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

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Abstract

This paper focuses on the role of market power to study 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, which are estimated to possess less market power. Two-thirds of the employment effect arises from plant size expansion and one-third from input substitution. Reduced-form estimates reveal that consumers pay 75% of payroll taxes, firm owners 11%, and workers 14%. Estimates of a monopsonistically competitive model of factor demand suggest that a targeted alternative tax policy focusing on small firms could amplify the efficiency gains of the tax cut by 36% and enhance workers' welfare gains by 95%. These results establish that market power not only mitigates the distortionary costs of taxation but also redistributes the tax burden from workers to firm owners and consumers.

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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 and efficiency.

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. Eligible and ineligible firms are remarkably similar and exhibited similar trends before the reform. These similar groups provide a compelling basis for comparison, which I implement in a difference-in-differences framework. 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 9% employment increase, 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 2% on average, and by 3% three years after the reform. Even though these effects could, in theory, be influenced by compositional changes, I detect no evidence of such adjustments. These results provide clear evidence 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 12% 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. There are two threats to the validity of this design. The first relates to selection on eligibility, i.e., that Congress anticipated sector-specific trends when defining eligibility. I address this concern in several ways. I show not only that pre-trends aren't statistically indistinguishable from zero in any of the outcomes, but also that eligibility is balanced in baseline levels. These results are not surprising per se, as the political process that determined eligibility often assigned remarkably similar sectors to different eligibility statuses, as illustrated by the cases of hotels and motels. Additionally, as a 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 relates to the manipulation of sectoral choice. Identification would be compromised if firms strategically select into eligible sectors after the reform has been announced. To address this concern, I first show in the data that firms rarely change sectors. This is consistent with the fact that to change sectors, a firm must provide extensive supporting documentation to multiple agencies to confirm a proper shift in its core activities. Even in the rare instances in which I do observe sector changes, there is not a trend of switching toward eligible sectors. As an additional robustness check, I restrict the sample to firms that have never changed sectors and the results remain similar.

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 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 studies in industrial policies that document similar findings (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; Kroft et al. 2020; Lamadon et al. 2022), as well as with findings by Lagos 2019 in Brazil. 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, two-thirds of the employment response is due to firms boosting their scale and the remaining one-third to capital-labor substitution. Relative to the baseline, I demonstrate that for smaller firms, the scale effect increases from 6% to 11%. The greater expansion is primarily a consequence of their limited market power, which allows for more growth without exerting excessive pressure on prices. Also, a full-incidence analysis indicates that consumers pay 75% of payroll taxes, workers 14%, and firm owners 11%. 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 $0.44 in efficiency gains. This relates to a marginal value of public funds (MVPF) of 1.44, 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.

I propose an alternative policy that targets tax cuts for small firms, which are estimated to possess less market power. This exercise informs policy makers about winners and losers, as well as the overall effect of such counterfactual policy. I find that workers' welfare gains would be amplified amplified by 95%, driven primarily by greater scale expansion, which moves firms further along the labor supply curve. The lack of product market power limits the potential for price reduction in response to decreased labor costs. As a result, consumers' benefits shrink by 61% compared with the baseline. As small firm owners possess negligible power to capture rents, the alternative policy channels less welfare toward entrepreneurs. This exercise elucidates how market power can shifts the incidence toward workers and away from consumers and firm owners. In terms of efficiency, the proposed policy generates a 36% welfare increase, echoing the principle of the "second-best", which suggests that distortions introduced by market power can indeed mitigate tax-induced distortions.

Literature and Contributions. The paper's main contribution is to offer theoretical and empirical evidence on the role of market power for tax incidence and efficiency. Also, 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 policy, while I study a firm-specific variation, which allows me to more 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). Studies on the Brazilian payroll tax reform (Dallava 2014, Scherer 2015, Baumgartner et al. 2022) differ in several ways. They rely on aggregated sector data,[1] and do not analyze business outcomes such as capital, revenue, and profits. I break new ground by examining heterogeneities across different types of firms. From a theoretical perspective, this work contributes by delving into mechanisms, estimating structural parameters, proposing and evaluating alternative policies for the first time.

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; Lamadon et al. 2022; Lagos 2019; Jäger and Heining 2022; Kline et al. 2019; Garin and Silvério 2019; Benmelech et al. 2022; Burdett and Mortensen 1998; Azar et al. 2022). 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. Recent work by Kroft et al. 2020 incorporates labor and product market power within a procurement setting in the construction sector. In contrast, I explore the interplay between "double market power" and tax pass-through, revealing implications for the deadweight loss and incidence of taxation. As argued by Manning 2021, a 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.

