Third, I find that the effects on earnings-per-worker are consistent whether estimated at the firm level or the worker level. This consistency is reassuring that the earnings effect is not driven by changes in the composition.

[16]Classificação Brasileira de Ocupação (CBO) is the legal norm for classifying occupations in the Brazilian labor market. It was established on decree approval 397/2002.

Table 2: Effect on Labor Composition

	(1) Share High School +	(2) Share College +	(3) Share Male	(4) Share White	(5) Avg Worker's Age	(6) Log Earnings New Hires (bf hired)	(7) Log Quality
Post × Treatment	.0091	.0099	.0132***	.0005	-.1343	-.0014	-.0005
	(.0085)	(.0061)	(.0034)	(.0045)	(.1497)	(.0116)	(.005)
Mean	.52	.11	.59	.67	39.72	7	1
Firm FE	✓	✓	✓	✓	✓	✓	✓
Sector x Year FE	✓	✓	✓	✓	✓	✓	✓
# Clusters	7,925	7,925	7,930	7,924	7,930	6,924	7,561
N	2,494,842	2,494,842	2,521,030	2,491,523	2,521,030	604,988	1,739,827

Note: This table reports difference-in-differences coefficients to assess the effect of the reform on the firm's labor composition. The empirical specification is the same as presented in equations 3 and 4. The regression is estimated in the balanced sample of firms to isolate any noise due to firm entry and exit. The goal is to depict the firm-level compositional effect. Column (6) reports the effects on new hires' previous earnings. Column (7) depicts the effects on a measure for worker's quality based on a Mincer regression of log earnings on a quartic in age fully interacted with gender and race, estimated annually. Standard errors are reported in parentheses.

3.4 Worker-level Analysis

An alternative approach to evaluating earnings pass-through involves tracking workers as opposed to firms, a strategy that offers two main advantages. First, it enables the assessment of whether firm-level earnings response is the result of pass-through or shifts in labor force composition. A zero wage pass-through could be consistent with positive firm-level earnings response in the instances of upscaling the labor force. Second, it yields insights into how tax variation impacts workers' career paths, particularly for various types of workers. As described in Section 2.2, the worker-level sample focuses on stable workers to minimize the interference of turn over. Table H.3 confirms that these workers are likely to stay within the same firm. Their probability of changing jobs is 7%, and during the 10 years of sample they are observed for 8.31 years on average. To conduct the worker-level analysis, I fit the empirical specification outlined in Section 3.1 to the worker sample, which includes worker fixed effects.

Earnings-per-worker. Consistent with the positive earnings response measured at the firm-level, Figure 4 reveals that workers' take-home payments increased by 2%. The effect intensifies to 3% three years after the tax cut. This result reinforces the notion that the positive earnings response is rationalized by pass-through rather than compositional changes. I also show the first stage version of the figure, using gross earnings as an outcome. Figure A.4 depicts a pronounced drop in the gross earnings paid by firms, which is mostly attributed to the mechanical reduction in payroll tax rates.

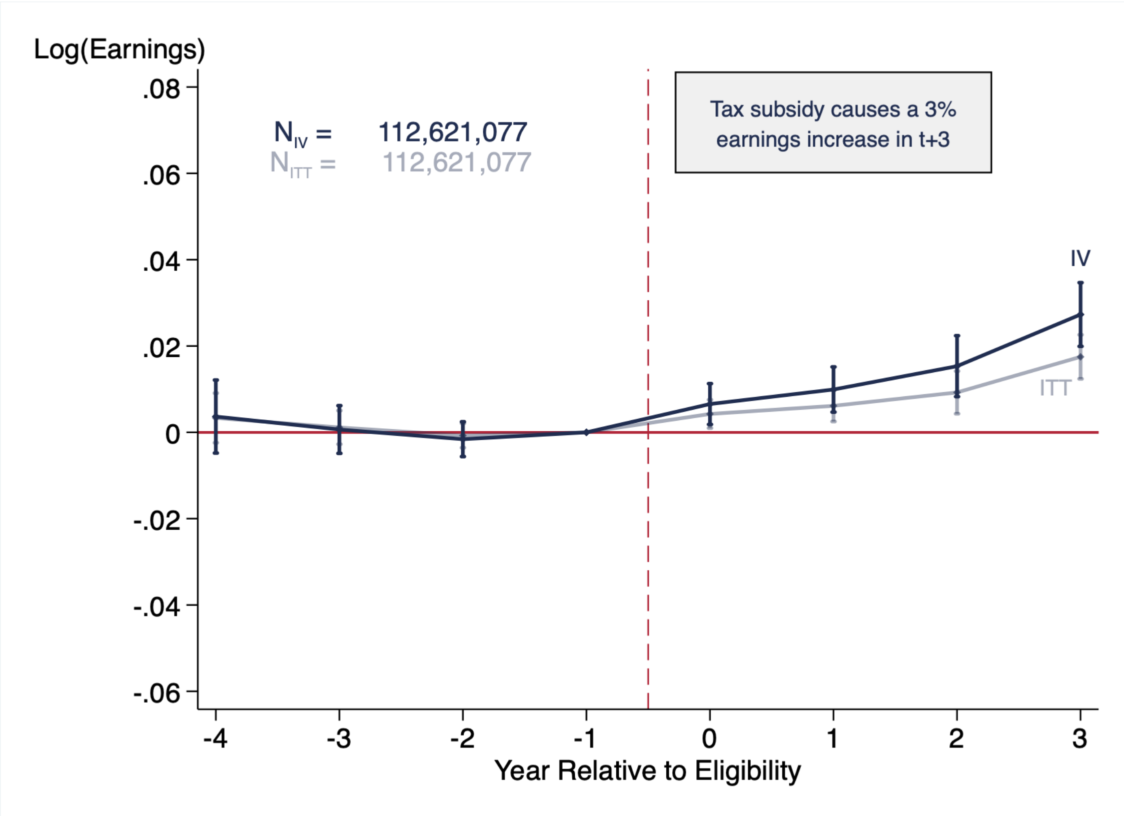
Occupation. Similarly to the earnings inequality result found within firms, I show that workers in high-skill and managing positions benefit relatively more from the reform. To implement this analysis, I rely on the CBO to split employees into two occupation groups. Managers, directors, and qualified technical positions are in the top bucket and comprise 15% of the sample, and the remaining 85% of lower positions are evaluated separately. Figure A.5 shows that the pass-through to highly skilled workers is 6%, and is almost zero to low-skilled workers. Appendix F provides an extension of the model that includes two types of labor and is able to rationalize the findings in a setting in which low-skill workers have higher labor supply elasticity. One way to interpret this finding is that low-skill labor markets operate closer to perfect competition, while the local labor market for high-skill labor is more concentrated among fewer employers.

Racial Wage Gap. The payroll tax program does not distinguish workers based on background characteristics such as race. It offers a flat 20 p.p. cut that remains constant across all income levels, which suggests no explicit intention to disadvantage workers of a specific race. However, if race correlates with occupation or other factors that determine unequal pass-through, the tax system can inadvertently widen the racial wage gap. A unique feature of the Brazilian data is its ability to identify workers and their race. I use the policy-induced tax variation to find that white workers benefit significantly more from the reform than non-white workers. This intriguing result holds even after controlling for firm fixed effects, which suggests that the racially unequal pass-through is not attributed to firm sorting. Furthermore, the result remains true even after controlling for education and worker fixed effect, indicating that racial disparities in tax pass-through are not driven by other worker-specific observable, such as education. I also evaluated heterogeneous pass-through according to gender but found zero statistical difference. Figure A.6 summarizes the analysis across different types worker-level heterogeneity.

Unintended Discrimination. This paper introduces another critical aspect to the public debate. Despite the fact that racial discrimination is a pressing social issue in modern society, it has not been incorporated in the tax literature.[17] This might be because modern tax codes do not contain any explicit elements

[17] A few exceptions are Brown 2022 and Holtzblatt et al. 2023, who study racial inequality in the context of couples' taxation in the US.

Figure 4: Event Study Estimates of Workers' Earnings



Note: This figure presents event study estimates for average earnings (net of payroll taxes) for stable workers. I normalize results with respect to one year before the treatment event. The analysis spans four years before the payroll tax cut program and three years after. Blue markers report the IV estimates and gray markers are the intention-to-treat. Standard errors are conservatively clustered at 5-digit industry-by-state level.

of racial discrimination that could be classified as either statistical or taste-based discrimination. However, taxes can exacerbate racial inequality through indirect channels that are substantiated in existing frictions. This paper provides novel evidence that behavioral responses to tax changes can lead to unintended consequences for racial inequality.

3.5 Validity of the Empirical Design

The identifying assumption is that eligibility, conditional on fixed effects, is uncorrelated with time-varying, unobserved factors that influence employment and wage growth. The validity of this assumption would be violated if Congress anticipated sector-specific trends in its definition of eligibility rules. Another issue could arise if firms strategically chose sectors after the reform was announced. In this section, I conduct multiple tests to address both of these concerns related to selection on eligibility and sector choice manipulation.

3.5.1 Selection on Eligibility

Trends. The concern about selection on eligibility is whether firms that were granted eligibility status might have exhibited different trends relative to those that weren't. To address this, it's common practice to evaluate pre-existing trends. Figures 2 and 4 depict event study coefficients that reflect reassuring pre-reform results that are not statistically different from zero. This suggests that in the absence of the tax reform, the outcomes for both eligible and ineligible firms (and workers) would have followed parallel trends in the post-period if the tax reform had not been enacted.

Baseline Levels. Besides parallel trends, firms across various eligibility statuses also demonstrated a balance in levels among several characteristics in the pre-reform period. This is not surprising per se, as we saw that eligible and non-eligible sectors are fairly similar, as evidenced by the example of hotels and motels. The one characteristic that did not exhibit a balanced distribution across groups was gender, a variable that I will control for in all specifications. Important to highlight that even though the two groups present balanced baseline levels, this is not required for identification. The empirical strategy will not require random treatment assignment, but rather that the two groups would have evolved similarly had the reform not happened.

Alternative Identification. The reason specific sectors were chosen was not disclosure, nor was there an objective criterion to determine eligibility. From an econometric standpoint and with respect to potential identification concerns, it has been established that sector choice did not seem to anticipate sector trends. To further investigate underlying criteria that determined eligibility, I fit a logit model on baseline firms' observable characteristics. I then use propensity scores to break ties in a procedure, which matches firms based on pre-reform deciles on average employment, workers' earnings, firm age, net revenue, and profits. Using this matched sample, I conducted a difference-in-differences analysis as a robustness check. Interestingly, in this alternative empirical strategy, the identification assumption hinges on the Conditional Independence Assumption (CIA), which is validated by the balance tables in Appendix G.2. Importantly, this strategy does not make any assumptions about the political process that determines eligibility.

Results from both the primary empirical strategy and the alternative matching approach are qualitatively similar. Detailed analysis and the corresponding results are provided in Appendix G.2. To further substantiate the matching approach, I conducted additional robustness checks. I randomly assigned a placebo treatment and applied the same matching process to these placebo-treated firms. As anticipated, the placebo tests yielded zero effects on employment and wages, and thereby providing evidence that the results are not influenced by any inconsistencies in the matching algorithm.

3.5.2 Manipulation on Sectoral Choice

Sector Immobility. Given the seemingly arbitrary nature of eligibility assignment, one might wonder whether firms could manipulate their sector classification to move to eligible sectors after the announcement of the reform. In this scenario, the concern is that firms that expect employment growth could self-select into treatment, and thereby compromise the causal interpretation. Fortunately, our panel data allow us to track firms and assess whether they changed sectors upon the reform's implementation. The data show only a small number of firms changing sectors, and among these there is no trend of switching to eligible sectors. This low manipulation response aligns with the bureaucratic challenges of changing sectors.

Bureaucratic Process. Firms in regular tax tiers, the focus of this study, encounter a lengthy and costly process to change sectors. Initially, they must amend their operating agreement, demonstrating that they have shifted their core activities and are functioning in a new industry. Subsequently, they need to secure new operational licenses from various administrative bodies, including city, state, and federal tax authorities. Additionally, they must obtain clearance from local tax authorities and civil registry offices. Any missteps during this process can lead to sanctions and fines.

Additional Robustness Tests. To further ensure that sector changes are not driving the results, I conducted several additional robustness checks. First, I assigned firms to eligibility based on their pre-reform sectors, and results remained qualitatively the same. Similarly, when I restricted the sample to firms that never changed sectors, results were unchanged. Taken together, these tests indicate that sector manipulation is not an active margin of response, which reinforces the causal interpretation of the results.

4 Model

The empirical evidence provided so far, particularly in Table 1, emphasizes the importance of the product market in shaping responses to payroll taxation. The presence of imperfect product competition allows the transmission of cost shocks to consumers, a phenomenon the payroll tax literature has not yet studied. In its original form, the Marshall-Hicks framework acknowledges that firms can set prices above marginal cost. However, it assumes that labor markets operate in perfect competition, which is in stark contrast to the positive earnings effect documented in this paper.

In this section, I extend the conventional pass-through framework (Criscuolo et al. 2019; Harasztosi and Lindner 2019) to incorporate imperfect competition in both product and labor markets. By combining these two competitive frictions, which are often modeled separately, I can interpret the empirical findings. The actual degree of market power in the economy is an empirical question uncovered by firms' response to the tax shock. The model yields key identifying equations that directly connect the reduced-form estimates to structural parameters of easy interpretation. Using the combination of model and data, this section quantify mechanisms of response and measures of tax incidence and efficiency.

4.1 Setup

Motivated by the firm-specific nature of the reform studied in this paper, the model considers a partial equilibrium framework in which firms operate as monopolists in the product market and monopsonists in the labor market. The model has a single period, in which firms choose their input mix and output level. After selling production, the firm concludes its operations. Firms are endowed with a CES technology with constant returns, which uses capital and labor as inputs.

$$f(L,K) = (s_L L^{\rho} + s_K K^{\rho})^{\frac{1}{\rho}}$$

where the aggregate L is the total efficiency units of labor at the firm and s_g are the inputs' cost share ($g \in \{L, K\}$). The capital market operates in perfect competition, which means that the marginal revenue product of capital equals its cost. However, the labor market operates in imperfect competition, and labor supply elasticity ϵ dictates the firm's ability to mark wages below the marginal revenue product of labor. Firms face an upward-sloping labor supply curve, and cannot discriminate wages across incumbents and new hires.

$$w_j = A_j L_j^{\frac{1}{\epsilon}}$$

The wage-setting rule suggests that if wages rise due to a firm-specific shock, both incumbents and new hires experience equal benefits — an observation supported by the data. From a theoretical standpoint, the static labor supply curve can be micro-founded by an analogy to Industrial Organization's discrete choice models, which are employed to estimate demand with differentiated goods. In the labor market context, the "differentiation" arises from workers' preference for particular employers. This argument is formalized in Appendix C. As in Card et al. 2018; and Haanwinckel 2023, I assume that firms ignore their contribution to the tightness of the labor market — an approximation that is appropriate when firms have small market share.

The output market operates in monopolistic competition, with firms determining quantity based on a constant price elasticity denoted as η (Hamermesh 1996; Criscuolo et al. 2019). Specifically, firms face the inverse product demand described by $P_j = Q_j^{\frac{-1}{\eta}}$. The subscript j indexes a specific firm, but for ease of notation this subscript will be omitted in the rest of the paper. The degree of monopolistic power is dictated by the parameter η, which is flexible to accommodate any market structure, including perfect competition. Given the output choice, firms solve a cost minimization problem to decide on the input mix. The Government can manipulate labor cost $(1+\tau)$ through perturbations in the payroll tax rate (τ). The percentage variation in labor cost induced by the Brazilian policy is denoted by ϕ_1.

4.2 Firm's Problem

Profit Maximization The firm chooses output to maximize profits, according to the following program:

$$\max_{Q} \underbrace{Q^{1-\frac{1}{\eta}}}_{\text{Revenue}} - \underbrace{A(1+\tau)L^{1+\frac{1}{\epsilon}} - rK}_{\text{Cost, C(Q)}}$$

At the optimum, firms choose quantity that equates marginal cost with marginal revenue:

$$\underbrace{\left(\frac{\eta-1}{\eta}\right)Q^{\frac{-1}{\eta}}}_{\text{Mg Revenue}} = \underbrace{\frac{\partial C(\tau, Q)}{\partial Q}}_{\text{Mg Cost}} \tag{5}$$

In contrast to a perfectly competitive environment, the marginal cost is no longer a linear function of the output level (see proof of Lemma 1, in Appendix C). The intuition is that there is an increasing cost to expand plant size due to inframarginal wages. As a result, imperfect labor competition limits the pass-through to employment. Mathematically, equation (5) reveals how output level influences labor demand by raising the marginal cost of scale expansion. This relationship is increasing in the firm's market power (decreasing in η). The employment effect is further determined in the cost minimization program, which I turn to next.

Cost Minimization Once the output quantity is fixed, firms decide on the input mix that minimizes cost. Formally,

$$\begin{aligned} \min_{K,L} \quad & A(1+\tau)L^{\frac{1}{\epsilon}+1} + rK \\ \text{s.t.} \quad & f(K,L) \geq Q \end{aligned}$$

At the optimum, the labor choice equates the marginal cost of labor to the marginal revenue product of labor:

$$\underbrace{\overbrace{\left(\frac{\epsilon+1}{\epsilon}\right)}^{\text{inverse mark down}} A(1+\tau)L^{\frac{1}{\epsilon}}}_{\text{MCL}} = \underbrace{\lambda f^{1-\rho} s_L L^{\rho-1}}_{\text{MRPL}}$$

Note that the marginal cost of labor is decreasing in the level of labor market competition, which guides the steepness of the labor supply curve. Putting together the optimal input choice and applying the envelope theorem, I can compute the cost function. The monopsony power in the labor market breaks the linear relationship between average and marginal cost:

$$\underbrace{\frac{\partial C}{\partial Q}}_{\text{Mg Cost}} = \underbrace{\frac{C}{Q}}_{\text{Avg Cost}} + \overbrace{\underbrace{\frac{C_L}{\epsilon}\frac{1}{Q}}_{\text{Avg Incumbent Rent}}}^{\text{Monopsony}}$$

I denote this new term as the average incumbent's rent because it is related to the wage increase perceived by inframarginal workers when the firm increases plant size. In particular, the rent converges to zero as we move to perfect competition ($\epsilon \to \infty$), similar to traditional models (Hamermesh 1996). The nonlinearity in the cost function will be key to understanding pass-through responses to payroll tax reforms.