This study also advances the literature by estimating elasticities of substitu-

[1] One advantage of anonymized firm-level tax data is that it allows for observation of the two margins of imperfect compliance: eligible firms that do not take-up, and those that are treated due to the product criteria. By considering these aspects of imperfect compliance, the mixed findings regarding the employment and wage effects of the Brazilian reform can be reconciled.

tion between capital and labor (Karabarbounis and Neiman 2014; Raval 2019; Doraszelski and Jaumandreu 2018; 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. To highlight the role of monopsony, I also estimate this elasticity in the analog perfectly competitive model and find a 33% bias.

Finally, an important industrial policy literature studyies 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 incentives. In addition, it posits that market power is 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. In Section 2, I discuss the institutional background and the 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, discusses mechanisms, and examines alternative policies. Section 6 concludes.

2 Institutional Background and Data

This section describes the institutional background about the payroll tax system in Brazil, and provides details about the payroll tax reform implemented in 2012. Then the section describes the main datasets used to measure the effects of payroll tax variation on various outcomes.

2.1 Brazilian Payroll Tax System and the 2012 Reform

Similarly to most OECD countries, the Brazilian payroll taxes are designed to fund social security programs, such as retirement pensions and unemployment insurance. Tax rates are also similar to other OECD countries (see Figure H.3 for cross-country comparison). In contrast to tax reforms studied in the past, the Brazilian payroll tax cut program offers unique advantages from an empirical perspective. First, the targeting was at the firm, rather than worker-level. Second, the Brazilian reform offered a large tax reduction. Third, only a few firms were affected, minimizing general equilibrium effects. 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 of them are collected from firms. The main component is a 20% flat tax over the total wage bill. Secondly, there is an accident risk insurance component that varies between 1 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 implies that the public social security system does not provide individual savings accounts, where resources are traceable and mapped to specific workers' benefits.

Policy Motivation. The tax reform was aimed at increasing the competitiveness of domestic 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 to tax benefits was associated with more contributions to political campaigns. Section 3.3 leverages 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. Important to highlight, that the research design does not assume random eligibility assignment. Instead, it posits that in the absence of the tax reform, eligible and non-eligible sectors would have followed a similar trajectory. Section 3.3 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 the Mercosur Common Nomenclature (NCM). Most of the product-eligible firms are in the manufacturing industry, but treatment due to NCM criterion is not restricted to the manufacturing sector. Indeed, all sectors in the Brazilian economy contain firms treated due to the product NCM 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.

The full list of eligible sectors can be found in Table H.1. Within broadly defined industries, the reform did not provide eligibility to all sectors. For example, the media industry becomes eligible, contemplating the sector of open television,

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

but not cable television. In the lodging industry, hotels are eligible, but motels are not. Similarly, Table A.1 provides a non-exhaustive list of numerous additional examples of similar sectors granted opposite eligibility status. 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 specific sectors at such a granular level. In the empirical analysis, I add industry-year fixed effect to leverage variation within broadly defined industries, further alleviating concerns related to other industry-specific shocks.

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

Tax Variation. On 14^{th} December 2011 Congress enacted the payroll tax cut reform that waived the main component of the payroll taxation, to a small share of sectors and products. Treated firms faced a uniform decrease in payroll tax rates, from 30 to 10 percentage points 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, the targeted firms were forced to pay a small 1 to 2.5% tax on the gross revenue. Importantly, the reform did not affect individuals' 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. Among granular eligible sectors, there are several firms 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. Additionally, firms in the "Simples" tax regime are also not subjected to payroll taxes, therefore not affected by the reform even if in eligible sectors.[4] Finally, among

[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.

[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.

firms that satisfy all the eligibility requirements, a substantial share of those do not take-up the benefit. Section 2.2 focuses on understanding the 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.2 shows that at the peak of its implementation in 2014, the payroll tax cut program has covered only a relatively small share of Brazilian sectors (9%), firms (1.7%), and workers (6%).[5] Section 3.2 provides several spillover tests supporting the view that the reform should be seen as a firm rather than a market-level shock.

2.2 Data and Descriptive Statistics

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

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 data is compiled annually and contains information on all formal job spells in the country. It uniquely identifies firms and workers based on tax codes (PIS and CNPJ, respectively), which do not change over time. The data includes 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 about informal and non-employed 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 at 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). This data includes information on the universe of corporate tax returns, including payroll and revenue taxes, as well as gross revenue, and

[5]The fact that "Simples" firms are not eligible, and there is imperfect take-up in eligible sectors contribute to the share of firms being lower 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.

capital, and profits.[6] The data structure is a panel at the firm-by-year level, ranging the years 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 the 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 not participated in the "Simples Nacional", which is a special tax tier not subjected 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, encompassing 19 out of 21 industries of the Brazilian economy. The construction industry is not included because the reform applied to construction firms on a site-specific basis, rather than at the 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. Additionally, construction was at the epicenter of the "Car Wash" operation, a massive corruption scandal uncovered during the decade of this study. Investigations revealed that economic transactions within the construction industry were heavily influenced by illicit business arrangements, leading to the bankruptcy of major construction players.

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, where over 85% of the tax 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 have engaged in multiple VAT tax reforms, which include sector and product-specific exemptions, rate changes, as well as variations in withholding policies and auditing programs. Unfortunately, I do not observe state-level tax data to control for VAT reforms. The main sample is not winsorized or balanced, but the results are robust to these procedures (see Appendix G).

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

Worker Sample. To maintain consistency between the firm and worker-level analysis, I apply the same sample 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 Szerman 2019), and impose a tenure restriction to focus on workers that have been employed by the same employer 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 that they end up working for.

Descriptive Statistics. Table A.3 provides descriptive statistics for the worker sample during the baseline period (2008 to 2011), with each observation representing a worker by year. Columns (1) and (2) present the pre-reform values for non-eligible and eligible workers, respectively. Column (3) combines these two groups and reveals that, on average, workers' monthly earnings amount to $2,315 (approximately $450 USD) as of December 31st of each year. The average age of workers is 39 years, with 67% of the sample being white, 54% male, 70% having achieved at least a high school degree, and 27% being college educated. The table reveals baseline balance across eligible and non-eligible workers. The one exception is gender, a variable that I will control for in all empirical specifications.

In the firm-level sample, there are 1,775,601 observations in the pre-reform period (2008-2011). These firms are distributed across 19 industries, which are further broken down into 1,300 seven-digit sector codes (CNAE). Table A.4 provides summary statistics for non-eligible firms (column 1), eligible firms (column 2), and the average of both (column 3). Column (4) reports the descriptive statistics for treated firms within eligible sectors. As Figure A.1 highlights, larger firms are more likely to take up the treatment when eligible, explaining why column (4) reveals a larger firm size compared to the other groups. When pooling eligible and non-eligible firms together, the pre-reform employment as of December 31st of each year averaged 55 workers. The average payroll tax rate was 31.78%, and low-skilled occupations accounted for 89% of employment.

Take-up. Figure A.2 shows that there is a substantial share of eligible firms that do not take-up the benefit. This phenomenon is generalized across all cohorts of eligibility, since the begining 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 lenses of a perfect tax optimization choice. There are a few facts that help to rationalize the imperfect take-up. First, the tax bills never mentioned any 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 for prosecutors to suit firms that do not opt in a supposedly beneficial tax system. Second, enrollment in the program was not automatic as in the Swedish case studied by Saez et al. 2019.

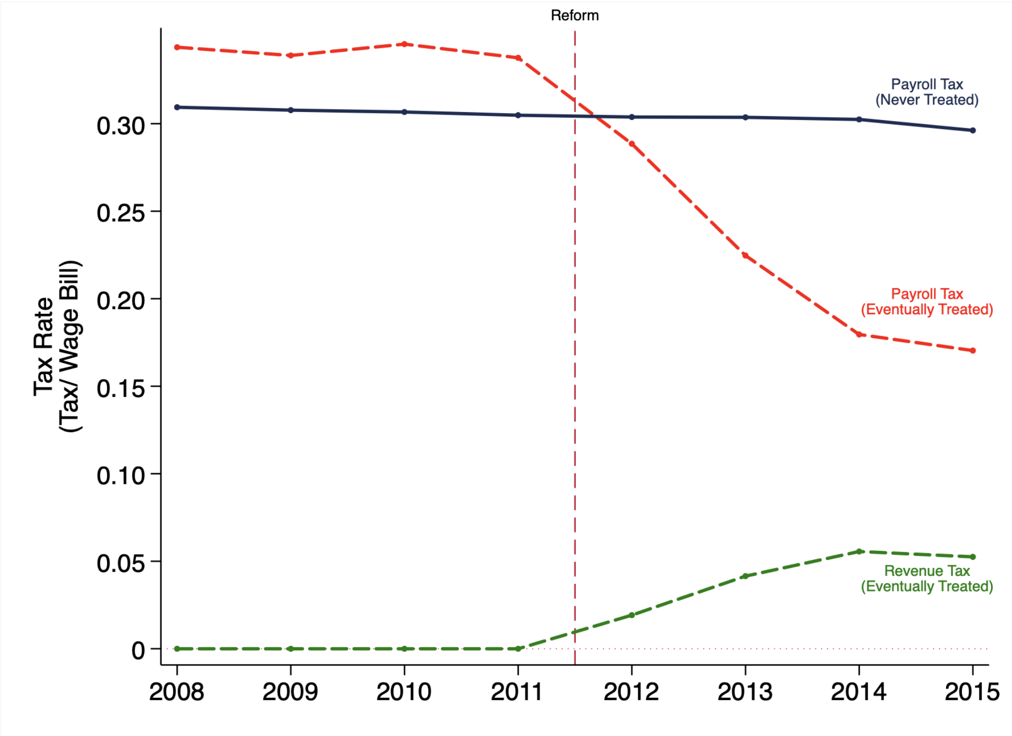
In Brazil, firms have to self-report eligibility on Government provided software to enable tax exemptions, through separate tax forms. Figure H.1 displays the set of information requested in the tax platform, to enroll in the payroll tax cut program. Even though enrollment implied a net tax cut, empirical findings in other countries (Kleven and Waseem 2013; Janet et al. 2006; Zwick 2021; Moffitt 2007) suggest that the operational filling process can lead to non-responsiveness even in dominated tax regions.[7] In line with this view, Figure A.1 shows that take-up is monotonically increasing with firm size. This 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 the payroll and revenue tax rates for firms that were treated at some point in time (eventually treated) vis-à-vis firms 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 that all tax rates are comparable. Reassuringly, tax rates from the raw data in Figure 1 align perfectly well with statutory rates. The figure reports unprecedented payroll tax reduction. For context, studies leveraging 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, reinforcing the interpretation of an overall tax cut, as opposed to a tax substitution.[8]