4.3 Pass-Through

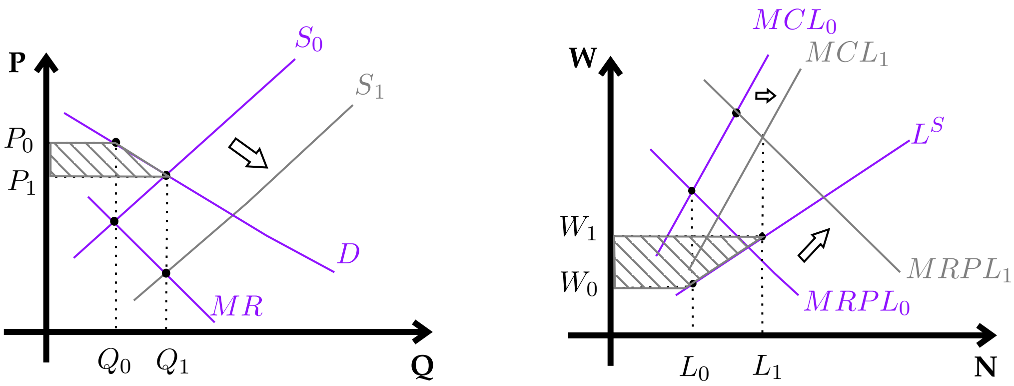
Thus far, I have presented the framework for firms' decisions in both output and input markets. This section develops intuition on the interaction between these decisions and the policy-induced tax variation. In particular, this section sheds light on the role of market power in shaping the pass-through, which ultimately drives the incidence and efficiency of the payroll tax system. To comprehensively address all elements of the Brazilian tax reform, I have also considered the variation in revenue tax. Appendix C.3 utilizes the model to examine the impact of the revenue tax, which appears to have limited effects due to both the minor variation in the tax rate and the small proportion of firms affected by the reform. Given the negligible impact of the revenue tax, I will exclude it from the main text to streamline the discussion.

4.3.1 Output Market

In the output market, a payroll tax reduction shifts the supply of goods. The consequences for output depend on two factors: (i) the behavioral response, which determines the magnitude of the shift in product supply/ labor demand, and (ii) the slope of the demand curve. Figure 5 illustrates how the price effect increases with market power. To quantify this effect, I totally differentiate the pass-through equations to compute price elasticity with respect to labor cost:

$$\epsilon^P_{1+\tau} = \frac{-1}{\eta}\epsilon^Q_{1+\tau}$$

Figure 5: Conceptual Framework



(a) Product Market (b) Labor Market

Note: This figure illustrates pass-through in the product and labor markets from a firm-specific payroll tax cut. The left graph shows the intuition for the case of product monopolistic competition. Compared with the perfectly competitive case, there is a smaller quantity (or scale) effect due to the price-setting power. On the right, the graph depicts the intuition for the monopsonistic case. In this framework, the employment effect is not as large as in perfect labor competition, but as the tax reform expands the labor demand, it provokes a wage increase.

The price elasticity depends on the scale response, and product market power, which is determined by the constant elasticity η. Due to monopsony power in the labor market, the scale effect cannot be evaluated solely on the basis of the product market. Remember that now the inframarginal rent affects the plant size decision. At the optimum, the effects of tax policy on marginal revenue should equal the effects on marginal cost:

$$\underbrace{-\frac{1}{\eta}\epsilon^{Q}_{1+\tau}}_{\substack{\text{Effect on mg revenue} \\ (\eta \text{ drives slope of D})}} = \underbrace{\overbrace{\epsilon^{\lambda}_{1+\tau}}^{\text{Direct}} + \overbrace{\epsilon^{\lambda}_{Q}\epsilon^{Q}_{1+\tau}}^{\text{Inframarginal}}}_{\substack{\text{Effect on mg cost} \\ \text{(shift in supply curve)}}} \tag{6}$$

Equation (6) sheds light on two mechanisms through which imperfect competition affects firm responses. First, competition in the output market flattens the demand curve ($\frac{-1}{\eta}$), which enhances the scale effect. Second, competition increases the pass-through to the marginal cost, which amplifyies the shift in the supply curve and thereby the scale effect. It is important to highlight the fact that the elasticities expressed in equation (6) are endogenous to the tax system. Appendix C further develops this formula to establish a closed-form solution for the pass-through as a function of primitives, which are expressed in equations (10)-(13).

4.3.2 Labor Market

Labor market forces determine the tax pass-through according to the effects on the marginal cost of labor and marginal revenue product of labor. Figure 5 provides intuition on the interaction between imperfect labor competition and a firm-specific shock. Equations (7) and (8) quantify the elasticity of the marginal cost of labor and the elasticity of the marginal revenue product of labor with respect to labor cost in the case of monopsonistic labor markets.

$$\frac{\partial \log MCL}{\partial \log(1+\tau)} = \overbrace{1}^{\text{Direct effect on MCL}} + \overbrace{\frac{\epsilon^L_{1+\tau}}{\epsilon}}^{\text{Inframarginal on MCL}} \tag{7}$$

$$\frac{\partial \log MRPL}{\partial \log(1+\tau)} = \overbrace{\epsilon^{\lambda}_{1+\tau} + \epsilon^{\lambda}_{Q}\epsilon^{Q}_{1+\tau}}^{\text{Direct + inframarginal on mg rev}} + \overbrace{(1-\rho)(\epsilon^{Q}_{1+\tau} - \epsilon^{L}_{1+\tau})}^{\text{Effect on MPL}} \tag{8}$$

Marginal Cost of Labor. The pass-through to the marginal cost of labor (MCL) is comprised of two components. As in a perfectly competitive labor market, the first component perfectly correlates MCL with variations in labor cost. The second component is unique to monopsonistic firms and can be decomposed into two channels: (i) the behavioral response, which governs the shift in the marginal cost of labor, and (ii) the slope of the marginal cost of labor. Market power affects these two channels in opposite directions. While it dampens behavioral responses, it amplifies the steepness of the marginal cost of labor.

Marginal Revenue Product of Labor. As equation (8) suggests, the effect of the tax policy on the marginal revenue product of labor depends on the pass-through to the marginal product of labor (MPL) and marginal revenue. Pass-through to the marginal product of labor is negatively related to the substitution across inputs ($\sigma_{KL} = \frac{1}{1-\rho}$), positively related to the scale effect, and negatively related to the employment effect. Pass-through to marginal revenue depends on the direct and inframarginal effects of the firm-specific labor cost variation. Note that marginal revenue depends on the labor cost $(1+\tau)$ and the output level. Therefore, when the firm reacts to a labor cost reduction by increasing plant size, the scale effect inflates costs and offsets part of the initial cost reduction. Equation (9) relies on evelope arguments to quantify these responses, and the associated effect to the marginal cost.

$$\overbrace{\frac{\partial \lambda(Q,\tau)}{\partial(1+\tau)}}^{\text{Effect on mg cost}} = \overbrace{\frac{AL^{1+\frac{1}{\epsilon}}}{Q}}^{\text{Effect on avg cost}} + \overbrace{\frac{AL^{1+\frac{1}{\epsilon}}}{Q\epsilon}}^{\text{Direct effect on incumbent rent}} + \overbrace{\frac{A(1+\tau)}{\epsilon}\left(\frac{\epsilon+1}{\epsilon}\right)\frac{L^{\frac{1}{\epsilon}}}{Q}\frac{\partial L}{\partial(1+\tau)}}^{\text{Indirect effect on incumbent rent from L response}} \tag{9}$$

The effect of the interaction between labor market power and pass-through to marginal cost is unambiguous. The higher the market power, the higher the direct pass-through to the incumbent's rent; it also amplifies the indirect effect on the incumbent's rent due to labor responses.

5 Structural Estimation

An advantage of empirically observing many margins of responses is that it allows me to evaluate the coherence of model's predictions among multiple margins. This section connects model and data to credibly estimate parameters of interest, and understand mechanisms of adjustment to a payroll tax cut such as scale and substitution.

5.1 Identification and Interpretation

To operationalize the structural estimation, I derive the model's predictions for the Brazilian payroll tax reform. These responses form a system of equations that depend on parameters: the labor supply elasticity faced by the firm (ϵ); capital-labor elasticity of substitution (σ); output demand elasiticity (η). I present direct connection between the structural parameters and reduced-form estimates.

Pass-through Formulae. Following the derivation outlined in Section 4 (and detailed in Appendix C), I compute closed-form solutions for the tax pass-through to employment, capital, earnings, and revenue. To embrace all elements of the Brazilian tax reform, I also take into account the revenue tax variation, which turns out to have muted effects due to the small rate variation on the revenue side and the small share of firms subject to this tax.[18] Once I account for product and labor market power, the effects of the Brazilian tax reform on employment, capital, revenue, and earnings can be expressed as a function of observables and the three parameters to be estimated (ϵ, η, ρ):

[18]Since the revenue tax has negligible effects, I will omit them in the main text. Careful derivation of the revenue tax perturbation can be found in Appendix C.3.

$$\beta_L = \left(\frac{\epsilon\sigma}{\sigma+\epsilon}\right)\left[\left(\frac{(\epsilon+2\epsilon^L_{1+\tau})(\sigma-\eta)}{\sigma\epsilon}\right)\left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)-1\right]\phi_1 \tag{10}$$

$$\beta_K = \left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+2\epsilon^L_{1+\tau}}{\epsilon}\right)\left(\sigma-\eta\right)\phi_1 \tag{11}$$

$$\beta_{Rev} = (1-\eta)\left[\left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{\epsilon+2\epsilon^L_{1+\tau}}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\right]\phi_1 \tag{12}$$

$$\beta_W = \frac{\epsilon^L_{1+\tau}}{\epsilon}\phi_1 \tag{13}$$

where s_L is the labor share, $\epsilon^L_{1+\tau}$ is the empirically estimated elasticity of employment with respect to the labor cost, and ϕ_1 measures the first stage associated with the policy — i.e., the percentage variation in tax rates induced by the reform. Using anonymized tax data, I precisely estimate ϕ_1. The pass-through formulae developed here are more general than the ones employed in recent studies that assume perfect labor competition. My framework can accommodate perfect labor competition as a particular case, in which ϵ goes to infinity. Taking the limit of pass-through equations (10)-(11), I recover the same expressions derived in a standard Marshall-Hicks analysis and estimated by Curtis et al. 2021; Criscuolo et al. 2019; and Harasztosi and Lindner 2019. In the standard competitive case, substitution and scale effects are separable, as illustrated below.

$$\lim_{\epsilon\to\infty}\beta_L = \Big(\underbrace{-s_K\sigma}_{\text{substitution}} - \underbrace{s_L\eta}_{\text{scale}}\Big)\phi_1 \qquad\qquad \lim_{\epsilon\to\infty}\beta_K = s_L\Big(\underbrace{\sigma}_{\text{substitution}} - \underbrace{\eta}_{\text{scale}}\Big)\phi_1$$

Identification. Manipulating the pass-through expressions (10-13), I find closed-form solutions for the structural parameters. Equation (14) directly maps the labor supply elasticity faced by the firm to the reduced-form elasticities estimated in the data. The intuition is that the ratio of the employment and earnings effect identifies the slope of the labor supply curve faced by firms:

$$\epsilon = \frac{\beta_L}{\beta_W} \tag{14}$$

From the capital and labor responses, σ_{KL} is identified:

$$\sigma = \frac{\beta_K-\beta_L}{\beta_W+\phi_1} \tag{15}$$

The parameter σ is derived from contrasting the capital and labor responses. Equation (15) depicts the intuition that as β_K decreases relative to β_L, this is an indication that firms are substituting capital for labor. Also, it is interesting to note that when β_W goes to zero, σ boils down to the standard expression from previous studies that assumed perfect labor market competition. Another angle to read equation (15) is that when β_W is non-zero, ignoring labor market power would generate a biased estimate for the capital-labor elasticity of substitution. Finally, I can identify the output demand elasticity using the capital and revenue responses:

$$\eta = \frac{\sigma\beta_R - \beta_K}{\beta_R - \beta_K} = \frac{-\beta_Q}{\beta_P} \tag{16}$$

The economics behind equation (16) is that the slope of the demand curve in the product market identifies η, which is equal to the ratio between scale and price responses to the tax reform.

Estimation Methods. I rely on the Classical Minimum Distance (CMD) approach to estimate structural parameters. The CMD minimizes the squared difference between the model and data, weighting it by the inverse variance-covariance matrix, $\hat{W}^{-1}$. Formally, the method solves $\min_\beta[\xi(\hat{\beta}) - \xi(\beta)]'\hat{W}^{-1}[\xi(\hat{\beta}) - \xi(\beta)]$, where $\xi(\beta)$ is the vector of model predictions = $[\epsilon^L_{1+\tau}, \epsilon^K_{1+\tau}, \epsilon^W_{1+\tau}, \epsilon^R_{1+\tau}]$, and $\xi(\hat{\beta})$ is the vector of reduced-form estimates = $[\hat{\epsilon}^L_{1+\tau}, \hat{\epsilon}^K_{1+\tau}, \hat{\epsilon}^W_{1+\tau}, \hat{\epsilon}^R_{1+\tau}]'$. Standard errors are computed based on a parametric bootstrap. I also compute these parameters using a seemingly unrelated regression (SUR) to jointly estimate equations (14)-(16). I use the Delta method to estimate standard errors for each structural parameter. Results from both approaches are similar (Table H.4).

Parameter Estimates. Table 3 presents estimates for the three parameters of interest. Column (1) presents baseline results for all firms, and columns (2) and (3) break the estimates down based on firm size. There are several reasons to break the estimates down this way. First, they are highly correlated with measures of market concentration we can observe, such as market share in the local labor market. Second, it is policy informative, given that firm size is a characteristic that is easy to target in policy design. Third, a large literature finds that small firms react more to industrial policies, which contributes to general interest in understanding small firms' behavior.

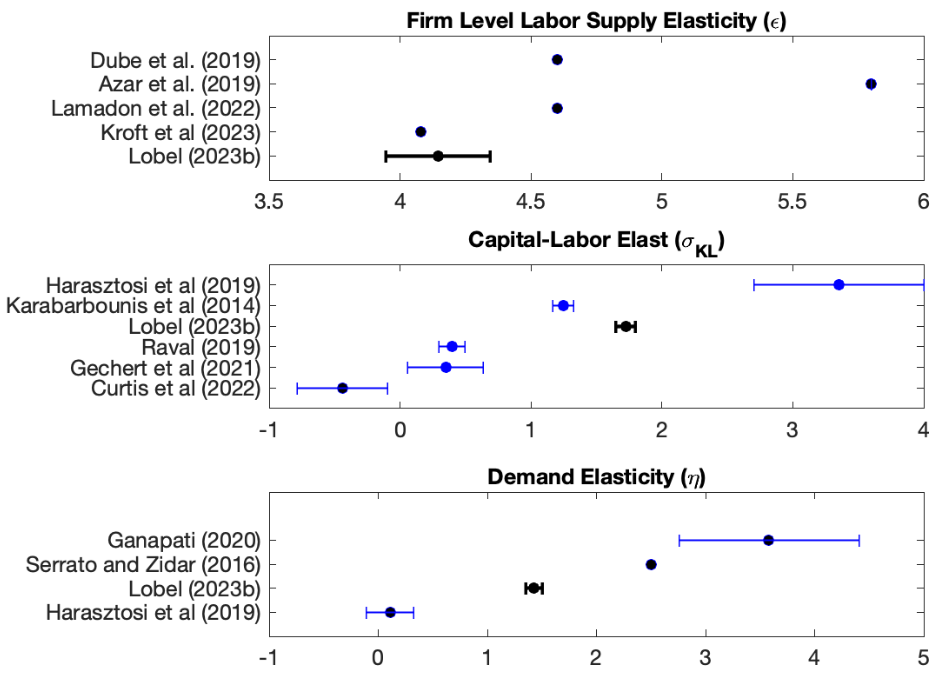
Elasticity of Substitution. The labor-capital elasticity of substitution ($\sigma_{KL} = \frac{1}{1-\rho}$) is equal to 1.72 (se 0.08) at baseline. This result is similar to that of Karabarbounis and Neiman 2014 and implies that capital and labor are substitutable, which supports the view that lowering the cost of capital may increase income inequality (Piketty and Zucman 2014).[19] Interestingly, I find that capital and labor are more substitute in small firms (5.01, se 0.34) than in large firms (1.25, se 0.08). This result is valuable, because most of the literature on capital-labor elasticities focuses on large firms in manufacturing. In contrast, my study encompasses a wide range of firm sizes and sectors. Greater substitutability identified in smaller firms can reconcile my estimates with recent literature, focused on large manufacturing firms, which finds that capital and labor are complementary.

Labor Supply. The labor supply elasticity faced by the firm ϵ is 4.15 (se 0.20), which is remarkably close to recent estimates: 4.08 (Kroft et al. 2020); 4.0 (Card et al. 2018); 4.6 (Lamadon et al. 2022). Figure 6 summarizes the literature and points out several studies that report labor supply elasticity between 3 and 5. My baseline estimate implies a wage markdown of 0.81 ($\mu = \frac{\epsilon}{1+\epsilon}$), which suggests that Brazilian firms capture 19% of the marginal revenue product of labor. Columns (2) and (3) report that the labor supply elasticity for small and large firms is 5.75 (se 0.33) and 4.25 (se 0.28), respectively. This result is consistent with the increasing and monotonic relationship between labor market power and firm size demonstrated by Yeh et al. 2022.

Demand Elasticity. Output demand elasticity with respect to price is 1.43 (se 0.07) a value greater than one, which aligns with the conventional notion that monopolies operate on the elastic side of the demand curve. If a firm independently decides to raise prices, the quantity loss outweighs the revenue gains from higher prices. Heterogeneous responses to the tax variation reveal that large firms have substantially more market power in the product market. The output demand elasticity for small and large firms is 5.21 (4.21) and 1.10 (se 0.06), respectively. These elasticities are key to examining the theoretical implications of market power on the scale response to a tax cut — a phenomenon that will play a central role in the subsequent discussion of underlying mechanisms.

[19] Other recent studies have found that capital and labor are complements (Raval 2019).

Figure 6: Literature Benchmark



Note: This figure places my estimates with respect to estimates in the literature. Parameters are outlined on the x-axis and their respective estimates are on the y-axis. The top panel refers to the labor supply elasticity faced by the firm. The middle panel reports the capital-labor elasticity of substitution. Finally, the bottom panel depicts the output elasticity with respect to price.

Overidentification Test. To assess the validity of the model, I compare the reduced-form estimates with their corresponding model predictions, based on equations (10)-(13) and the estimated structural parameters. Note that there are four moments and only three unknown parameters (ϵ, σ, η), which enables me to test for overidentifying restrictions. The p-value for the J-test, reported in the last row of Table 3, indicates that the restriction is not rejected. This finding provides evidence that the model fits the data well and is appropriately specified.

Cost Shares. I borrow cost share measures from existing literature, for several reasons. First, variations in cost shares appear to have minimal impact on structural estimates (Curtis et al. 2021). Second, there is a broad consensus in the literature regarding the levels of cost shares. Third, our data is particularly rich for the two primary input costs: capital and labor. Although there is substantial evidence suggesting that most firms' input adjustments are concentrated in these two inputs (Criscuolo et al. 2019; Harasztosi and Lindner 2019), it is possible that there are other cost items that affect levels of cost shares and we do not observe.