[7]It relates 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)

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

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.

3 Empirical Analysis

The payroll tax cut causes a sharp expansion in employment, with small but significant effects on long-term wages. In this section, I present details about 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. The IV is necessary to adjust for two margins of imperfect compliance: imperfect take-up in eligible sectors, and take-up in non-eligible sectors due to the product eligibility criteria (NCM). The first set of analysis relies on the firm-level sample, where 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, X_{jt} are set of controls on workforce composition (e.g., education, gender, race, age and its square), $\xi_{I(j),t}$ 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. 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,

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

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

I also estimate an IV difference-in-differences model, where 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 non-eligible firms would follow similar trends. There are two main threats to this design. 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 3.3 provide several tests that mitigate these concerns.

3.2 Spillover Analysis

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 advance in this understanding, 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 at the occupation x region cells.[9] To evaluate spillovers within the local labor market, I provide a set of evidence supporting that the Brazilian reform was a firm, rather than market-level shock.

First, I provide purely descriptive evidence that even at the local labor market level, the share of treated firms is small. Figure H.2 provides intuition that within a labor market there are eligible and non-eligible sectors. 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.2 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, then we should expect to see a negative employment effect in these firms compared to other "Simples" firms in non-eligible sectors. Figure A.3 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

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

similar trends in the pre-reform period. In contrast, it shows that among "non-Simples" firms, there are substantial effects of being in the eligible sectors.

Finally, if there were a spillover effect we would expect to see relatively more wage pass-through in intensively treated local labor markets.[10] The reasoning is that spillovers affect workers' outside options, therefore generating more wage hikes. Figure H.5 shows that the wage effect for workers in high versus low 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 seen as a firm-specific shock.

3.3 Validity of the Empirical Design

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

3.3.1 Selection on Eligibility

Trends. The concern about selection on eligibility is whether firms that were granted the eligibility status might have been on 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 display reassuring pre-reform results, not statistically different from zero. This suggests that, in the absence of tax reform, the outcomes for both eligible and non-eligible 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 and workers across various eligibility statuses also demonstrated a balance in baseline levels during the pre-reform period, as corroborated by Tables A.3 and A.4. One characteristic that did not exhibit a balanced distribution across groups was gender. Therefore, I included gender as a control variable in all specifications. Another method of illustrating the relationship between a firm's characteristics and its eligibility status is through a regression analysis conducted pre-reform:

[10] 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.

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

where $L_{s(j)}$ is a dummy to indicate if the firm is eventually eligible; X_{jt} are characteristics of the firm; and the fixed effects are the same used in the main empirical specifications. Table H.3 reports the results. In particular, it shows that under the TWFE model - identical to the one implemented in the main specification - there is a precisely estimated zero difference across eligibility groups in the pre-reform period.

Alternative Identification. The reason why specific sectors were chosen was not disclosure, nor was there an objective criterion to determine eligibility. From an econometric standpoint and potential identification concerns, it has been established that the 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 the propensity scores to break ties in a procedure, which matches firms based on pre-reform deciles on employment, wages, and hires. In 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), validated by the balance tables in Appendix G.2. Importantly, this strategy does not make any assumptions about the political process that determines eligibility.

The 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, thereby providing evidence that the results are not influenced by any inconsistencies in the matching algorithm.

3.3.2 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 toward eligible sectors. In this scenario, the concern is that firms expecting employment growth could self-select into treatment, thereby compromising the causal interpretation. Fortunately, our panel data allows us to track firms and assess whether they changed sectors upon the reform's implementation. The data shows only a small number of firms changing sectors, and among these, there is not a trend of switching toward eligible sectors. This low manipulation response aligns with the bureaucratic challenges of changing sectors.