Mechanisms. We further utilize the data to explore whether the observed increase in employment is driven by scale effects or substitution responses. To this end, Equation 17 illustrates how the scale response can be identified using reduced-form coefficients and structural parameters estimated from the data. Appendix C provides a detailed derivation of this equation.

$$\beta_Q = -\left(\frac{\epsilon+1}{\epsilon}\right)\left(1+\frac{\epsilon^L_{1+\tau}}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\eta(\epsilon+2\epsilon^L_{1+\tau})}{\epsilon+\epsilon^L_{1+\tau}}\right)\phi_1 \tag{17}$$

In a perfectly competitive labor market ($\epsilon \to \infty$), this equation reduces to:

$$\lim_{\epsilon\to\infty}\beta_Q = -s_L\eta\phi_1 \tag{18}$$

Equation 18 aligns with standard expressions in the literature used to identify scale effects. The scale effect essentially reflects a uniform increase in the use of all inputs, without any substitution effects. Under constant returns to scale, the increase in the quantity sold is identical to the scale effect. Substitution, on the other hand, refers to the extent to which employment increases as a replacement for capital. Table 3 reports the scale effect using empirical estimates reported in Table 1. The analysis suggests that two thirds of the 12% employment boost is due to firms expanding production, while the remaining one third stems from capital-labor substitution. The scale response varies across different types of firms. As previously noted, the empirical responses from small firms suggest that they may have less market power in both labor and product markets.

Column (2) of Table 3 points out that for firms estimated to have less market power, the employment effect can be decomposed into 24% scale and 11% substitution away from capital. Column (3) reports that the empirically observed 9% employment boost for large firms comprises 6% scale and only 3% substitution. This result is aligned with the notion that firms' market power and their influence on prices can paradoxically limit their ability to scale up their plant size in response to tax relief. The interplay between scale and substitution has consequential implications for analyzing tax incidence and efficiency, which will be discussed in the subsequent section. A more pronounced scaling effect leads to larger price reductions, ultimately benefiting consumers. However, substitution without scaling does not translate tax cuts into consumer benefits but does create additional demand for labor, which can benefit workers.

Table 3: Structural Parameters and Mechanisms

Structural Estimates	(1) Baseline	(2) Small Firms	(3) Large Firms
Labor Supply Elasticity, ϵ	4.15	5.75	4.25
	(0.20)	(0.33)	(0.28)
Labor-Capital Elasticity, σ_{KL}	1.72	5.01	1.25
	(0.08)	(0.34)	(0.08)
Output Demand Elasticity, η	1.43	5.21	1.10
	(0.07)	(4.21)	(0.06)
Empirical Estimates			
Employment effect, β_L	0.12	0.35	0.09
Capital effect, β_K	-0.03	0.01	-0.03
Earnings effect, β_W	0.03	0.06	0.02
Revenue effect, β_R	0.05	0.18	0.02
Mechanisms			
Price effect, β_P	-0.06	-0.05	-0.06
Scale effect, β_Q	0.08	0.24	0.06
Scale / Employment, β_Q/β_L	0.65	0.70	0.64
Cost Shares			
Labor	0.80	0.80	0.80
Capital	0.20	0.20	0.20
J-test			
Overid test (pvalue)	0.74	0.17	0.79
Observations			
N	450,387	184,924	265,452

Notes: This table presents the parameters estimated, according to the method presented in Section 5.1. Column (1) reports results for the baseline case, which includes all firms. Columns (2) and (3) restrict the analysis to small and large firms, respectively. Firm size is measured in the pre-reform years, and small/ large are defined based on below/ above the median, respectively. In the "Mechanisms" section, the table reports effects on prices (β_P), scale (β_Q), and the share of employment effect that is explained by the scale response $\frac{\beta_Q}{\beta_L}$. In the empirical section, the table displays coefficients estimated in Section 3, and used for the structural estimation. At the bottom, the table displays the cost shares, number of observations, and p-values associated with the J-test for overidentification.

6 Incidence and Efficiency Gains

In this section, I establish the incidence of payroll taxes on workers, firm owners, and consumers. The computation of tax incidence lays the groundwork for a welfare measure, which delivers a measure for the deadweight loss associated with payroll taxation. This section leverages empirical estimates to provide two key insights. First, a novel payroll tax examination that accounts for the role of consumers in the tax pass-through. Second, a credible design to precisely measure the distortionary costs arising from payroll taxes in Brazil.

6.1 Incidence Framework

Government. The tax base is determined by the total wage bill. Therefore, when payroll tax rates drop, there is a mechanical effect on tax collection,

$$dM = Bd\tau = B(\tau_1 - \tau_0)$$

where τ_0 is the payroll tax rate in the pre-reform period, and τ_1 is the post-reform rate. Nonetheless, the empirical analysis in Section 3 reveals substantial employment and wages responses to tax variation, which partially offset the mechanical tax loss. The resulting behavioral effect on tax revenue is given by:

$$dH = \tau_0 dB = \tau_0 B \left(\frac{\epsilon + 1}{\epsilon} \right) \beta_L$$

Putting all together, the impact of the reform on total tax collection is the mechanical effect net of behavioral adjustments:

$$dR = dM + dH = B \left[d\tau + \tau_0 \left(\frac{\epsilon + 1}{\epsilon} \right) \beta_L \right]$$

This equation offers two direct interpretations. First, a greater employment response implies less tax revenue loss. Second, for a given employment response, labor market power exacerbates wage pass-through, which results in reduced tax revenue loss. For each dollar that is effectively lost in tax collection, it is possible to identify the associated gains. To ensure comparability with existing literature, I rely on a money metric approach for welfare measurement.

Firm owners. As in Suárez Serrato and Zidar 2016b; Fuest et al. 2018, the incidence of the reform to firm owners is quantified based on the share of tax dollars captured by firms in the form of profits. The difference is that in this paper I directly observe profits, as opposed to relying on structural assumptions. To compute the surplus appropriated by firm owners I use the reduced-form coefficients:

$$d\pi = \epsilon^{\pi}_{1+\tau} B \frac{s_\pi}{s_L} \phi_1$$

where, $\epsilon^{\pi}_{1+\tau}$ is the elasticity of profits with respect to the labor cost, while s_L and s_π represent the labor and profit shares, respectively. I rearrange terms to write the effect on firm owners as a function of the total wage bill. The benefit of this approach is that it allows all individual welfare measures to be referenced to the same base, which appropriately weights the welfare attributed to each stakeholder.

Workers. In a monopsonistic labor market, the tax impact on worker surplus is illustrated by the tax-induced variation in area above the labor supply curves, and below the wage times the number of workers. The change in worker surplus can be computed by,

$$dB = w_1 L_1 - \int_0^{L_1} A L^{\frac{1}{\epsilon}} dL - \left(w_0 L_0 - \int_0^{L_0} A L^{\frac{1}{\epsilon}} dL \right) = B\beta_W$$

where w_0, L_0, w_1, L_1 refer to the wage level and employment before and after the reform, respectively. The intuition is that the incidence borne by workers is dictated by the wage effect. Thus, in a perfectly competitive labor market — where all jobs offer equivalent compensation for a given skill set — the incidence to workers is null. This is because under perfect competition, employment at a specific firm provides no additional benefits, since workers have equally attractive opportunities elsewhere.

Consumers. Analogously, the tax impact on a monopolistic product market equilibrium illuminates the welfare effects to consumers surplus, which is computed by the variation in the area between the demand curve and the price times the quantity. The change in consumers' surplus can be computed by:

$$dC = \int_0^{Q_1} Q^{\frac{-1}{\eta}} dQ - P_1 Q_1 - \left(\int_0^{Q_0} Q^{\frac{-1}{\eta}} dQ - P_0 Q_0 \right) = \frac{B}{s_L} \frac{\beta_R}{\eta - 1}$$

The intution is that the effect on consumers is driven by the output price reduction. The reform mitigates labor costs, and a portion of this cost reduction is transferred to the output price, thereby benefiting consumers. Despite prices not being directly observed, they can be inferred from the revenue effect and the demand elasticity η, which is estimated based on the perfect fit between the model and data. Intuitively, the combination of observed effects on revenues and production inputs allows us to back out the price response.

Directly from the data we can also compute the impact on consumers by relying on a residual method. The benefits of a payroll tax cut must be distributed somewhere. If only a small portion is allocated to profits and workers' earnings, then majority of the benefits must be directed to consumers. An advantage of this approach is that it relies solely on empirical estimates. The incidence estimates show that the precise and residual approaches are consistent with each other.

6.2 Efficiency Gains

The efficiency gain induced by a discrete payroll tax cut can be computed following the steps outlined in Appendix D. Equation 19 precisely measures, from the empirical responses, the deadweight loss associated with the payroll tax reform.

$$\Delta W = B\left[\underbrace{\beta_w}_{\text{worker, dw}} + \underbrace{\frac{\beta_\pi s_\pi}{s_L}}_{\text{firm owner, } d\pi} + \underbrace{\frac{\beta_R}{s_L(\eta-1)}}_{\text{consumer, dp}} + \underbrace{\overbrace{(\tau-\tau_0)}^{<0 \text{ (tax cut)}} + \tau_0\frac{\beta_L(\epsilon+1)}{\epsilon}}_{\text{Governement, dT}}\right] \tag{19}$$

Relatedly, the efficiency gains can be measured through the "Marginal Value of Public Funds" (MVPF) metric, applied across a variety of contexts to evaluate the willingness to pay in relation to the net fiscal cost (Mayshar 1990; Slemrod and Yitzhaki 2001; Kleven and Kreiner 2006; Hendren 2016; Bailey et al. 2020).

Estimates. Upon establishing the theoretical incidence and efficiency, I proceed with the structural estimation, as shown in Table 4. Panel B reveals that consumers bear 65% of payroll taxes, while firm owners and workers bear 23% and 12%, respectively. Risch 2024 reports a similar incidence to workers, yet observes no change in employment following a tax increase on S-Corp's owners. A possible explanation for the distinct employment effect is that reductions in payroll taxes decrease the cost of labor, generating incentives for businesses to expand plant size and substitute capital for labor. The aggregate welfare gains experienced by these stakeholders surpass the decrease in Government revenue, which results in a MVPF of 1.66.

Table 4: Structural Parameters and Incidence Estimation

	(1) Identified by	(2) Estimate
Panel A. Parameters Estimate		
Labor Supply Elasticity, ϵ	$\frac{\beta_L}{\beta_W}$	4.15
K-L Elasticity of Substituion, σ	$\frac{\beta_K - \beta_L}{\beta_W + \phi_1}$	1.72
Demand Elasticity, η	$\frac{\sigma\beta_R - \beta_K}{\beta_R - \beta_K}$	1.43
Panel B. Incidence		
Worker, dB	β_W	0.12
Firm Owner, $d\pi$	$\frac{\beta_\pi s_\pi}{s_L}$	0.23
Consumer, dp	$\frac{\beta_R}{s_L(\eta - 1)}$	0.65
Government, dT	$\Delta\tau + \tau_0\beta_L\frac{\epsilon + 1}{\epsilon}$	-0.60
Welfare, dW	$\frac{dB + d\pi + dp + dT}{dT}$	0.66
MVPF	$\frac{dB + d\pi + dp}{dT}$	1.66

Note: This table bridges reduced form and structural estimation. Panel A identifies and estimates structural parameters. Reduced form coefficients are estimated from the quasi-experimental variation and presented in Table 1. Panel B identifies and estimates payroll tax incidence to workers, firm owners, and consumers. Panel B also reports efficiency measures such as the MVPF.

Discussion. The analysis conducted herein yields insights for the tax incidence literature. A key takeaway is that payroll taxes are predominantly paid by consumers. This novel insight, although not yet thoroughly explored in the tax literature, aligns remarkably with minimum wage incidence studies (Harasztosi and Lindner 2019). Furthermore, the efficiency gain from a tax cut is inversely proportional to the distortionary effects of the tax. Essentially, a higher efficiency gain signifies prior to the tax cut there was higher deadweight loss from taxation. The substantial welfare gain calculated for Brazil underscores the prevailing notion that taxes exert particularly distortionary effects in developing economies. This view is supported by the MVPF calculation, which falls in the upper end of the 0.5-2 range reported by Hendren and Sprung-Keyser 2020.

7 Conclusion

In this paper, I study an unprecedentedly large payroll tax reduction that affected a small subset of firms in Brazil. I use firm-level microdata and an empirical strategy that leverages exogenous variation in the eligibility rules to estimate firm and worker-level responses to the tax cut. While capital decreases, a payroll tax reduction causes an increase in employment, wages, revenue, and profits. Firms respond to the reduction in labor cost by substituting capital for labor, and by prominently increasing production. This expansion in production then pushes output prices down, leading revenue to respond less than inputs. These results shed light on a novel and important insight for tax policy: payroll taxes are primarily absorbed by consumers. Furthermore, the empirical findings reveal that skilled workers capture a larger share of the tax benefits relative to low-skilled employees.

The combination of firm-specific shock and positive workers' earnings effect provides compelling evidence against perfectly competitive models. I use direct evidence of firm-level pass-through to underscore the role of (product and labor) market power in mediating tax incidence and efficiency. Imperfect competition can account not only for the incidence to consumers but also for heterogeneous firm responses. One key takeaway for industrial policies - characterized as subsidies targeted to specific businesses - is that responses vary depending on the type of firms. Notably, the analysis strongly suggests that firms with market power tend to increase inputs and output to a lesser extent in response to tax subsidies. With that in mind, it may be advantageous to integrate tax policy with market power regulation.

In macro-level policy that affects the entire economy, market power dynamics also come into play and identifying their effects under general equilibrium is an important topic for future research. The insights from this study illuminate the role of firms in dictating the consequences of tax policy. Particularly, emphasizing the principle that "taxes may not stay where they land", with labor taxes being primarily borne by consumers. This lesson is relevant not only for understanding the distributional consequences of payroll taxes but also other labor policies, such as the minimum wage.

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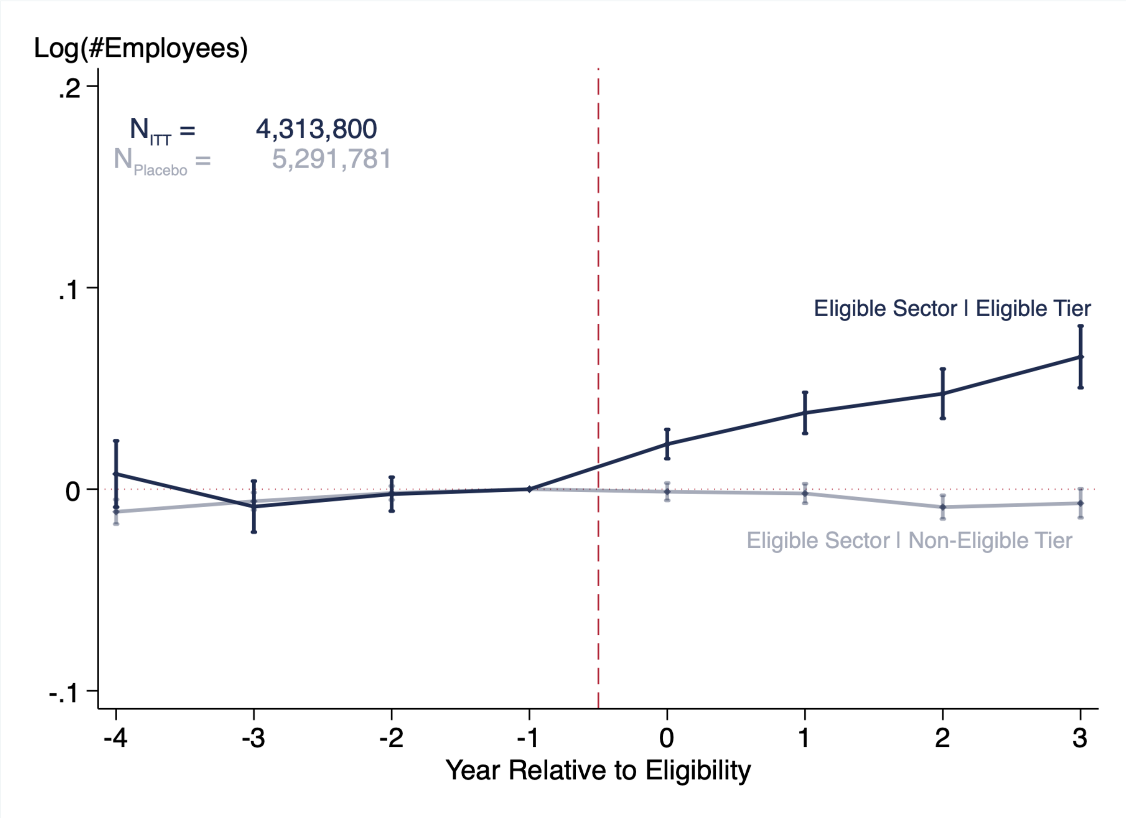
Who Benefits from Payroll Tax Cuts?
Market Power, Tax Incidence and Efficiency

Appendix

Felipe Lobel - UC Berkeley

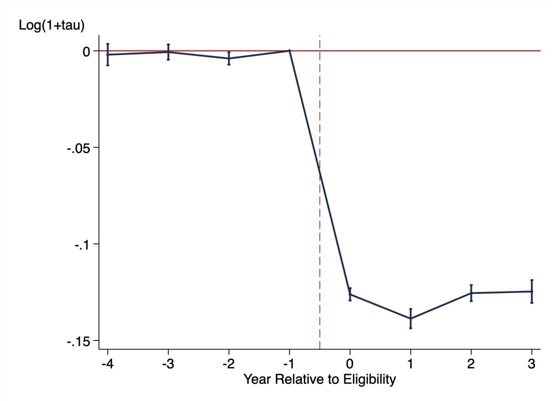
A Figures and Tables

Figure A.1: Spillover Test

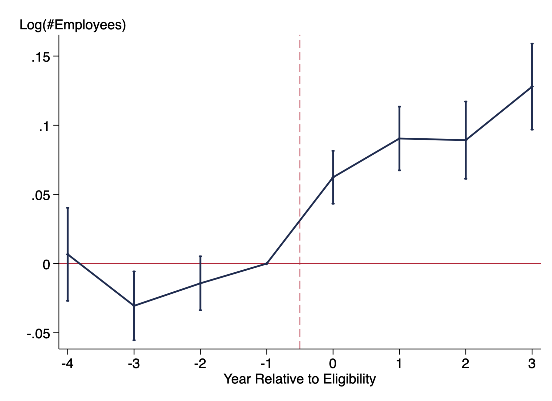


Note: The gray line plots event study coefficients that show non-statistically significant spillover effect to firms in eligible sectors, but ineligible tax tiers. The gray line is estimated on a sample that is restricted to firms in non-eligible tax tiers ("Simples" regime) and depicts a comparison between firms in eligible and non-eligible sectors. To avoid concerns about tier changes, this analysis is restricted to firms that have never changed tiers. The blue line is estimated on a sample that is restricted to firms in eligible tax tiers ("non-Simples" regime). It reports the intention to treat (ITT), i.e., compares eligible firms in eligible vs non-eligible sectors. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

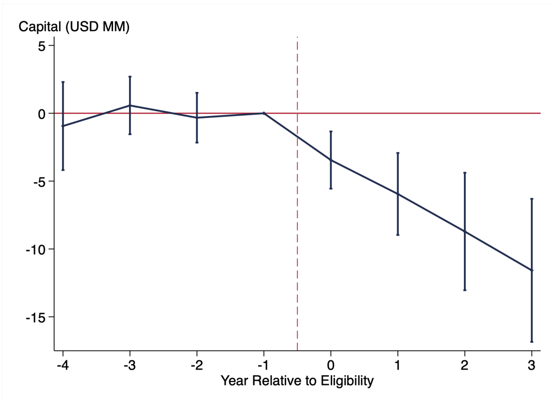
Figure A.2: Firms' Margins of Adjustment



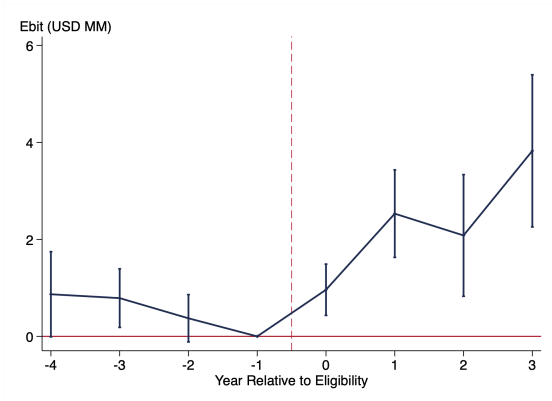
(a) Effects on payroll tax rates



(b) Effects on employment



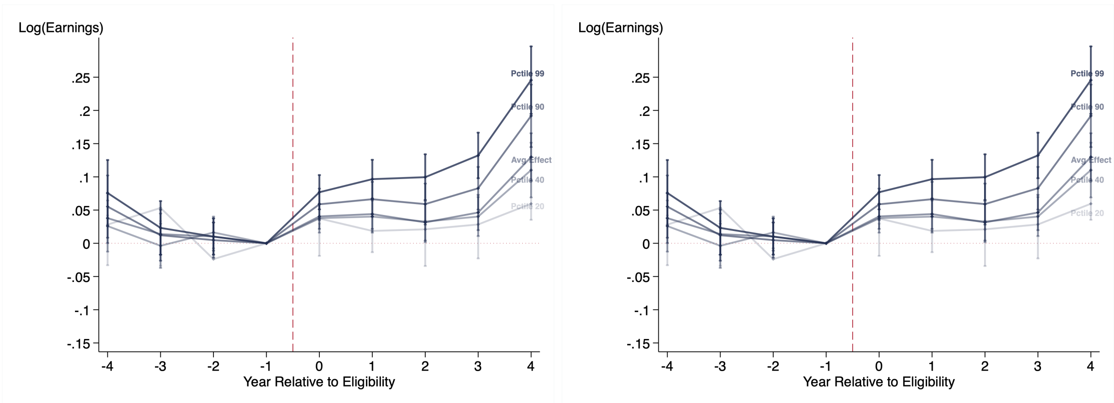
(c) Effects on capital



(d) Effects on profits

Note: This figure plots event study coefficients for multiple of the firms' margins of adjustment after the payroll tax cut. First, at the top left plot it shows the first stage, i.e., the reform induced a reduction in payroll tax liability. On the top right plot, it depicts the employment increase that has already been documented. The two bottom graphs shed light on other business outcomes, such as capital and profit.