Bureaucratic Process. Firms in the regular tax tiers (the object of this study) face a long and costly process to change sectors. To do so, they need first to change their operating agreement, which requires proof that they are operating in a new industry. Subsequently, they must request new operational licenses from multiple administrative bodies, including city hall, state, federal tax authorities, and others. Additionally, they must obtain clearance from local tax authorities and civil registry offices. Failure at any of these steps can result in sanctions and fines.

Additional Robustness. 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 the results remained qualitatively the same. Similarly, when I restricted the sample to firms that have never changed sectors, the results were unchanged. All these tests taken together, indicate that sector manipulation is not an active margin of response, which reinforces the causal interpretation of the results.

3.3.3 Informality

The informality analysis is not intended to provide further validation of the empirical design, but is aimed at exploring the economic interpretations of the results. 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 indicating that informality is not driving the results.

Transition. The panel structure of the data allows to track previous employment spells for workers who held formal jobs in the past. Essentially, the data enables 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 the hiring of 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.6 indicates, the proportion of new hires coming from non-employment and informality remains constant over time and across treatment status, suggesting that formalization is not a significant margin of response.

Regional Variation. Another approach to the informality question is to leverage the regional variation in informality rates. Brazil, a 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.7 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.6). 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, reinforcing that formalization is not driving the results.

Workers' Education and Capital Response. As Ulyssea 2018b notes, informal employment is concentrated among firms with a lower average education. The labor data provides information on workers' educational level, allowing us to the 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.6). 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 3.4, the reform prompts a shift from capital utilization, suggesting that employment responses are real.

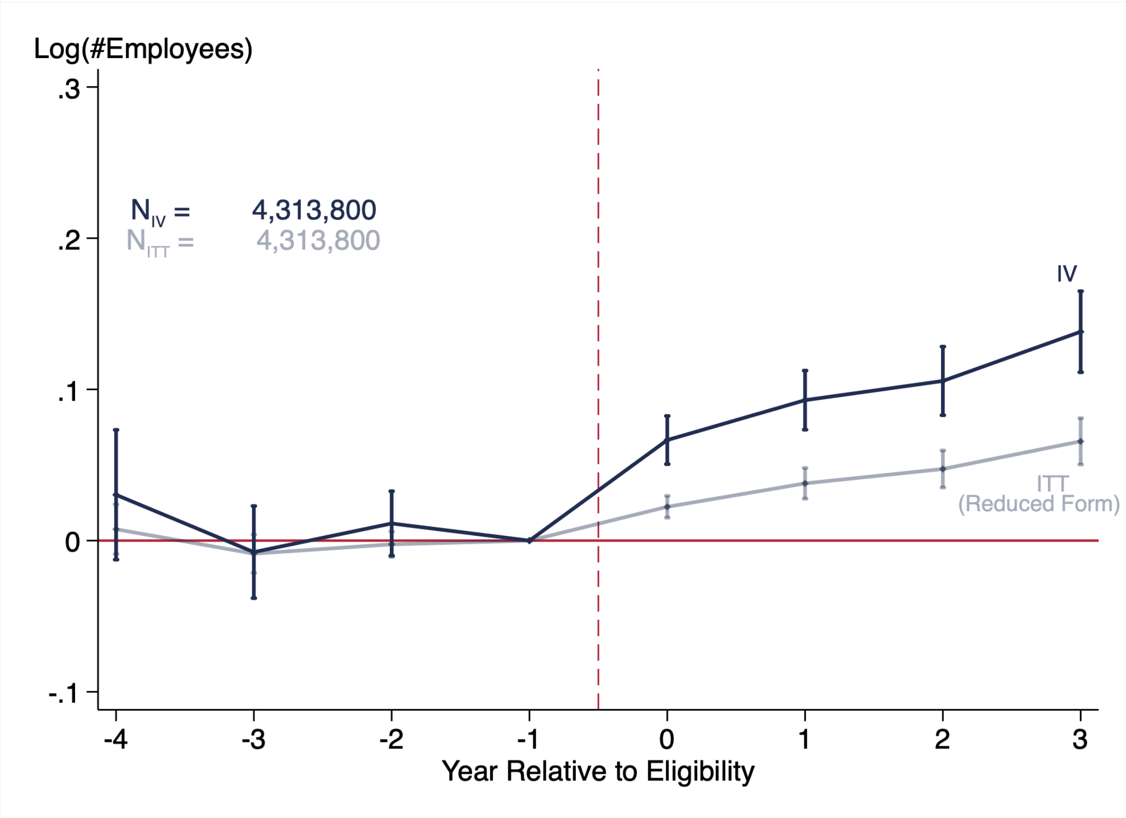
Discussion. The empirical evidence suggesting that informality is not a primary driver of employment responses aligns with several factors highlighted in previous research. First, informality in Brazil is primarily driven by self-employment rather than informal employment in a formal firm (PNAD, 2012). This implies that informal workers are more similar to entrepreneurs than employees, and their formalization decision involves other 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).

3.4 Main Results

In this subsection, I report the causal effects of the payroll tax reform on a comprehensive set of outcomes measured at the firm-level. The findings indicate that after a firm-specific payroll tax cut, employment and wages rise, providing evidence of labor market power. The reduced form estimates also reveal that the reform causes an increase in revenue, profits, and decrease in capital. These results, along with heterogeneous break downs, are key for the structural estimation, and to provide direct evidence on the incidence of payroll taxation.

Employment. The difference-in-difference estimation, from Equation (4), reveals that the payroll tax program causes a highly significant 9% increase (se = 0.03) in the number of employees. The event study analysis validates the parallel trends assumption and shows that there is an immediate employment response that is sustained and slightly increased over time, as shown in Figure 2. Based on the policy-induced labor cost variation (see Table 1, Column (1)), the corresponding empirical elasticity of employment with respect to the labor cost ($\epsilon^{L}_{1+\tau}$) is -0.71. These results remain qualitatively similar within the balanced sample of firms (Appendix G), which suggests that the employment effect is not governed by the dynamics of firm entry and exit. However, the average effect masks substantial heterogeneity, which I exploit next.

Figure 2: Event Study Estimates on Employment



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

Firm Size. The richness of the data allows me to evaluate heterogeneity responses based on firm size, which is measured in the pre-reform period. Table 1, columns (2)-(4), reveal statistically significant differences in employment responses between small and large firms. These findings are consistent with the heterogeneity per market concentration, measured by the market share at the local labor market.[11] Figure H.8 shows both employment and wage effects are greater for firms exhibiting less market concentration.

The firm size analysis is implemented by separately fitting the same specification for a sample of small, medium, and large firms. One advantage of this approach is that by comparing small firms in treatment and control groups, it

[11]Based on job switcher patterns, and following Felix 2021, local labor markets are defined at the commuting zone x 2-digits occupation level

eliminates the mean reversal channel that could potentially explain the heterogeneous pattern. Important to mention that this heterogeneity is not mechanically driven by differences in the first stage. Figure H.9 reassures that the reform impacts labor costs uniformly across the entire firm size distribution. Another potential explanation for the firm size heterogeneity could be financial constraints, which I discuss next.

Table 1: Firm Level Estimates

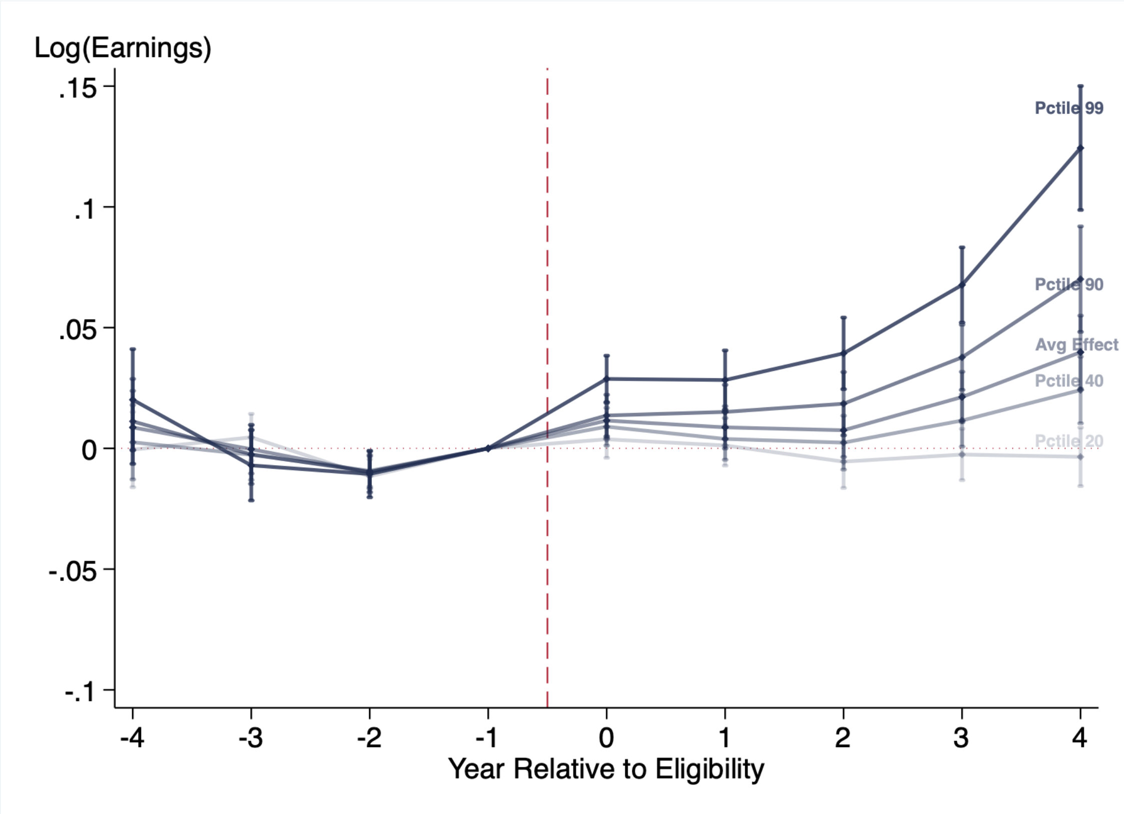
	Log(1+τ)	**Log(#Employees)**	**Log(#Employees)**		
	All Sample	All Sample	Small	Medium	Large
	(1)	(2)	(3)	(4)	(5)
Panel A: IV					
Diff-in-Diff	-.132***	.092***	.245***	.134***	.05*
	(.003)	(.029)	(.047)	(.031)	(.029)
Long Diff	-.118***	.13***	.303***	.191***	.102**
	(.005)	(.028)	(.046)	(.039)	(.043)
Panel B: ITT					
Diff-in-Diff	-.063***	.044***	.096***	.07***	.03*
	(.002)	(.014)	(.017)	(.017)	(.017)
Long Diff	-.066***	.063***	.133***	.101***	.054**
	(.003)	(.016)	(.02)	(.021)	(.026)
Controls	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓
Sector x Year FE	✓	✓	✓	✓	✓
# Clusters	10,489	10,679	8,548	7,040	6,082
N	4,095,696	4,234,882	2,613,652	685,405	457,937