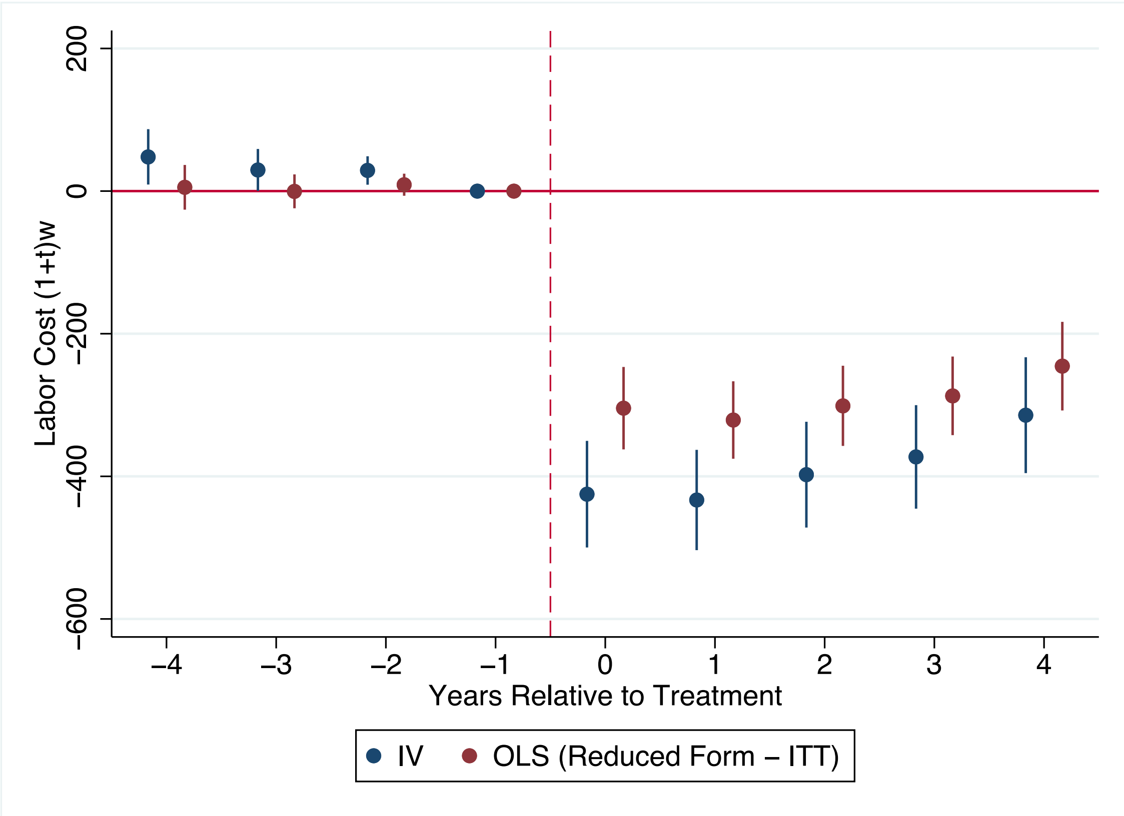
Figure A.3: Earnings Effect Within Firm Wage Distribution per Firm Size



(a) Small firms (below median) (b) Large firms (above median)

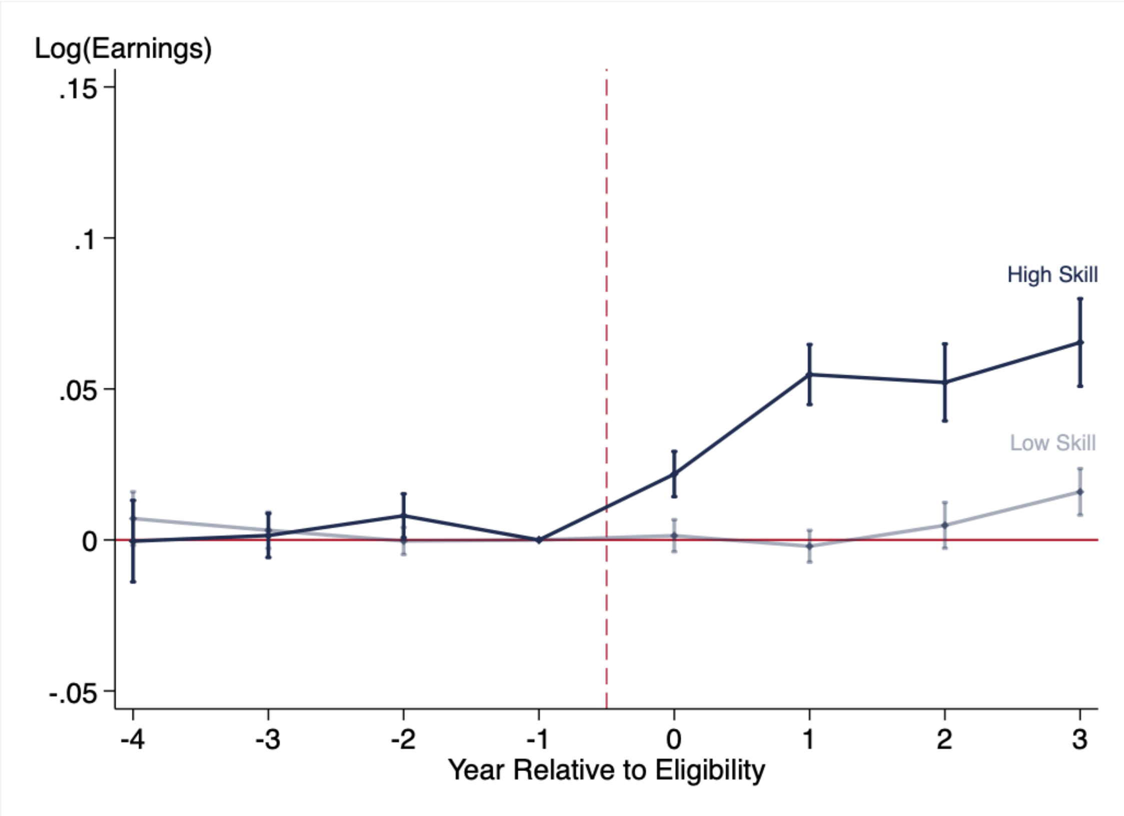
Note: This figure presents event study estimates for wages at different percentiles of the within-firm wage distribution. Figure (a) on the left reports results for small firms. Figure (b) on the right reports results for large firms. Firm size is measured according to the pre-reform median. The event is the year in which the firm enters treatment for the first time. I normalize results with respect to one year prior to the event. Standard errors are conservatively clustered at 5-digit industry-by-state level.

Figure A.4: Worker Level: Gross Earnings Effect



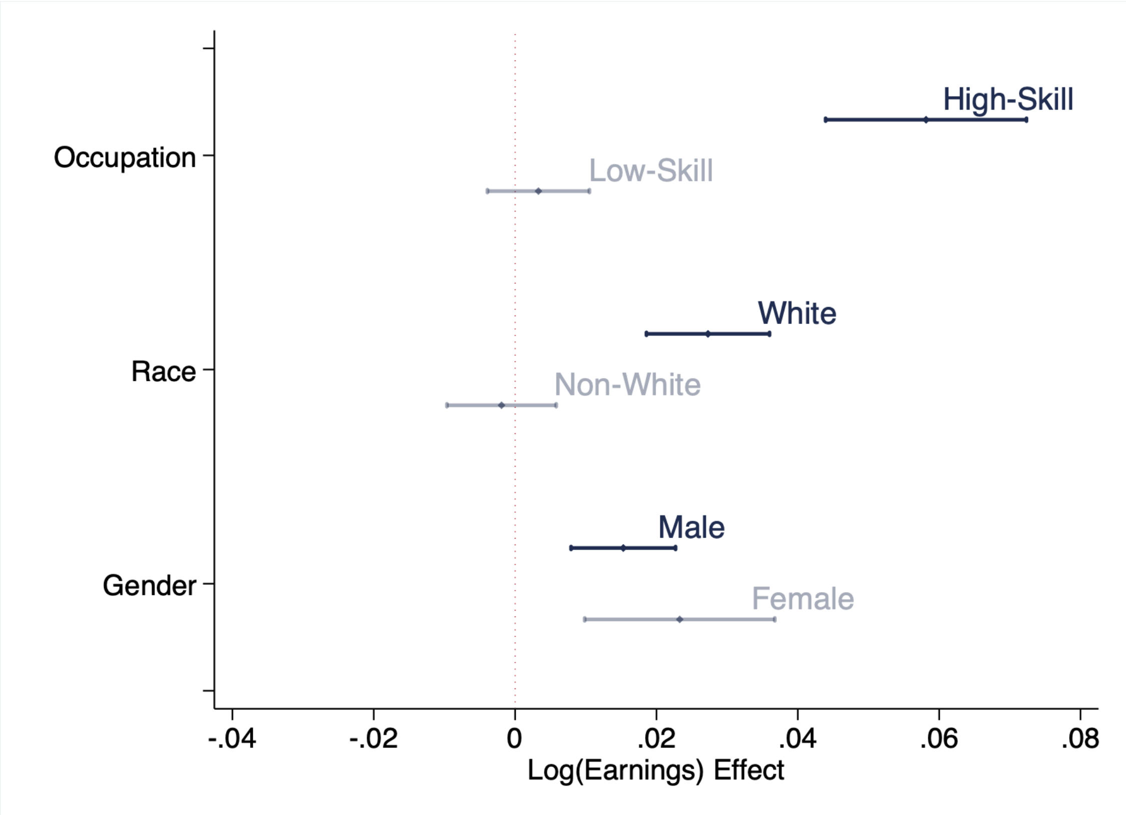
Note: This figure presents the event study estimates for average gross earnings (including payroll taxes) paid workers that were employed for at least three years in the same firm during the pre-reform period. The labor cost is computed using firm-level tax data, and worker-level earnings data. I apply 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 four years after. The plot shows an average decrease of $400 on the gross earnings, which has an approximate average of $2,300 during the pre-reform period. The blue markers depict IV coefficients, and the red markers intention-to-treat. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Figure A.5: Worker Level: Earnings per Occupation



Note: This figure presents the event study estimates for the log of pre-tax earnings per occupation group, at the worker-level based on pre-reform occcupations. Leaders are directors, managers and qualified technical positions according to the CBO classification. While leaders experience high pass-through to earnings of 6%, low-skilled occupation didn't see any significant earnings increase. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Figure A.6: Heterogeneities by Worker Type



Note: This figure presents the IV difference-in-differences coefficient for the earnings effect at the worker-level sample, across many characteristics of interest, such as, occupation, gender and race. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table A.1: Macro Relevance of the Reform

	2012	2013	2014
# Sectors	10	81	124
Share	0.0076	0.0617	0.0944
# Firms	20,865	33,705	49,253
Share	0.0079	0.0121	0.0170
# Workers	2,950,925	5,028,078	6,113,091
Share	0.0304	0.0513	0.0618

Note: This table shows the comprehensiveness of the policy rollout over the years that new sectors gained eligibility (2012-2014). In the first part of the table it shows the number of 7-digit sectors eligible for the tax reform, and their representativeness computed as the share of existing sectors in the Brazilian economy. The second part of the table shows the number of formal firms in the final sample that were treated in each year. To adjust for informal firms that do not appear in my sample, I multiply the share by 0.55, which is the average formalization rate in Brazil, according to PNAD (official survey administered by the Brazilian Census Bureau, IBGE). In the last rows, the table reports the number of workers employed in treated firms. I compute the share of treated workers by dividing # of workers by the universe of Brazilian workers according to PNAD-C.

Table A.2: Worker Level Estimates

Worker Level	**Log(Earnings)**	**Log(Earnings)**	
	All Sample (1)	Blue Collar (2)	White Collar (3)
Panel B: IV			
Diff-in-Diff	.018** (.007)	.003 (.007)	.058*** (.014)
Long Diff	.027*** (.007)	.016** (.008)	.064*** (.014)
Panel A: OLS			
Diff-in-Diff	.009** (.004)	.002 (.004)	.031*** (.008)
Long Diff	.017*** (.005)	.01* (.005)	.044*** (.01)
Controls	✓	✓	✓
Worker FE	✓	✓	✓
Firm FE	✓	✓	✓
Sector x Year FE	✓	✓	✓
# Clusters	10,458	10,309	8,938
N	112,621,077	84,007,708	25,118,914

Note: This table presents IV and reduced form (ITT) estimates for the worker-level sample. Difference-in-differences coefficient is estimated in equations 3 and 4, where there is only one post-period. The long difference comes from the period t=+3, in the event study design. Panel A reports the IV coefficients, which adjust for the imperfect compliance and are interpreted as the local average treatment effect on compliers. Panel B reports the reduced form coefficients, which are interpreted as the intention to treat (ITT). The dependent variable is log of workers' earnings. Column (1) presents the average effect in the all sample. Columns (2-6) present heterogeneity based on pre-reform occupation. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table A.3: Informality Analysis

	(1) Log(1+τ)	(2) Log(#Employees)	(3) Log(Earnings)
Panel A: Low Informality Areas			
Diff-in-Diff	-0.133***	0.135***	0.025*
	(0.004)	(0.039)	(0.014)
Long Diff	-0.121***	0.204***	0.018
	(0.005)	(0.035)	(0.015)
Panel B: High Informality Areas			
Diff-in-Diff	-0.131***	0.031	-0.003
	(0.004)	(0.031)	(0.011)
Long Diff	-0.116***	0.011	0.022*
	(0.006)	(0.043)	(0.012)
Panel C: High Education Firms			
Diff-in-Diff	-.135***	.201***	.03**
	(.004)	(.034)	(.014)
Long Diff	-.119***	.216***	.038***
	(.005)	(.038)	(.014)
Panel D: Low Education Firms			
Diff-in-Diff	-0.129***	0.008	0.003
	(0.004)	(0.033)	(0.013)
Long Diff	-0.121***	0.004	0.010
	(0.006)	(0.039)	(0.012)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Sector x Year FE	✓	✓	✓
# Clusters	9,548	9,953	9,953
N	3,908,467	4,225,726	4,225,726

Note: This table reports results from the informality analysis, showing that effects are concentrated in low informality regions, and firms employing relatively more educated workforce, which are settings less prone to informality. Panel A presents results for low informality municipalities, which are defined as the bottom 50% of the informality distribution. Panel B presents results for high informality areas. Panel C presents results for firms that employ relatively more educated workers, which are defined as above the median, while Panel D presents results for below median on average education. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table A.4: Heterogeneity Across Liquidity Constraints

	(1) Employment Low Liquidity	(2) Employment High Liquidity
Currently Treated	0.107***	0.109***
	(0.0283)	(0.0289)
Observations	228,087	233,691
Firm FE	Yes	Yes
Sector (1 digit) x Year FE	Yes	Yes
Worker FE	No	No

Standard errors in parentheses

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

Note: This table reports IV difference-in-differences coefficients for firms below/above the median on liquidity constraint, during the pre-reform period. Liquidity constraint is defined as the ratio of current assets over current liabilities. An example of current assets is cash, whereas an example of current liabilities is short term bills, such as the wage bill. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table A.5: Within-Firm Earnings Inequality

	Log(Earnings)				Occup Pctile
	firm (99p)	firm (90p)	firm (40p)	firm (20p)	firm level
	(1)	(2)	(3)	(4)	(5)
Panel A: IV					
Diff-in-Diff	.041***	.022	.01	.003	.001
	(.016)	(.013)	(.011)	(.01)	(.002)
Long Diff	.068***	.038***	.012	-.003	.005
	(.016)	(.013)	(.011)	(.011)	(.003)
Controls	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓
Sector x Year FE	✓	✓	✓	✓	✓
# Clusters	10,679	10,679	10,679	10,679	10,674
N	4,234,882	4,234,882	4,234,882	4,234,882	4,232,627

Note: This table presents IV estimates for the firm-level sample. Difference-in-differences coefficient is estimated in equations 3 and 4, where there is only one post period. The long difference comes from the period t=+3, in the event study design. Column (1)-(4) reports the earnings effect at different percentiles of the within-firm distribution, indicating that the pass-through predominantly affects employees at the higher end of the spectrum. Column (5) reports zero effect on the average occupation percentile. Occupations are ranked based on average earnings during the years prior to the reform. After each occupation has been allocated to a specific percentile, we calculate, for each t, the mean occupation percentile that firms are employing from. The zero occupation response reinforces that the within-firm inequality response is not driven by an upscale in employed occupations. It also reinforces that the tax cut did not induce a structural change in the production process at the firm-level. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

B Details on the Empirical Model

B.1 Derivation of the Reduced Form Equations

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

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

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; $\xi_{I(j),t}$ is industry (broader than sector) x year fixed effect; α_j is the firm fixed effect; η_{jt} and ϵ_{jt} are residuals. Standard errors are conservatively clustered at the 5-digit industry-by-state level. Reorganizing terms,

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

Thus, the reduced form coefficient is,

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

Note 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 equation (2) 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.