Note: This table presents IV and reduced form (ITT) 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. Panel A reports the IV coefficients, which adjust for imperfect compliance. Panel B reports the reduced form coefficients, which are interpreted as the intention to treat (ITT) coefficients. Column (1) reports the policy-induced labor cost variation, which provides evidence on the first stage. The remaining columns have log of employment, as the dependent variable. Column (2) presents the average effect in the whole sample. Columns (3-5) present heterogeneity based on pre-reform firm size. Firms are categorized as small if they have less than 9 workers in the pre-period. Medium if they have 10-49, and large if they have more than 50 workers. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Earnings. On average, I find that the reform causes an earnings increase of 2%. In a competitive labor market, firm-specific shocks that do not alter the outside option of workers should generate employment, but no earnings response. The combination of both employment and earnings positive effects serves as evidence of labor market power. In addition to this key insight, several other findings emerge when looking at the effect on workers' earnings.

First, there is a positive average earnings increase that gradually builds over time. More strikingly, however, is the inequality in the wage pass-through across the within-firm distribution. Figure 3 illustrates this by showing that the earnings effect across different percentiles of the within-firm distribution displays a monotonic pattern. The effect is null at the bottom and rises to more than 4% at the firm's 99^{th} percentile, which is consistent with Carbonnier et al. 2022; and Kline et al. 2019. The absence of pre-trends across the entire range of the earnings distribution further corroborates these findings. These results shed light on an important consequence of the tax policy, the within-firm wage inequality. As the government reduces payroll tax rates, wages for those that already had higher earnings increase relatively more. To accommodate the more pronounced earnings pass-through to high-skill workers, Appendix E extends the model to allow for skill-specific labor supply.

Figure 3: Earnings Effect Within Firm Wage Distribution



Note: This figure presents the 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 the results with respect to one year prior to the event. The analysis spans three years prior to entering the payroll tax cut program and three years after. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Other Margins of Adjustment So far I have empirically established that upon a sizable tax cut, firms present substantial employment growth and moderate increases in workers' earnings. However, these two response dimensions alone do not provide a comprehensive understanding of the tax incidence. For instance, it is not immediately clear whether the increase in employment arises from scale or substitution. These alternative channels influence prices and output differently, leading to disparate implications for firm's revenue and consumer surplus. Moreover, to infer the incidence on firm owners entails examining not only employment and earnings pass-through, but also the effect on profits.

To measure all these responses, we rely on firm-level anonymized balance sheet information from tax records. Unfortunately, not all formal firms have to report balance sheet information. Only firms in the "Real Profit" tax tier are obligated to file that information.[12] Table 2 displays the results. Notably, within this tax tier, the treatment effects on labor market outcomes closely resemble those estimated for the entire universe of formal firms (refer to Columns (2) and (4)). Figure *A*.5 further validates the parallel-trends assumption, demonstrating that the pre-reform coefficients are not statistically different from zero for all outcomes.

The payroll tax reform presents an unambiguous incentive for firms to expand employment from both the scale and substitution perspectives. Nevertheless, the usage of capital is subject to two counteracting forces. On one hand, reduced labor costs stimulate production, thus positively affecting capital demand. On the other hand, lower labor costs generate incentives to substitute labor for capital. Column (3) quantifies the net effect on capital, as well as the relative importance of these two channels. It shows that as payroll taxes decrease, firms expand employment and shift away from capital.