B.2 Characterizing Compliers

Section 3.1 stresses that the causal interpretation for the LATE is restricted to the set of compliers. Oftentimes, compliers are not representative of the population, therefore it is useful to have a deeper understanding of who the compliers are. The challenge is that different from *always-takers* and *never-takers* compliers' characteristics are not observationally identified. Even though it is observable if an eligible firm took up treatment, it is not observable if the take-up decision is because the firm is an *always-taker* or *complier*. This comes from the fact that the counterfactual decision (what an eligible firm would do if it were not to be eligible) is not observable in the data.

Abadie 2002 proposes a 2SLS approach to detect compliers. This method relies on the fact that *never-takers* (eligible firms that do not take-up) and *always-takers* (ineligible firms that take-up) are observable. Concretely, it estimates the pair of regressions:

$$X_{jt} \times \mathbb{I}_{D_j=d} = \alpha_d + \gamma_d \mathbb{I}_{D_j=d} + \nu_{jtd} \tag{1}$$

$$\mathbb{I}_{D_j=d} = \zeta_d + \pi_d L_{s(j)} + \eta_{jtd} \tag{2}$$

where X_{jt} is a vector of firm's characteristics at the baseline; d ={0,1} indicates if $L_{s(j)}$ is instrumenting eventual treatment or never treatment; and α_d, ζ_d are constants. The IV coefficients for d ={0,1} recover average characteristics for never and eventually treated compliers, respectively. To obtain baseline characteristics for *never-takers* I regress $X_{jt}(1-D_j)L_{s(j)}$ on $(1-D_j)L_{s(j)}$. Finally, the characterization of *always-takers* comes from regressing $X_{jt}D_j(1-L_{s(j)})$ on $D_j(1-L_{s(j)})$. Table B.1 reports results for the same regressions when we incorporate the 1-digit sector x year dummies and set of controls that are included in the main specification.[20] The table shows that covariates' means for treated and untreated compliers are not statistically distinguishable between each other. As Angrist et al. 2022 point out, the balance check across compliers is equivalent to the hidden complier RCT embedded in the treatment assignment with imperfect compliance. Comparisons to the remaining columns showcase that *always-takers* are larger firms, and *never-takers* are smaller firms compared to compliers.

[20] The interpretation of coefficients is compliers' weighted average characteristics within sector x year cells.

Table B.1: Compliers' Characteristics

	Compliers			
	Untreated (1)	Treated (2)	Always-Takers (3)	Never-Takers (4)
Employment	108.54 (29.592)	103.69 (16.237)	188.94 (16.86)	27.1 (2.692)
Payroll Tax Rate	.33 (.011)	.35 (.002)	.35 (.002)	.29 (.004)
Share Male	.73 (.02)	.73 (.02)	.71 (.017)	.74 (.022)
Age	35.17 (.326)	35.17 (.326)	33.36 (.087)	36.68 (.354)
High School +	.58 (.026)	.6 (.022)	.57 (.007)	.58 (.022)
White	.75 (.025)	.75 (.025)	.76 (.007)	.71 (.027)
Blue Collar	.8 (.018)	.8 (.018)	.92 (.003)	.86 (.016)
Sector x Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Note: This table reports baseline estimates characteristics of compliers, always-takers and never-takers in the context of the Brazilian tax reform. Values for each covariate are computed in the pre-reform period at the firm x year level, and the regressions include 1-digit sector x year fixed effects and set of controls considered in the main specification (Section 3.1). Standard errors are reported in parentheses and conservatively clustered at the 5-digit industry-by-state level.

C Model

In this appendix, I present the model derivation. For didactic purposes, I start by analyzing the revenue and payroll taxes, separately. In the end, I put both taxes together to map the structural equations to the reduced form estimates.

C.1 Microfounding the Labor Supply

As in Card et al. 2018, workers exhibit idiosyncratic preferences for employers. These preferences can be understood through non-pecuniary match factors such as corporate culture and commuting distance. Unlike traditional search models, this approach posits that wage-posting behavior induces firms to pay identical wages to all workers of the same quality. Upon meeting the requisite quality standards, a firm hires any worker willing to accept the posted wage. In this scenario, worker i is fully knowledgeable of available job opportunities, and derives the following utility from working at firm j:

$$u_{ij} = \epsilon \ln(w_j - b) + a_j + \nu_{ij}$$

where, w_j is the wage level paid by firm j, b is the competitive wage level defined by the workers' outside option, a_j is a firm-specific amenity, and ν_{ij} is the idiosyncratic preference for worker i to be at firm j. Assuming that ν_{ij} comes from an extreme type I distribution, I follow McFadden et al. 1973 to compute the logit probabilities to work at firm j:

$$p_j = \frac{\exp(\epsilon \ln(w_j - b) + a_j)}{\sum_{k=1}^{J} \exp(\epsilon \ln(w_k - b) + a_k)}$$

If the total number of firms J is large enough, the logit probabilities can be approximated by exponential probabilities of the form,

$$p_j = \lambda \exp(\epsilon \ln(w_j - b) + a_j)$$

where λ is a constant common to all firms in the market. Therefore, for large J, we can write the firm-specific supply function as:

$$\ln L_j(w_j) = \ln \mathbb{L}\lambda + \epsilon \ln(w_j - b) + a_j$$

where $\mathbb{L}$ represents the total number of workers in the market. Taking exponential transformations on both sides, we can compute the labor supply function:

$$L_j = \exp(\epsilon \ln(w_j - b)) \exp(a_j) \exp(\lambda \mathbb{L}) \iff L_j^{\frac{1}{\epsilon}} \underbrace{\exp\left(\frac{-\mathbb{L}\lambda - a_j}{\epsilon}\right)}_{\equiv A_j} = (w_j - b)$$

As b $\to 0$, then

$$w_j = A_j L_j^{\frac{1}{\epsilon}} \tag{1}$$

In this case, ϵ is the constant labor supply elasticity faced by the firm.

C.2 Effects of Payroll Taxation

The labor supply function gives rise to the cost function faced by firms,

$$C = A((1+\tau))L^{\frac{1}{\epsilon}+1} + rK$$

Production function exhibits constant returns to scale, and the firm faces demand at the product market given by, $P = Q^{\frac{-1}{\eta}}$. The firm solves two related problems. First, it chooses plant size to maximize profit. Second, for a given plant size (Q), it chooses inputs of production (L and K) to minimize costs, according to the following program:

$$\begin{aligned} \min_{K,L} \quad & A(1+\tau)L^{\frac{1}{\epsilon}+1} + rK \\ \text{s.t.} \quad & f(K,L) \geq Q \end{aligned} \tag{2}$$

Summing and rearranging the optimality conditions, I obtain the cost function:

$$C = \underbrace{\lambda(w,r,Q)}_{=\frac{\partial C}{\partial Q}} \underbrace{(Lf_L + Kf_K)}_{=Q} - \underbrace{A(1+\tau)\frac{1}{\epsilon}L^{1+\frac{1}{\epsilon}}}_{new} \tag{3}$$

Differently from the perfectly competitive labor market, under monopsony average and marginal cost no longer align. Lemma 1 proves this point.

Lemma 1. *In a perfectly competitive labor market, the marginal cost of production is constant in the quantity Q.*

Proof. From FOC,

$$C(w,r,Q) = \lambda(w,r,Q)Q \iff C(w,r,\alpha Q) = \lambda(w,r,\alpha Q)\alpha Q$$

From constant returns,

$$\begin{aligned} C(w,r,\alpha Q) = \alpha C(w,r,Q) = \alpha\lambda(w,r,Q)Q \\ \lambda(w,r,\alpha Q)\alpha Q = \lambda(w,r,Q)\alpha Q \Rightarrow \lambda(w,r,Q) = \lambda(w,r) \end{aligned}$$

□

The profit maximizing firm chooses output Q,

$$\max_Q P(Q)Q - c(Q,\tau)Q + \frac{1}{\epsilon}A(1+\tau)L^{1+\frac{1}{\epsilon}}$$

At the optimal, marginal cost and marginal revenue are equated:

$$\left(\frac{\eta-1}{\eta}\right)Q^{\frac{-1}{\eta}} = \lambda(Q,\tau) \tag{4}$$

To evaluate the policy induced scale effect, I take logs and differentiate with respect to the labor cost $(1+\tau)$,

$$\epsilon^{Q}_{1+\tau} = \frac{-\epsilon^{\lambda}_{1+\tau}}{\left(\frac{1}{\eta} + \epsilon^{\lambda}_{Q}\right)} \tag{5}$$

Also note that from 4,

$$P\left(\frac{\eta-1}{\eta}\right) = \lambda \iff \frac{\partial \log P}{\partial \log(1+\tau)} = \frac{\partial \log \lambda}{\partial \log(1+\tau)} = \epsilon^{\lambda}_{1+\tau} + \epsilon^{\lambda}_{Q}\epsilon^{Q}_{1+\tau}$$

$$\frac{\partial \log Q}{\partial \log(1+\tau)} = \underbrace{\frac{\partial \log Q}{\partial \log P}}_{-\eta} \overbrace{\frac{\partial \log P}{\partial \log(1+\tau)}}^{\epsilon^{\lambda}_{1+\tau}+\epsilon^{\lambda}_{Q}\epsilon^{Q}_{1+\tau}}$$

$$\frac{\partial \log Rev}{\partial \log(1+\tau)} = \frac{\partial \log PQ}{\partial \log(1+\tau)} = (1-\eta)(\epsilon^{\lambda}_{1+\tau} + \epsilon^{\lambda}_{Q}\epsilon^{Q}_{1+\tau}) \tag{6}$$

Applying the envelope theorem to derive equation (3) with respect to $(1+\tau)$,

$$AL^{\frac{1}{\epsilon}+1} = \lambda_{1+\tau}Q - \frac{AL^{1+\frac{1}{\epsilon}}}{\epsilon} - \frac{A(1+\tau)}{\epsilon}\left(\frac{1+\epsilon}{\epsilon}\right)L^{\frac{1}{\epsilon}}\frac{\partial L}{\partial(1+\tau)}$$

$$\frac{\partial \log \lambda}{\partial \log(1+\tau)} = \frac{(1+\tau)AL^{1+\frac{1}{\epsilon}}}{\lambda Q}\left(\frac{\epsilon+1}{\epsilon}\right)\left(1 + \frac{(1+\tau)}{\epsilon L}\frac{\partial L}{\partial(1+\tau)}\right) \tag{7}$$

Equation (7) refers to the elasticity of the marginal cost with respect to the labor cost, which is a key aspect of the incidence analysis. Taking this expression to the data is challenging because we do not observe either λ, or Q. However, by manipulating equation (3) and dividing both sides by the total wage bill we obtain,

$$\frac{\lambda Q}{(1+\tau)AL^{1+\frac{1}{\epsilon}}} = \frac{C + (1+\tau)AL^{1+\frac{1}{\epsilon}}(\frac{1}{\epsilon})}{\underbrace{(1+\tau)AL^{1+\frac{1}{\epsilon}}}_{wL}} = \frac{1}{s_L} + \frac{1}{\epsilon} \tag{8}$$

The right hand side of equation (8) depends on s_L and ϵ. It turns out that we do observe labor share (s_L), and we can estimate ϵ. Plugging 8 in 7,

$$\epsilon^{\lambda}_{1+\tau} = \left(\frac{1}{\frac{1}{s_L} + \frac{1}{\epsilon}}\right)\left(\frac{\epsilon+1}{\epsilon}\right)\left(1 + \frac{\epsilon^{L}_{1+\tau}}{\epsilon}\right) \tag{9}$$

Equation (9) shows that the effect of the labor cost on the marginal cost depends on three components. First, is the monopsony-adjusted labor share. The more relevant is the labor share, which means that reducing labor costs will have a greater impact on the marginal cost. Second, is the inverse markdown. The intuition for this term is that as labor market power increases, there is more rents to be shared with incumbent workers when the firm expands plant size. Finally, the last term says that the pass-through to marginal cost is directly affected by the pass-through to the marginal cost of labor. Differentiating both sides of equation (3) by Q, after some manipulation I obtain,

$$\epsilon_Q^\lambda = \left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{\epsilon_Q^L}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right) \tag{10}$$

Note that,

$$\epsilon_{1+\tau}^L = \frac{\partial \log L}{\partial \log(1+\tau)} = \frac{\partial \log L}{\partial \log Q}\frac{\partial \log Q}{\partial \log(1+\tau)}$$

$$\epsilon_Q^L = \frac{\epsilon_{1+\tau}^L}{\epsilon_{1+\tau}^Q} \tag{11}$$

Using 5 in 11,

$$\epsilon_Q^L = \frac{-\epsilon_{1+\tau}^L(\frac{1}{\eta}+\epsilon_Q^\lambda)}{\epsilon_{1+\tau}^\lambda} \tag{12}$$

Now, 12 and 9 in 10,

$$\epsilon_Q^\lambda = \frac{-\epsilon_{1+\tau}^L}{\eta(2\epsilon_{1+\tau}^L+\epsilon)} \tag{13}$$

To compute $\epsilon_{1+\tau}^Q$ substitute 9 and 13 in 5,

$$\epsilon_{1+\tau}^Q = -\left(\frac{\epsilon+1}{\epsilon}\right)\left(1+\frac{\epsilon_{1+\tau}^L}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\eta(\epsilon+2\epsilon_{1+\tau}^L)}{\epsilon+\epsilon_{1+\tau}^L}\right) \tag{14}$$

To compute the tax reduction pass-through to employment and capital, I can differentiate optimal choices in 2 with respect to the labor cost ($(1+\tau)$):

$$\epsilon_{1+\tau}^L = \frac{\epsilon}{1-\epsilon\rho+\epsilon}(\epsilon_{1+\tau}^\lambda + \epsilon_Q^\lambda\epsilon_{1+\tau}^Q - 1) + \left(\frac{(1-\rho)\epsilon}{1-\epsilon\rho+\epsilon}\right)\epsilon_{1+\tau}^Q$$

Plugging 9, 13 and 14, I obtain the model's prediction for the pass-though to employment, in terms of observables and parameters to be estimated:

$$\epsilon_{1+\tau}^L = \left(\frac{\epsilon}{1+\epsilon(1-\rho)}\right)\left[\left(\frac{(\epsilon+2\epsilon_{1+\tau}^L)(\sigma-\eta)}{\sigma\epsilon}\right)\left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)-1\right] \tag{15}$$

Recall, that the elasticity of employment with respect to labor cost $\epsilon_{1+\tau}^L$ I empirically estimate in the reduced form analysis. The remaining structural parameters are jointly estimated in Section 5. Similarly, I can find equations for the pass-through to capital, and revenue.

$$\epsilon^K_{1+\tau} = \left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+2\epsilon^L_{1+\tau}}{\epsilon}\right)\left(\sigma-\eta\right) \tag{16}$$

$$\epsilon^R_{1+\tau} = (1-\eta)\left[\left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{\epsilon+2\epsilon^L_{1+\tau}}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\right] \tag{17}$$

Taking logs and differentiating the labor supply function,

$$\beta_W = \frac{\epsilon^L_{1+\tau}}{\epsilon}\phi_1 \tag{18}$$

C.3 Effects of Revenue Taxation

Under revenue taxation (τ_r), the firm solves the following program in the product market:

$$\max_Q P(Q)Q - \frac{C(Q)}{1-\tau_r}$$

The firm equates marginal revenue to marginal cost,

$$\left(\frac{\eta-1}{\eta}\right)Q^{\frac{-1}{\eta}} = \frac{\lambda(Q)}{1-\tau_r}$$

where the right-hand side is a direct application of the envelope theorem on the cost minimization problem. The plant size has a direct implication on prices through demand, so if we take logs and differentiate with respect to $\log \tau_r$,

$$\frac{\partial \log P}{\partial \log \tau_r} = \frac{\tau_r}{1-\tau_r}$$

I know the relationship between the elasticity of prices and quantity with respect to revenue taxes,

$$\frac{\partial \log P}{\partial \log Q}\frac{\partial \log Q}{\partial \log \tau_r} = \frac{\partial \log P}{\partial \log \tau_r} \iff \frac{\partial \log Q}{\partial \log \tau_r} = -\frac{\tau_r}{1-\tau_r}\eta \tag{19}$$

where the $\frac{\partial \log P}{\partial \log Q} = \frac{-1}{\eta}$ is known based on the iso-elastic demand function. The price and quantity responses allow me to compute the effect of revenue taxes on revenue,

$$\epsilon^R_{1+\tau_r} = \frac{\tau_r}{1-\tau_r}(1-\eta)$$

Once firms, choose the plant size, they will choose the inputs mix to minimize cost,

$$C(Q) = \min_{K,L} \quad AL^{\frac{1}{\epsilon}+1} + rK$$
$$\text{s.t.} \quad (s_L L^\rho + s_K K^\rho)^{\frac{1}{\rho}} \geq Q$$

The optimal choices of capital and labor are:

$$L = \left[\left(\frac{\epsilon}{\epsilon+1}\right)\frac{s_L}{A}\lambda(Q)\right]^{\frac{\epsilon}{1-\epsilon\rho+\epsilon}} Q^{\frac{(1-\rho)\epsilon}{1-\epsilon\rho+\epsilon}} \qquad K = \left(\frac{r}{\lambda(Q)s_K}\right)^{\frac{1}{\rho-1}} Q$$

Taking logs and differentiating with respect to $\log \tau_r$, we obtain the revenue tax pass-through to employment and wages,

$$\frac{\partial \log L}{\partial \log \tau_r} = \frac{-\epsilon}{1-\epsilon\rho+\epsilon} + \left(\frac{(1-\rho)\epsilon}{1-\epsilon\rho+\epsilon}\right)\frac{\partial \log Q}{\partial \log \tau_r} + \frac{\epsilon}{1-\epsilon\rho+\epsilon}\left(\frac{\partial \log \lambda(Q)}{\partial \log Q}\frac{\partial \log Q}{\partial \log \tau_r}\right) \tag{20}$$

$$\frac{\partial \log K}{\partial \log \tau_r} = \frac{\partial \log Q}{\partial \log \tau_r} - \left(\frac{1}{\rho-1}\right)\left[\frac{\partial \log \lambda(Q)}{\partial \log Q}\frac{\partial \log Q}{\partial \log \tau_r}\right] \tag{21}$$

To obtain closed form solution for the pass-through expressions we need to compute the elasticity of marginal cost with respect to quantity ϵ^λ_Q, which we can pin down by differentiating the cost function with respect to Q,

$$\epsilon^\lambda_Q = \left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+1}{\epsilon}\right)\frac{\epsilon^L_Q}{\epsilon} \tag{22}$$

Note that,

$$\epsilon^L_Q = \frac{\epsilon_{L\tau_r}}{\epsilon^Q_{\tau_r}} \iff \epsilon^L_Q = \frac{-\epsilon^L_{\tau_r}(1-\tau_r)}{\tau_r\eta} \tag{23}$$

Plugging 23 in 22,

$$\epsilon^L_{\tau_r} = -\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+1}{\epsilon}\right)\frac{\epsilon^L_{\tau_r}(1-\tau_r)}{\tau_r\eta}$$

Plugging ϵ^λ_Q and $\epsilon^Q_{\tau_r}$ in 20 and 21, we obtain the closed form pass-through expressions for the revenue taxation,

$$\epsilon^L_{\tau_r} = \frac{-(1-\rho)\epsilon}{1+\epsilon(1-\rho-\chi(\epsilon,s_L))}\frac{\tau_r}{1-\tau_r}\eta \tag{24}$$
,
$$\epsilon^K_{\tau_r} = \frac{\tau_r\eta}{1-\tau_r}\left(\frac{-\chi(\epsilon,s_L)\epsilon}{1+\epsilon(1-\rho-\chi(\epsilon,s_L))}-1\right) \tag{25}$$

where, I denote $\chi(\epsilon, s_L) = \left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+1}{\epsilon}\right)$ to simplify notation. The elasticity

η makes the model versatile to accommodate different degrees of competition in the product market. As η increases, we move to a more competitive product market. At first, we will be agnostic about its value, and let η be determined by the data. For the specific case of the Brazilian tax reform, the revenue tax rate is small (around 1.5%). For this reason, the effects depicted on equations 24 and 25 are negligible compared to the responses coming from the payroll tax side.

D Deadweight Loss

Payroll taxes depresses wages, profits, and consumption, while increases Government revenue. To compute the efficiency effect of taxation, Equation (1) relies on a money metric approach that aggregates the net benefit and costs of payroll taxes.

$$W = \underbrace{wL - \int_0^L Ak^{\frac{1}{\epsilon}}dk}_{\text{worker surplus}} + \underbrace{PQ - wL(1+\tau) - rK}_{\text{firm owner surplus}} + \underbrace{\int_0^Q z^{\frac{-1}{\eta}}dz - PQ}_{\text{consumer surplus}} + \underbrace{wL\tau}_{\text{Gov revenue}}$$

$$W = -\int_0^L Ak^{\frac{1}{\epsilon}}dk + \int_0^Q z^{\frac{-1}{\eta}}dz - rK \tag{1}$$

Therefore, the efficiency gain induced by a discrete payroll tax cut can be computed according to the following formula:

$$\Delta W = B\left[\underbrace{\beta_w}_{\text{worker, dw}} + \underbrace{\frac{\beta_\pi s_\pi}{s_L}}_{\text{firm owner, } d\pi} + \underbrace{\frac{\beta_R}{s_L(\eta-1)}}_{\text{consumer, dp}} + \underbrace{\overbrace{(\tau-\tau_0)}^{<0\text{ (tax cut)}} + \tau_0\frac{\beta_L(\epsilon+1)}{\epsilon}}_{\text{Governement, dT}}\right] \tag{2}$$

Taking Equation 2 to the data, we obtain a precise measure of the deadweight loss associated with payroll taxation. To obtain further theoretical intuition about the forces at play on the determinants of the deadweight loss of payroll taxes, I totally differentiate Equation 1:

$$dW = -\frac{\partial L}{\partial \tau}AL^{\frac{1}{\epsilon}} + \frac{\partial Q}{\partial \tau}Q^{\frac{-1}{\eta}} - r\frac{\partial K}{\partial \tau}$$

$$dW = \underbrace{\frac{\partial L}{\partial \tau}\left[\overbrace{\frac{w}{\epsilon}}^{\text{Monopsony}} + \overbrace{w\left(\frac{\epsilon+1}{\epsilon}\right)\tau}^{\text{Tax wedge}}\right]}_{\text{labor wedge}} + \underbrace{\left[\frac{\partial Q}{\partial \tau}Q^{\frac{-1}{\eta}} - \frac{\partial L}{\partial \tau}\overbrace{\mu_L}^{\text{MCL}} - \overbrace{\mu_K}^{\text{MCK}=r}\frac{\partial K}{\partial \tau}\right]}_{\text{product wedge}}$$

The product market wedge can be expressed as a function of $\frac{\partial Q}{\partial \tau}$:

$$dW = \frac{\partial L}{\partial \tau}\left[\frac{w}{\epsilon} + w\left(\frac{\epsilon+1}{\epsilon}\right)\tau\right] + \frac{\partial Q}{\partial \tau}\left[Q^{\frac{-1}{\eta}} - \frac{\frac{\partial L}{\partial \tau}}{\frac{\partial Q}{\partial \tau}}a - r\frac{\frac{\partial K}{\partial \tau}}{\frac{\partial Q}{\partial \tau}}\right] \tag{3}$$

To compute the ratio of derivatives in equation 3, I recall the optimal input choices from the cost minimization problem:

$$\mathbb{L} = A(1+\tau)L^{\frac{1}{\epsilon}+1} + rK + \lambda[Q - (s_L L^\rho + s_K K^\rho)^{\frac{1}{\rho}}]$$

The lagragean multiplier λ is the shadow price of output, and it is equal to the marginal cost of production. The first order conditions are:

$$[L] : \underbrace{\frac{\epsilon+1}{\epsilon}A(1+\tau)L^{\frac{1}{\epsilon}}}_{\text{MCL}} = \underbrace{\lambda}_{\text{Mg Cost}} \underbrace{s_L(s_L L^\rho + s_K K^\rho)^{\frac{1}{\rho}-1}L^{\rho-1}}_{\text{MPL}=\frac{\partial Q}{\partial L}}$$

Therefore,

$$\frac{1}{\frac{\partial Q}{\partial L}} = \frac{\lambda}{\mu_L} \tag{4}$$

$$[K] : \underbrace{r}_{\text{MCK}} = \underbrace{\lambda}_{\text{Mg Cost}} \underbrace{s_L(s_L L^\rho + s_K K^\rho)^{\frac{1}{\rho}-1}K^{\rho-1}}_{\text{MPK}=\frac{\partial Q}{\partial K}}$$

$$\frac{1}{\frac{\partial Q}{\partial K}} = \frac{\lambda}{MCK} = \frac{\lambda}{r} \tag{5}$$

Given that Q depends on K and L, I can write the derivative of Q as:

$$\frac{\partial Q}{\partial \tau} = \frac{\partial Q}{\partial L}\frac{\partial L}{\partial \tau} + \frac{\partial Q}{\partial K}\frac{\partial K}{\partial \tau}$$

$$\frac{\partial L}{\partial \tau}/\frac{\partial Q}{\partial \tau} = \frac{\partial L}{\partial \tau}/\left(\frac{\partial Q}{\partial L}\frac{\partial L}{\partial \tau} + \frac{\partial Q}{\partial K}\frac{\partial K}{\partial \tau}\right) = \frac{\partial L}{\partial \tau}/\left(\frac{\mu_L}{\lambda}\frac{\partial L}{\partial \tau} + \frac{r}{\lambda}\frac{\partial K}{\partial \tau}\right) \tag{6}$$

$$\frac{\partial K}{\partial \tau}/\frac{\partial Q}{\partial \tau} = \frac{\partial K}{\partial \tau}/\left(\frac{\partial Q}{\partial L}\frac{\partial L}{\partial \tau} + \frac{\partial Q}{\partial K}\frac{\partial K}{\partial \tau}\right) = \frac{\partial K}{\partial \tau}/\left(\frac{\mu_L}{\lambda}\frac{\partial L}{\partial \tau} + \frac{r}{\lambda}\frac{\partial K}{\partial \tau}\right) \tag{7}$$

where the last equalities in 6 and 7 come from the optimal input choices, as depicted in equations 4 and 5. Plugging 6 and 7 back into 3:

$$dW = \frac{\partial L}{\partial \tau}\left[\frac{w}{\epsilon} + w\left(\frac{\epsilon+1}{\epsilon}\right)\tau\right] + \frac{\partial Q}{\partial \tau}\left[\frac{Q^{\frac{-1}{\eta}}}{\eta}\right] \tag{8}$$

The first term in Equation 8 depicts the deadweight loss originated from the labor market, while the second term depicts the deadweight loss originated from the product market. The terms outside the brackets are the behavioral responses, which capture the deadweight loss from quantity distortions. The terms inside the first bracket are the monopsony wedge, and the payroll tax wedge, respectively. The term inside the second bracket captures the monopoly wedge due to price markup. If we take the limit of ϵ and η to infinity, Equation 8 reduces to the standard textbook deadweight loss formula.

E Revenue Maximizing Payroll Tax Rate

The payroll tax rate variation induces a mechanical change on Governments' revenue, measured by $dM = B(\tau_1 - \tau_0)$. It also induces a behavioral response to tax revenue given the labor supply responses induced by the policy: $H = \tau B(\frac{\epsilon+1}{\epsilon})\frac{\partial L}{\partial 1+\tau}\frac{1}{L}$. To compute the behavioral effect we rely on the empirically estimated employment response ($\frac{\partial L}{\partial 1+\tau}$). Note that this response is locally estimated. To extrapolate the counterfactual employment response at hypothetical tax rates far from the observed level, I undertake a Taylor expansion, with rates varying from τ_0 to τ_1:

$$\frac{\partial L}{\partial 1+\tau}(1+\tau_1) = \frac{\partial L}{\partial 1+\tau}(1+\tau)\Big|_{\tau=\tau_0} + \frac{\partial L}{\partial^2 1+\tau}(1+\tau)\Big|_{\tau=\tau_0}(\tau_1-\tau_0) + \frac{1}{2}\frac{\partial L}{\partial^3 1+\tau}(1+\tau)\Big|_{\tau=\tau_0}(\tau_1-\tau_0)^2 + \dots \quad (1)$$

$$\frac{\partial L}{\partial 1+\tau}(1+\tau_1) = \frac{L\epsilon_{L,1+\tau}}{1+\tau}\Big|_{\tau=\tau_0}\Bigg[1 + \frac{(\epsilon_{L,1+\tau}-1)}{1+\tau}\Big|_{\tau=\tau_0}(\tau_1-\tau_0) + \frac{1}{2(1+\tau)^2}\Big|_{\tau=\tau_0}(\epsilon_{L,1+\tau}(\epsilon_{L,1+\tau}-1)+2)(\tau_1-\tau_0)^2\Bigg] \quad (2)$$

In counterfactual scenarios, where the payroll tax rate moves to τ_1, I compute the behavioral response by evaluating $dH = \tau B(\frac{\epsilon+1}{\epsilon})\frac{\partial L}{\partial 1+\tau}\frac{1}{L}$ at the counterfactual employment response delineated in Equation 2. With this framework, I simulate the revenue impact of perturbing the labor tax rate. Figure E.1 presents a shape similar to the so-called, *Laffer curve*, and shows that the Brazilian tax revenue would be maximized if the labor tax rate were 56%.[21]

Figure E.1 plots the net effect of two opposing forces at play when the payroll tax rate is increased. On one hand, the mechanical effect increases tax revenue. On the other hand, the behavioral response decreases tax revenue, as the tax rate is increased. This curve illustrates the zone where the mechanical effect outweighs the behavioral response, thereby enabling us to visually observe the revenue maximizing rate.

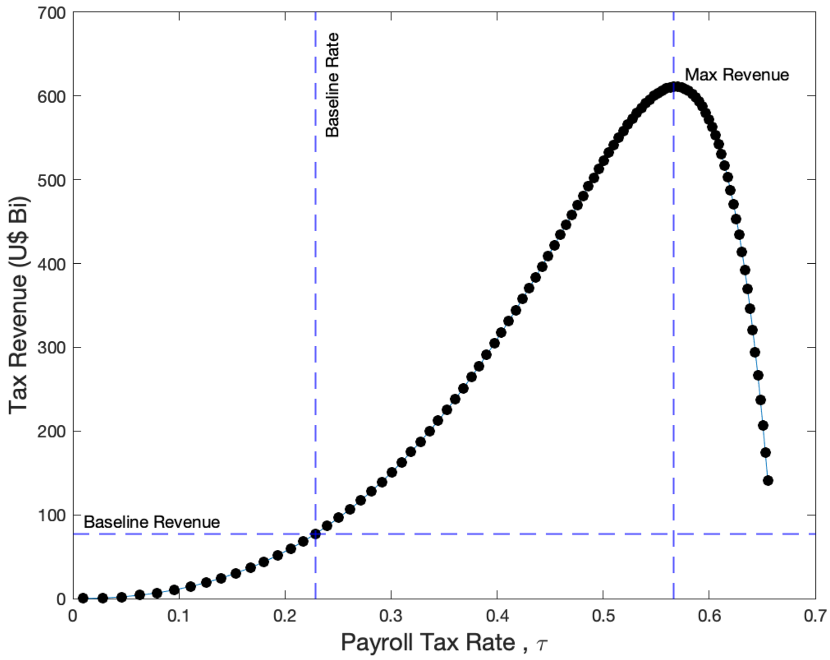
One immediate takeaway from this exercise is that payroll tax rates in Brazil are fairly far from the revenue-maximizing rate, which is indicative that existing average tax rates are on the "right side of the Laffer curve". The direct conse-

[21] Alternatively, this could be expressed as a firm's payroll tax rate of 130%. There is a one-to-one relationship between the firms' and workers' take-home tax rates:

$$\underbrace{\frac{wL}{1-t}}_{\text{Received by worker}} = \underbrace{wL(1+\tau)}_{\text{Paid by firm}}$$

quence is that Brazilian policymakers can increase the payroll tax without fearing a decline in tax revenue. This conclusion is further supported by the positive MVPF reported in Table 4.

Figure E.1: Laffer Curve for Payroll Taxation



Note: This figure plots the "Laffer curve" for the Brazilian payroll tax system. As we simulate increases in the payroll tax rate, there are two opposing forces: mechanical and behavioral effects. When payroll tax rates are increased, the behavioral response prompts a drop in revenue as a result of adjustments in labor supply. This curve illustrates the zone where the mechanical effect outweighs the behavioral response, thereby enabling us to visually observe the revenue maximizing rate.

F Capital-Skill Complementarity

Inequality in modern society is not only persistent, but it has also risen over time, a concern emphasized by Saez and Zucman 2019. An array of recent research, including studies by Katz and Murphy 1992 and Autor et al. 2020, explores this escalating phenomenon through the perspective of capital-skill complementarity. This theory suggests that capital and skilled labor are complementary inputs, with technological advancements increasingly benefiting skilled workers. To examine the plausibility of this theory, I leverage the quasi-experimental payroll tax variation in an extension of the model that includes two types of labor.

Extended Model. Consider two types of workers, say high (L_h) and low skill (L_l). Consequently, a firm's production decisions are now based on three inputs: high-skilled labor (L_h), low-skilled labor (L_l), and capital (K). We maintain the constant elasticity of substitution (CES) technology with constant returns but introduce an additional nesting layer to the model.

$$f = (s_{hl}(s_h L_h^{\rho} + s_l L_l^{\rho})^{\frac{\gamma}{\rho}} + s_k K^{\gamma})^{\frac{1}{\gamma}}$$

where, s_{hl} is the labor (high plus low skill) share; ρ is the parameter driving the substitution across the two types of workers. Consider the high and low skill labor supply elasticity given respectively by,

$$w_h = A_h L^{\frac{1}{\epsilon_h}}$$

$$w_l = A_l L^{\frac{1}{\epsilon_l}}$$

where, ϵ_i represents the labor supply of worker type i $\in (l, h)$. Note from the minimization program that marginal productivity of high-skill labor is,

$$f_{lh} = f^{1-\rho} L_h^{1-\gamma} (s_h L_h^{\rho} + s_l L_l^{\rho})^{\frac{\gamma}{\rho}-1} s_{hl} s_h L_h^{\rho-1}$$

By examining the optimal decisions of firms, I can calculate the demand for high and low-skilled labor. More importantly, I derive the labor cost pass-through for each type of labor, as a function of the auxiliary elasticities ($\epsilon^{\lambda}_{1+\tau}$, ϵ^{λ}_{Q}, $\epsilon^{Q}_{1+\tau}$):

$$\epsilon^{L_h}_{1+\tau} = \frac{1 + \frac{s_l}{s_h}(\frac{L_l}{L_h})^{\rho}}{1 + \frac{s_l}{s_h}(\frac{L_l}{L_h})^{\rho} - (\gamma - \rho)} \left[\frac{\epsilon_h}{1 + \epsilon_h(1-\rho)} (\epsilon^{\lambda}_{1+\tau} + \epsilon^{\lambda}_{Q} \epsilon^{Q}_{1+\tau}) - 1 + (1-\rho)\epsilon^{Q}_{1+\tau} + \frac{(\gamma - \rho)\epsilon^{L_l}_{1+\tau}}{1 + \frac{s_h}{s_l}(\frac{L_h}{L_l})^{\rho}} \right] \tag{1}$$

$$\epsilon^{L_l}_{1+\tau} = \frac{1 + \frac{s_h}{s_l}(\frac{L_h}{L_l})^{\rho}}{1 + \frac{s_h}{s_l}(\frac{L_h}{L_l})^{\rho} - (\gamma - \rho)} \left[\frac{\epsilon_l}{1 + \epsilon_l(1-\rho)} (\epsilon^{\lambda}_{1+\tau} + \epsilon^{\lambda}_{Q} \epsilon^{Q}_{1+\tau}) - 1 + (1-\rho)\epsilon^{Q}_{1+\tau} + \frac{(\gamma - \rho)\epsilon^{L_h}_{1+\tau}}{1 + \frac{s_l}{s_h}(\frac{L_l}{L_h})^{\rho}} \right] \tag{2}$$

To compute the auxiliary elasticities and obtain a closed form solution for the labor elasticities with respect to the labor cost, I start by re-writing the cost function in terms of the marginal productivity of each input, and the marginal cost. Standard envelope arguments enable me to compute ($\epsilon^{\lambda}_{1+\tau}$, ϵ^{λ}_{Q}, $\epsilon^{Q}_{1+\tau}$), and obtain an expression for the labor cost pass-through as a function of observables and structural parameters.

$$\epsilon^{L_h}_{1+\tau} = C_h\left[\left(K_h\left(\frac{\epsilon_h + 2\epsilon^{L_h}_{1+\tau}}{\epsilon_h}\right)+K_l\left(\frac{\epsilon_l + 2\epsilon^{L_l}_{1+\tau}}{\epsilon_l}\right)\right)\left(\frac{\epsilon_h}{1+\epsilon_h(1-\rho)}-(1-\rho)\eta\right)-1+\frac{(\gamma-\rho)\epsilon^{L_l}_{1+\tau}}{1+\frac{s_h}{s_l}(\frac{L_h}{L_l})^{\rho}}\right] \tag{3}$$

$$\epsilon^{L_l}_{1+\tau} = C_l\left[\left(K_l\left(\frac{\epsilon_l + 2\epsilon^{L_l}_{1+\tau}}{\epsilon_l}\right)+K_h\left(\frac{\epsilon_h + 2\epsilon^{L_h}_{1+\tau}}{\epsilon_h}\right)\right)\left(\frac{\epsilon_l}{1+\epsilon_l(1-\rho)}-(1-\rho)\eta\right)-1+\frac{(\gamma-\rho)\epsilon^{L_h}_{1+\tau}}{1+\frac{s_l}{s_h}(\frac{L_l}{L_h})^{\rho}}\right] \tag{4}$$

where,

$$K_h = \frac{-1}{\frac{1}{s_{L_h}} + \frac{W_l}{W_h}\frac{1}{\epsilon_l} + \frac{1}{\epsilon_h}}\left(\frac{\epsilon_h + 1}{\epsilon_h}\right) \qquad K_l = \frac{1}{\frac{1}{s_{L_l}} + \frac{W_h}{W_l}\frac{1}{\epsilon_h} + \frac{1}{\epsilon_l}}\left(\frac{\epsilon_l + 1}{\epsilon_l}\right)$$

$$C_h = \frac{1+\frac{s_l}{s_h}(\frac{L_l}{L_h})^{\rho}}{1+\frac{s_l}{s_h}(\frac{L_l}{L_h})^{\rho}-(\gamma-\rho)} \qquad C_l = \frac{1+\frac{s_h}{s_l}(\frac{L_h}{L_l})^{\rho}}{1+\frac{s_h}{s_l}(\frac{L_h}{L_l})^{\rho}-(\gamma-\rho)}$$

For the effect on capital and revenue, not very different from the main model specification with one type of labor, I find:

$$\epsilon^{K}_{1+\tau} = \left(K_h\left(\frac{\epsilon_h + 2\epsilon^{L_h}_{1+\tau}}{\epsilon_h}\right)+K_l\left(\frac{\epsilon_l + 2\epsilon^{L_l}_{1+\tau}}{\epsilon_l}\right)\right)\left(\underbrace{\frac{1}{1-\gamma}}_{subst}\underbrace{-\eta}_{scale}\right) \tag{5}$$

$$\epsilon^{R}_{1+\tau} = (1-\eta)\left[K_h\left(\frac{\epsilon_h + 2\epsilon^{L_h}_{1+\tau}}{\epsilon_h}\right)+K_l\left(\frac{\epsilon_l + 2\epsilon^{L_l}_{1+\tau}}{\epsilon_l}\right)\right] \tag{6}$$

The associated elasticity of substitution between low and high skill labor is:

$$\sigma_{LH} = \frac{1}{1-\rho}$$

Identification. In this augmented model, it is not feasible to obtain closed-form analytical solutions for all the structural parameters as functions of the reduced-form estimates. The notable exceptions are the labor supply elasticities (ϵ_h and ϵ_l), which can be directly computed from the employment and wage responses for each type of worker. To structurally estimate the parameters ρ, γ, and η, I employ the Classical Minimum Distance (CMD) approach. The CMD methodology is a non-parametric technique that draws on the moment conditions outlined in equations 3, 4, 5, and 6. Formally, the program solves, $\min_{\beta}[\hat{\beta} -$

$\xi(\beta)]'\hat{W}^{-1}[\hat{\beta} - \xi(\beta)]$, where $\xi(\beta)$ is the vector of model predictions, and $\hat{\beta}$ is the vector of reduced-form estimates. Given the availability of four moments to estimate three parameters, it's possible to assess the validity of the model by conducting a J-test for overidentification. The null hypothesis posits that the model is correctly specified. Notably, a J-test yielding a p-value of 0.86 provides support for the null. Table F.1 reports the structural estimates.

Structural Estimation. The elasticity of substitution between high and low-skill workers is tighly estimated at 1.27, corroborating the extensive literature that endorses the concept of capital-skill complementarity. This estimate sits comfortably within the range surveyed by Hamermesh 1996, and micro studies that found 1.5 (Johnson 1997), and 1.67 (Krusell et al. 2000). The smaller earnings pass-through to low skill workers identify greater elasticities, implying that firms exert greater labor market power over high-skilled workers. While initially, this finding might seem counterintuitive, it aligns with the fact that there are relatively fewer firms hiring in the high-skill market. I find that labor market concentration, proxied by HHI, is 32% greater in the high-skill labor market, reinforcing that unskilled labor operates more as in a commodity market. Such logic rationalizes extensive empirical evidence on the unequal pass-through presented on this paper. Table F.1 summarizes the results.

Policy Implication. Indeed, understanding the dynamics between skilled and unskilled labor is important for policy implications, as highlighted by Krusell et al. (2000). For example, increasing trade barriers to protect domestic unskilled labor may not be effective if foreign low-wage labor is not the only competitor. Other factors such as automation and technological advancements also play a significant role in the substitution dynamics of labor. Domestic unskilled labor also faces competition from increasingly affordable and advanced capital equipment. Therefore, a more impactful policy for combating inequality might be an investment in basic education, as posited by numerous studies and corroborated in the Brazilian context. By enhancing workers' skills, they can utilize new equipment and increase their productivity, reducing the risk of being replaced by machinery.

Table F.1: Structural Estimation *(Extended Model)*

Structural Elasticities	(1) **Baseline**
Low-High Skill Elasticity, σ_{LH}	1.27
	(0.04)
High Skill Labor Supply, ϵ_H	3.58
	(1.32)
Low Skill Labor Supply, ϵ_L	6.01
	(2.54)
Output Demand Elasticity, η	1.20
	(0.07)
Empirical Estimates	
High Skill Employment, β_{L_H}	0.14
Low Skill Employment, β_{L_L}	0.12
High Skill Earnings, β_{W_H}	0.04
Low Skill Earnings, β_{W_L}	0.02
Capital, β_K	-0.04
Revenue effect, β_R	0.05
Cost Shares	
High Skill Labor	0.12
Low Skill Labor	0.68
Capital	0.20
J-test	
Overid test (pvalue)	0.86

Notes: This table presents estimates based on the extended model with two types of labor. In the empirical section, the table displays coefficients empirically estimated, and used for the structural estimation. At the bottom, the table displays the p-values associated with the J-test for overidentification. The standard errors for the labor supply elasticities are directly computed from the reduced form estimates, which rely on the Delta Method. In contrast, the standard errors for the remaining structural elasticities are computed using the bootstrap method.

G Robustness Checks

This section presents additional robustness tests to further validate the findings from the main empirical analysis. These exercises help address potential concerns related to sample selection and empirical assumptions. Regarding sample restrictions, there may be concerns that our primary results are influenced by changes in firm composition, namely their initiation and dissolution. To mitigate this, I reapply the empirical analysis on a balanced sample. In terms of identification assumptions, we broaden our approach beyond the assumed exogenous legal variations and re-conduct the empirical study using a matched difference-in-differences methodology, which relies on the conditional independence assumption (CIA). It is noteworthy that across these alternative tests, all findings remain qualitatively the same.

G.1 Balanced Sample

The balanced sample is comprised of firms that consistently appear in the data across all sample years from 2008 to 2017. If anything, these point estimates are slightly above compared to the main estimates. However, balanced and unbalanced estimates are statistically indistinguishable from each other.

Table G.1: Within-Firm Earnings Inequality

	Log(Earnings)				Occup Pctile
	firm (99p) (1)	firm (90p) (2)	firm (40p) (3)	firm (20p) (4)	firm level (5)
Panel A: IV					
Diff-in-Diff	.054***	.025**	.015	.007	.001
	(.015)	(.013)	(.011)	(.01)	(.003)
Long Diff	.082***	.038***	.016	-.002	.003
	(.017)	(.013)	(.011)	(.01)	(.003)
Controls	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓
Sector x Year FE	✓	✓	✓	✓	✓
# Clusters	7,924	7,924	7,924	7,924	7,921
N	2,491,523	2,491,523	2,491,523	2,491,523	2,491,146

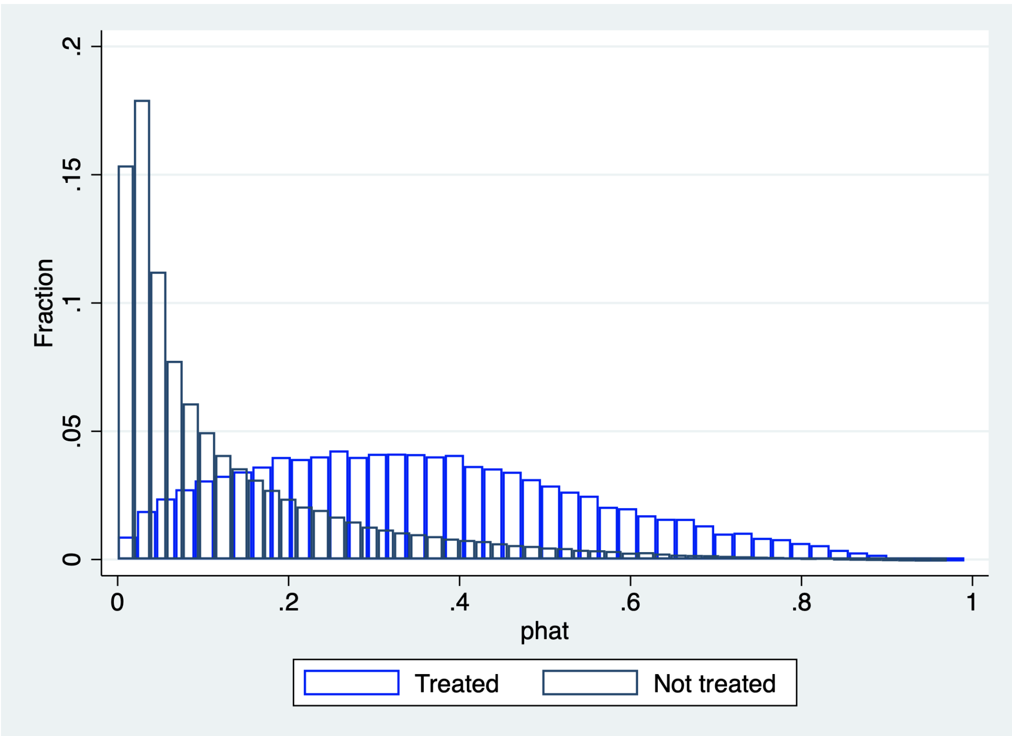
Note: This table presents IV estimates for the causal impacts of the reform on outcomes labeled on each column for the balanced sample. The instrument is the sector eligibility. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

G.2 Matched Sample

I follow extensive theoretical (Cochran and Rubin 1973; Rosenbaum and Rubin 1984; Ho et al. 2007) and applied (Campos and Kearns 2022) literature that propose matching methods to deal with potential imbalances at baseline.

Procedure. To ensure that pre-trends are not mechanically satisfied, the matching occurs only in two out of the four pre-reform years (2010 and 2011). The procedure goes as follows: each eventually treated firm matches a never treated one that belongs to a non-eligible sector and shares the same pre-reform deciles on average employment, workers' earnings, firm age, net revenue, and profits. In the case of multiple control firms matching the same treated one, I use propensity score to break ties. To compute the propensity score, I fit a logit in the pre-reform period to predict treatment status based on a vector of observables such as $\log$ of employment, wage bill, gross revenue, payroll taxes, profit, and some labor force average characteristics such as age, race, gender, and education. A coefficient ($\hat{\beta}$) is then estimated for each firm, enabling the calculation of the propensity score: $\hat{p} = \frac{\exp\{\hat{\beta}\}}{1+\exp\{\hat{\beta}\}}$. The distribution of propensity scores across the sample is illustrated in Figure G.1. The noticeable overlap between groups provide evidence of support across the estimated propensity score distribution, validating the matching procedure.

Figure G.1: Histogram of Propensity Scores



Note: This histogram plots the propensity-score overlap between eventually and never treated firms. The propensity scores are computed in the pre-reform years, and it is based on a logit regression of treatment status on firm-level characteristics.

Balance. The matched sample consists of 30,761 firms in each group. These are firms that appear at least once in the pre-reform years and have a matched counterpart that satisfies the matching conditions. Table G.2 presents descriptive statistics for both treated and control firms within the matched sample during the pre-reform years. The top five rows report variables used in the matching procedure. Noticeably, the balance holds even across dimensions that were not directly targeted. For instance, for both groups, payroll tax rates are about 34%, the total wage bill BRL 0.85 million, the average worker's age is 33.8 years, 70% are male, 73% are white, 60% have completed high school, and 12% have a college education. The minor discrepancies between the groups do not reach statistical significance at conventional confidence levels, for any characteristic.

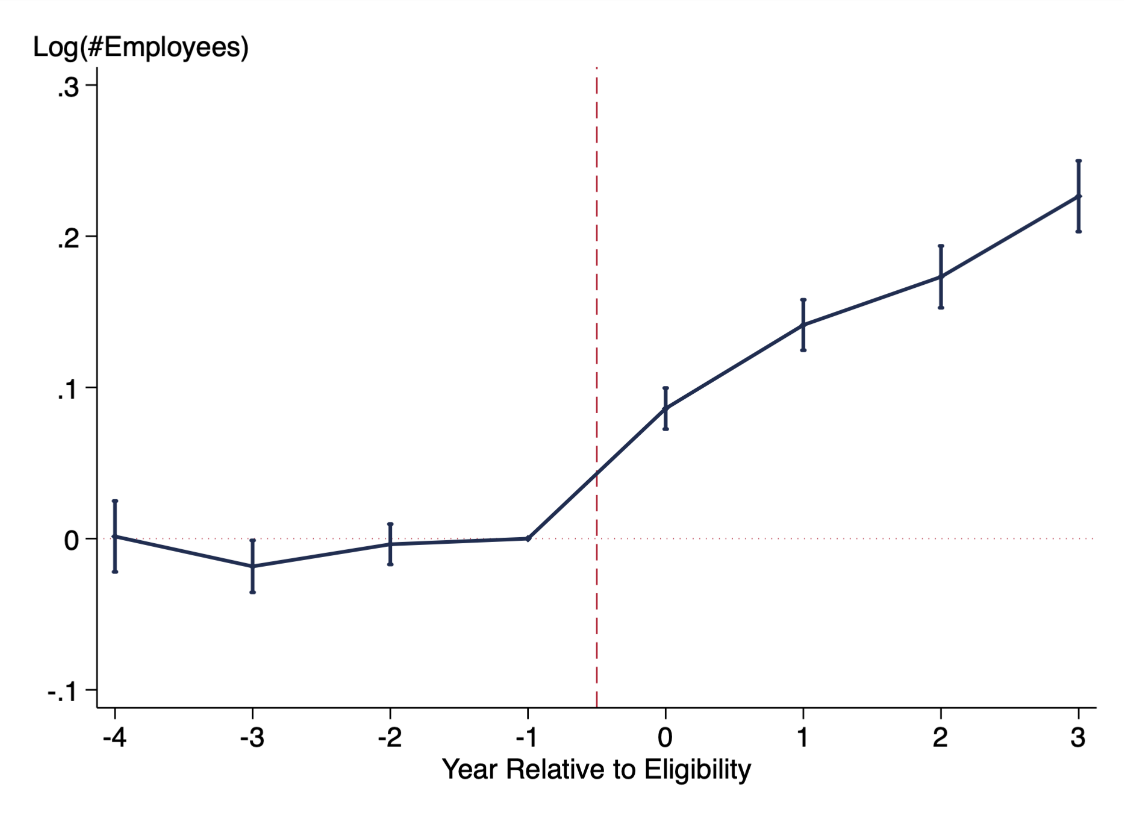
Table G.2: Balance on Matched Sample

	Treatment	Control
Employment	45.70	45.69
	(48.21)	(48.38)
Avg Monthly Earnings	1,461.83	1,449.26
	(1,086.82)	(1,348.23)
Firm Age	11.51	11.52
	(11.80)	(11.67)
Capital (Mil)	11.28	11.64
	(17.50)	(18.16)
Gross Revenue (Mil)	37.63	38.32
	(48.28)	(49.29)
Ebit (Mil)	1.40	1.42
	(3.98)	(4.08)
Payroll Tax Rate	0.34	0.33
	(0.08)	(0.09)
Total Payroll Tax (Mil)	1.29	1.39
	(8.94)	(19.59)
Total Wage Bill (Mil)	0.86	0.84
	(0.90)	(0.90)
Age	33.80	33.86
	(5.65)	(5.64)
Gender	0.73	0.70
	(0.26)	(0.29)
Share White	0.76	0.73
	(0.27)	(0.28)
Share High School +	0.61	0.60
	(0.31)	(0.33)
Share College +	0.11	0.12
	(0.21)	(0.21)
Observations	30761	30761

Note: This table provides mean characteristics for eventually treated versus never treated firms in the pre-period. Each observation depicts is a unique firm, which will be followed over time.

Results. I follow treated and control firms over time and estimate the difference-in-differences outlined in equations 3 and 4. The results are qualitatively similar to the main specification, which validates the empirical findings. Notably, the pre-trends in the matched sample are not statistically significant, as shown in Figure G.2. It is worth noting that this is not entirely attributable to a mechanical consequence of the matching procedure itself, as only two out of four pre-reform years are used in the matching.

Figure G.2: Event Study on Matched Sample



Note: This figure presents the event study estimates for the log of employment estimated at the matched sample. In this sample, firms are matched based on pre-reform characteristics in the years of 2010 and 2011. Standard errors are clustered at the firm-level.

Placebo. To further validate the matching design, I conducted a placebo test, randomly assigning firms to treatment, and applied the same matching procedure based on this fake treatment assignment. Given the absence of real tax variation in the fake treatment bucket, we should expect to see zero effects in this analysis. This is precisely what Table G.4 reports. To showcase that the matching algorithm still works in the placebo sample, Table G.3 shows that fake treatment and control are balanced in pre-reform characteristics. This finding provides compelling evidence that the main results in the matched sample are actual tax responses and are not mistakenly generated by the matching procedure.

Table G.3: Balance on Placebo Matched Sample

	Treatment	Control
Employment	15.46	15.29
	(33.02)	(32.50)
Avg Monthly Earnings	1,061.97	1,057.45
	(1,048.59)	(980.10)
Firm Age	13.82	13.82
	(10.93)	(11.00)
Capital (Mil)	8.71	8.58
	(16.13)	(15.86)
Gross Revenue (Mil)	26.82	26.93
	(42.55)	(42.45)
Ebit (Mil)	0.79	0.80
	(3.29)	(3.30)
Payroll Tax Rate	0.31	0.31
	(0.10)	(0.09)
Total Payroll Tax (Mil)	0.28	0.35
	(3.70)	(13.88)
Total Wage Bill (Mil)	0.27	0.27
	(0.59)	(0.58)
Age	37.12	36.41
	(8.97)	(8.83)
Gender	0.55	0.51
	(0.40)	(0.40)
Share White	0.67	0.69
	(0.37)	(0.37)
Share High School +	0.55	0.59
	(0.41)	(0.40)
Share College +	0.10	0.11
	(0.23)	(0.23)
Observations	35188	35188

Note: This table provides mean characteristics for eventually treated versus never treated firms in the pre-period. Each observation depicts is a unique firm, which will be followed over time.

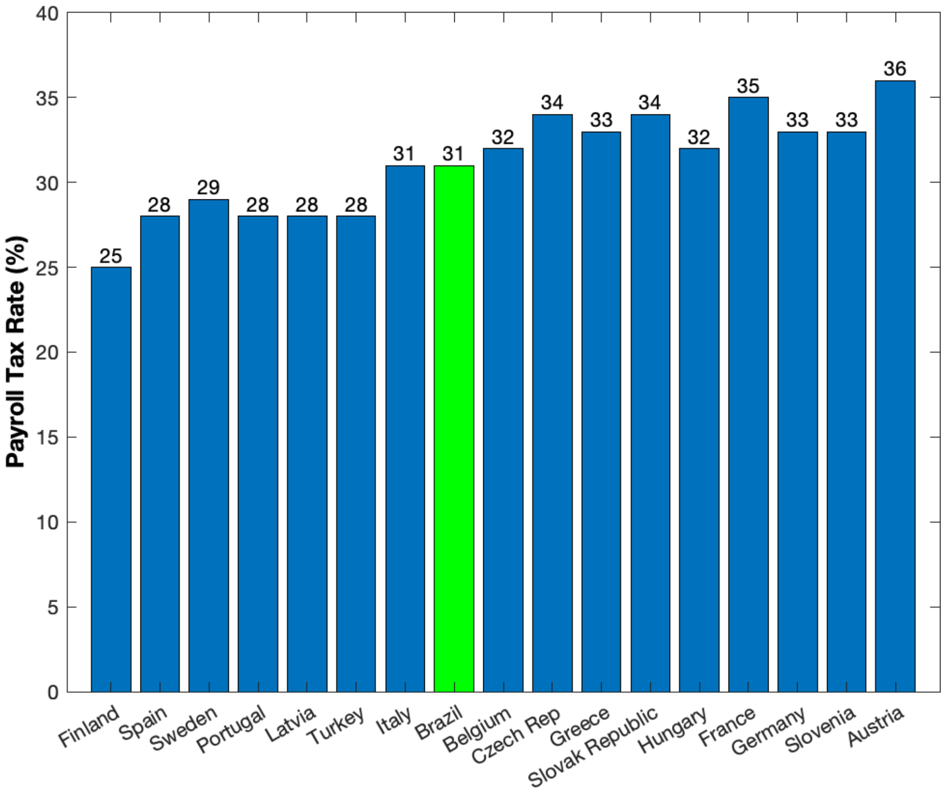
Table G.4: Reduced Form on Placebo Matched Sample

	(1) Log Labor Cost $(1+\tau)$	(2) Log Employment	(3) Log Earnings
Panel A: Diff-in-Diff			
Baseline	.0011	.0009	.0002
	(.0023)	(.0179)	(.0071)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Sector x Year FE	✓	✓	✓
N	$450,666$	$464,031$	$464,031$

Note: This table reports difference-in-differences coefficients instrumented by sector eligibility, estimated at the placebo matched sample. In this sample, randomly selected firms were assigned to a placebo treatment group, and then the same matching procedure is implemented. Given the absence of real tax variation in this fake treatment bucket, we should expect to see zero effects. Each column reports different outcomes, such as labor cost, employment, and earnings. Standard errors are clustered at the firm level and reported in parentheses.

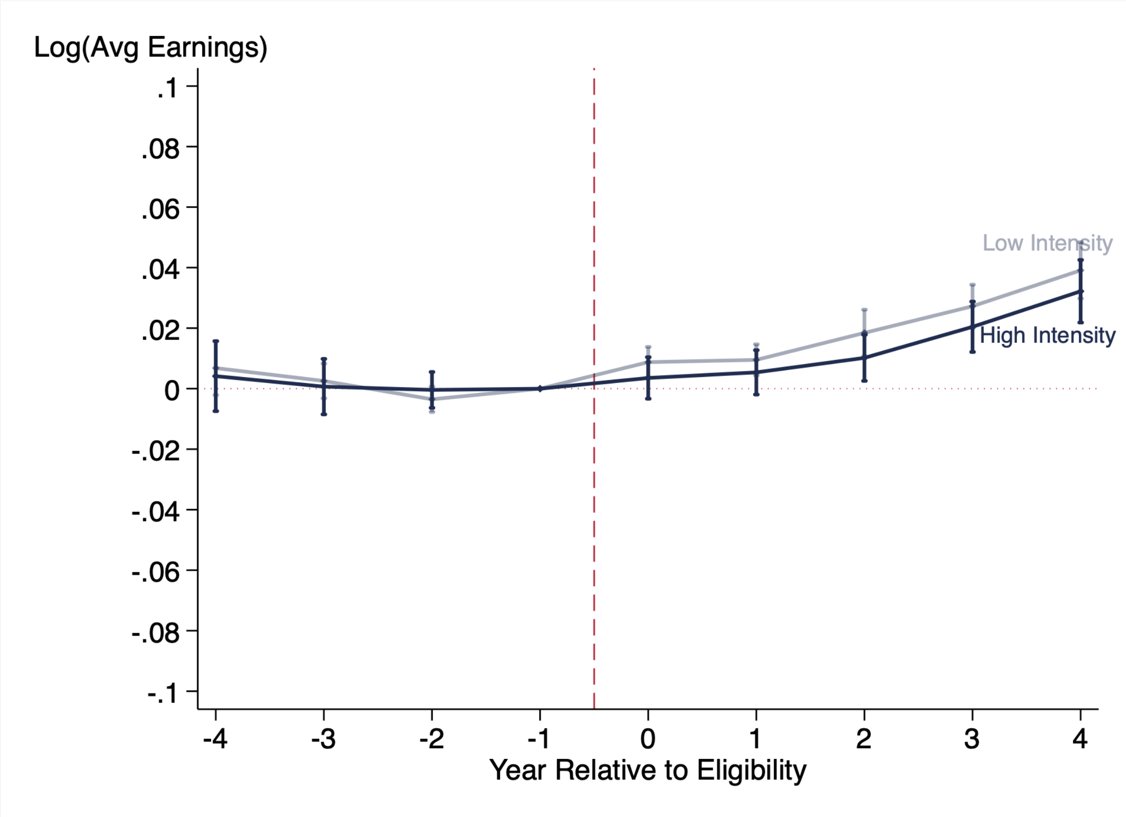
H Additional Figures and Tables

Figure H.1: Payroll Tax Rates Around the World



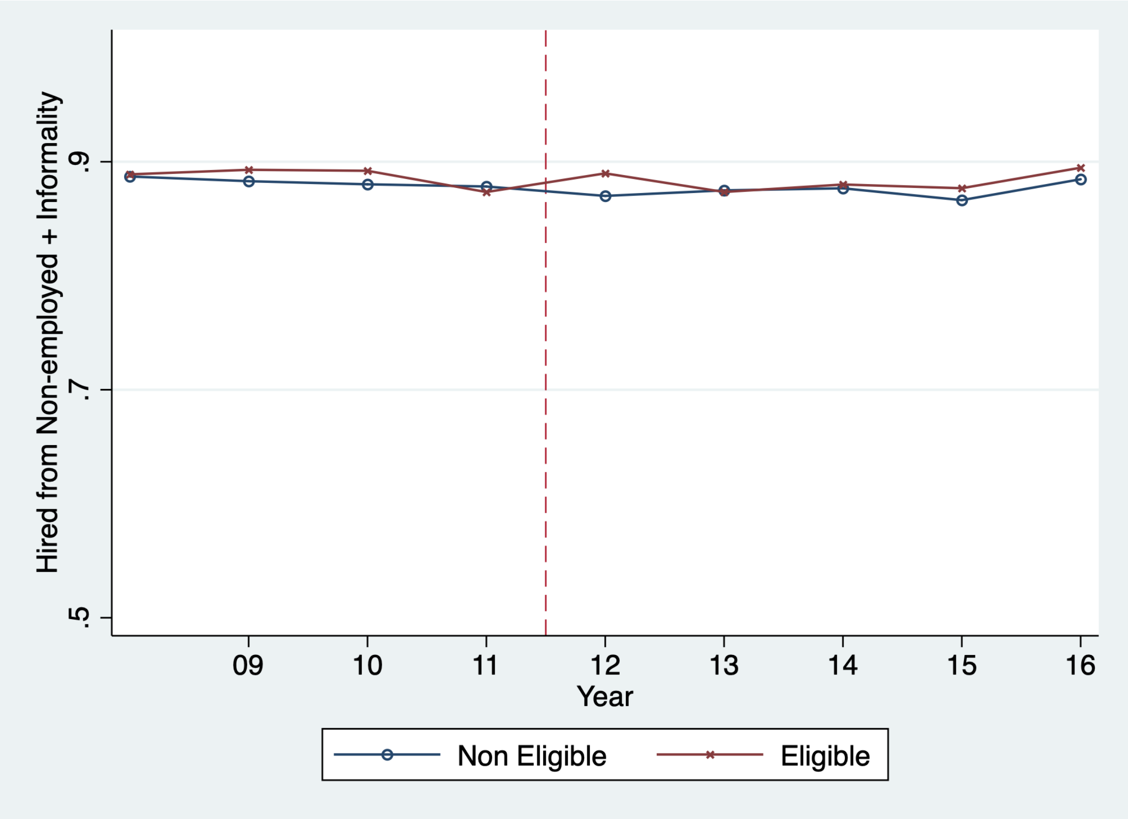
Note: This figure reports payroll tax rates around the world. The payroll tax rate is composed by the sum of employer and employee's contributions.
Source: Elaborated by author, based on information from OECD 2019.

Figure H.2: Firm vs Market Level Shock



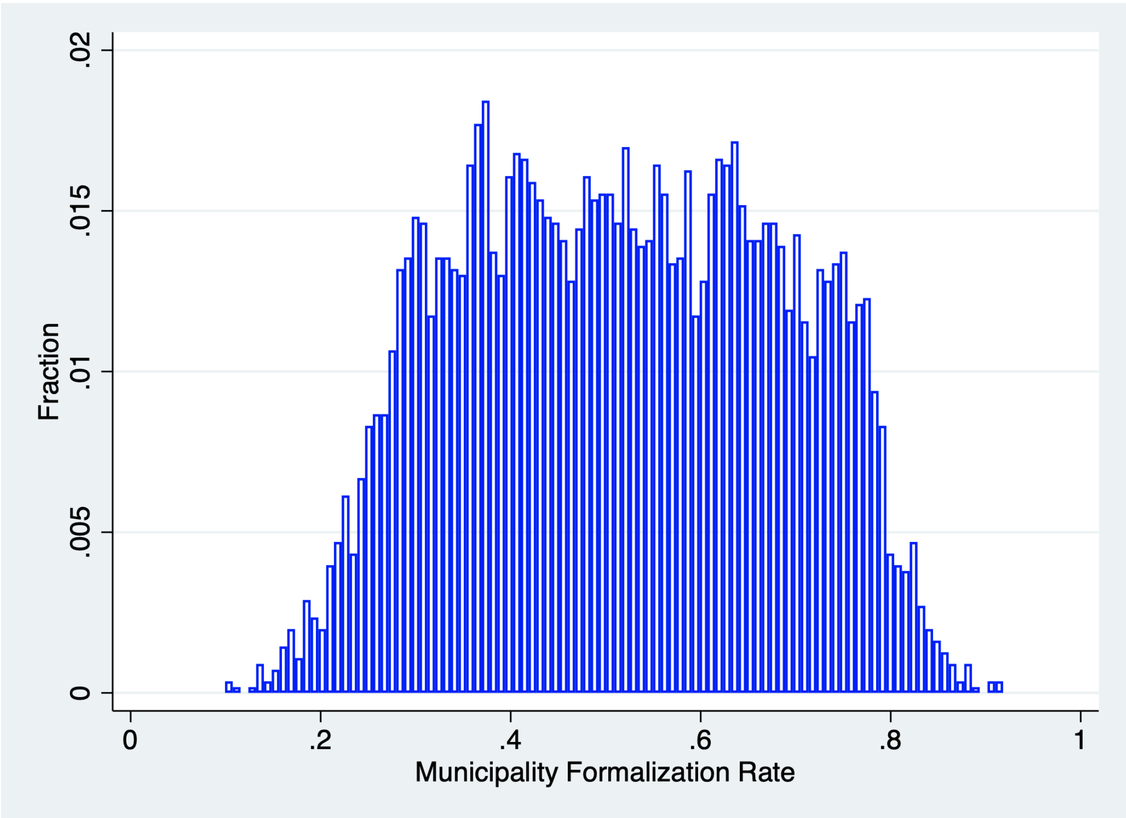
Note: This figure provides an additional test on the spillover effect. It compares the worker's earnings effect for high and low intensively treated markets. To measure market treatment intensity I compute the share of treated workers in each labor market, which are defined by the occupation x region cells. Then it separately estimates the earnings pass-through, for workers in markets below and above the median in market intensity. Standard errors are conservatively clustered at the 5-digit industry-by-state level. If the driving force for the earnings increase was a bump on workers' outside options through market spillover, we would expect to see more pass-through on high intensity markets. The figure shows no significant difference across market intensity.

Figure H.3: New Hires Origin by Eligibility Status



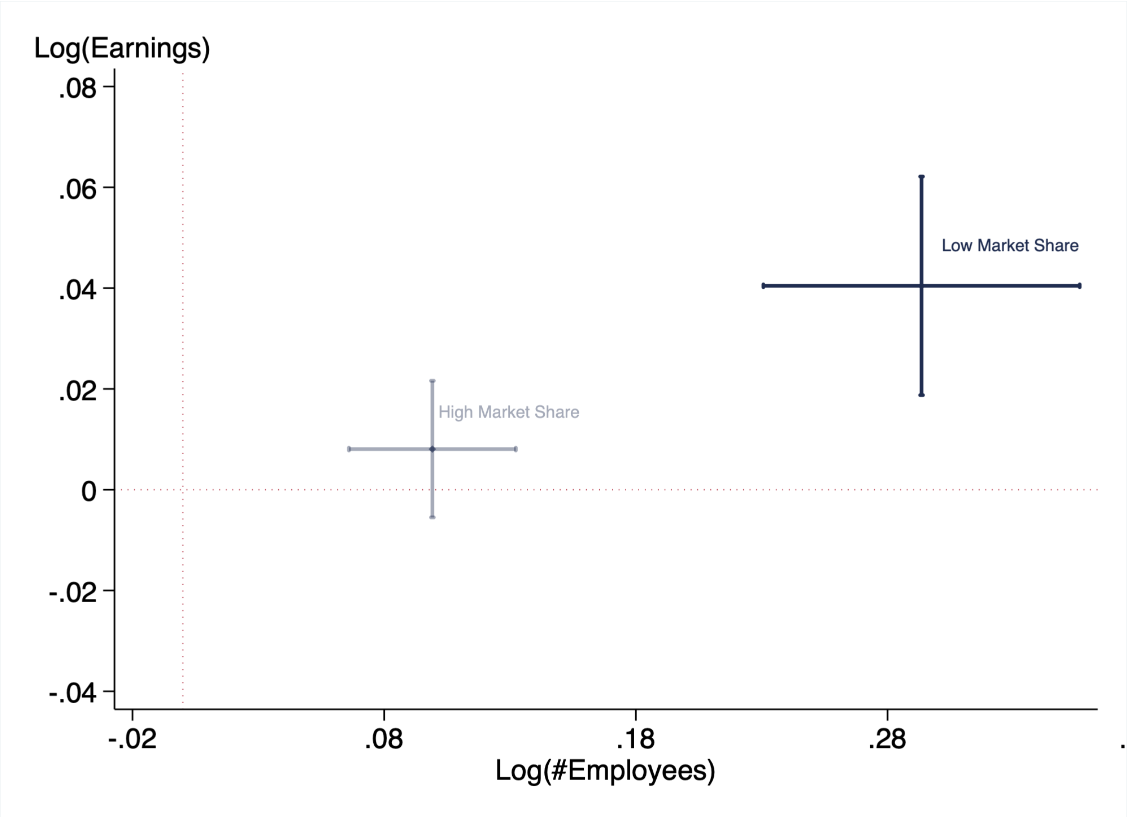
Note: This figure plots the share of new hires coming from non-employment or informality. A new hire is classified as previously informal or non-employed if she was not holding a formal job in the three months prior to being hired. Eligibility is defined based on the sector of employment.

Figure H.4: 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 H.5: Earnings and Employment per Market Concentration



Note: This figure presents firm-level IV difference-in-differences coefficients for above and below the median on pre-reform employment market share within each local labor market. These results are estimated using the comprehensive labor data described in Section 2.2. The outcomes are employment and earnings. The blue marker plots the effect for firms below the median (low market power), whereas the gray marker plots the effect for firms with high market power. Horizontal and vertical lines plot the confidence intervals for the employment and earnings estimates, respectively. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table H.1: Descriptives on Market Level Treatment

		Average
	Share of Treated Firms in a Treated Market (1)*(2)*(3)*(4)	**0.03**
(1)	Share of Treated Sectors per Market — At Least One	0.211
(2)	Formality Rate	0.550
(3)	Share of Eligible Tax Tier	0.520
(4)	Take Up Within Eligible	0.517

Note: This table breaks down the calculation of treatment share per local labor market. Row (1) reports the local labor market (LLM) share of eligible sectors, conditional on the existence at least one eligible sector on the given LLM. Row (2) reports workers' average formality rate in Brazil; row (3) reports the share of firms in the eligible tax tier; row (4) reports the take-up rate within eligible firms. The product of these 4 rows gives the share of treated firms in a treated market.

Table H.2: HHI Distribution for High and Low-Skill Occupation

HHI	High Skill Market	Low Skill Market
Average	**.338**	**.2585**
Pctile 5	.0046	.0028
Pctile 10	.014	.0076
Pctile 25	.0522	.0319
Pctile 50	.1852	.1236
Pctile 75	.5	.366
Pctile 90	1	.916
Pctile 95	1	1
Pctile 99	1	1

Note: This table shows the distribution of local labor market (LLM) concentration for high and low-skill occupations. The first line depicts the average labor market concentration, and the remaining line depict the respective concentration percentiles. LLM are granularly defined based on cells of 2-digits occupation $\times$ commuting zone.

Table H.3: Descriptive Statistics for Stable Workers

	Mean			
	mean	p25	p50	p75
Descriptives on Stable Workers				
Probability Change Jobs	0.07	0.00	0.00	0.11
Share that Ever Change Jobs	0.33	0.00	0.00	1.00
Avg # Years in Sample	8.31	7.00	9.00	10.00
Avg # Years per Firm	7.13	5.00	8.00	10.00

Note: This table presents descriptive statistics about job attachment of stable workers. For each measure of job stability, the table reports mean, 25^{th}, 50^{th}, and 75^{th} percentiles.

Table H.4: Comparison Across Methods

		Direct Estimation		*CMD*	
	(1)	(2)	(3)	(4)	(5)
Structural Estimates	**Baseline**	**Small Firms**	**Large Firms**	**Small Firms**	**Large Firms**
Labor Supply Elasticity, ϵ	4.15	5.75	4.25	5.75	4.25
	(1.63)	(2.65)	(2.23)	(0.33)	(0.28)
Labor-Capital Elasticity, σ_{KL}	1.72	5.01	1.25	5.01	1.25
	(0.57)	(2.95)	(0.56)	(0.34)	(0.08)
Output Demand Elasticity, η	1.43	6.46	1.10	5.21	0.78
	(0.29)	(2.93)	(0.22)	(4.21)	(0.06)

Notes: This table presents the parameters estimated, according to two alternative methods. In Columns (1-3) parameters were directly estimated based on seemingly unrelated regression. The advantage of this method is the clean and intuitive structural identification. In Columns (4-5) the structural estimation relies on the Classical Minimum Distance (CMD) approach, whose main advantage is providing the most efficient estimators. The estimates are fit for all firms in the baseline case, and then separately fitted for small and large firms.