The identification of scale effects, based on the choice of inputs, serves as a crucial step toward determining price responses. This process is formally executed in Section 5, where I estimate the demand elasticity. Combining scale and revenue pins down prices, which is instrumental in assessing the incidence passed onto consumers. The perfect fit between the model and data, plus the overidentification tests, enhance the reliability of the structural estimations. Another advantage of observing many margins of responses is that it allows us to evaluate the coherence among multiple channels and models' predictions, which I turn to in the next section.

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 administrative data used in this study enables direct observation of the tax cut captured by firm owners in the form of accounting profits. Given that profits can plausibly be negative or zero, I opted not to rely on logs for this specific outcome. Instead, I leverage the analysis in levels, where I estimate an average profit effect of $260,000, which represents a 15% profit increase relative to baseline (Figure *A*.5).[13] Reassuringly, results are robust to using the inverse hyperbolic sine transformation.

[12]"Real Profit" is a tax tier for larger firms with annual gross revenue above BRL $78 Million, approximately USD 15 million at the current exchange rate.

[13]The analysis in levels become more sensitive to outliers, for this reason, I winsorize the sample at the 99% level, for this analysis.

Table 2: Firms' Margins of Adjustment

	(1) Log Labor Cost $(1+\tau)$	(2) Log Employment	(3) Log Earnings	(4) Log Capital	(5) Log Revenue
Panel A: Diff-in-Diff					
Baseline	-.1321***	.1019***	.0251***	-.0967***	-.073***
	(.0032)	(.0206)	(.0063)	(.0335)	(.0232)
Small Firms	-.1352***	.2691***	.0563***	-.0858	-.0216
	(.0067)	(.0366)	(.016)	(.0784)	(.0516)
Large Firms	-.1308***	.0573**	.0152**	-.0953***	-.0831***
	(.0038)	(.0239)	(.0059)	(.0361)	(.0251)
Panel B: Long-Diff (t + 3)					
Baseline	-.1247***	.1279***	.0309***	-.0337	.0495
	(.0059)	(.0311)	(.0094)	(.0496)	(.0371)
Small Firms	-.1275***	.3461***	.0602**	.0085	.1864**
	(.0224)	(.0587)	(.0252)	(.1133)	(.0875)
Large Firms	-.1253***	.0969***	.0228***	-.0312	.0223
	(.0047)	(.0352)	(.0086)	(.0549)	(.0391)
Controls	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓
Sector x Year FE	✓	✓	✓	✓	✓
N	449,679	450,387	450,387	345,217	374,774

Note: This table reports difference-in-differences and event study coefficients instrumented by sector eligibility. Each column reports different margins of adjustment, such as labor cost, employment, revenue, earnings, and gross revenue. 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. Panel A reports the difference-in-differences coefficients. Panel B reports the long-diff coefficients, which are the event study coefficients for t+3. Standard errors are reported in parentheses.

3.5 Additional Results

To interpret the main results, additional analysis is necessary to clarify driving forces underlying the reduced form estimates. For example, the earnings effect could be driven by a change in the workforce composition rather than earnings pass-through. Similarly, the firm size heterogeneity could be driven by liquidity constraints. In this subsection, I provide additional evidence to address these alternative interpretations.

Liquidity Constraints. The observed heterogeneity in firm size is consistent with previous studies (Bronzini and Iachini 2014; Howell 2017; Zwick and Mahon 2017; Criscuolo et al. 2019; Saez et al. 2019) which found that tax subsidies yield greater employment responses by small firms in different contexts. Due to a lack of data and appropriate variation, the literature has not been able to rationalize the mechanism underlying this fact. Criscuolo et al. 2019 suggested that liquidity constraints might be a contributing factor. According to their argument, small firms, being financially constrained, may not be able to expand employment even when the marginal benefit exceeds the marginal cost. In this view, the payroll tax cut serves to alleviate the financial constraints of the firm, subsequently leading to an increase in employment.

To test this hypothesis, I utilize anonymized firm-level balance sheet data. I define liquidity constraint as the ratio of short-term assets to short-term liabilities. An example of current assets is cash, whereas an example of current liabilities are short-term bills, such as the wage bill. I divide the sample into firms that fall below and above the median based on the pre-reform measure of liquidity constraint. The employment effects for both groups turn out to be strikingly similar. The results are reported in Table A.7. In Section 5, I estimate that small firms have less market power and then discuss how market power provides a suitable explanation for the greater employment response observed in smaller firms.

Composition. The 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. However, Table 3 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, from a baseline share of 60 percent (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 from Kline et al. 2019. Column (6) investigates whether the reform influences firms to hire workers from different parts of the earnings distribution, finding no effect on the quality of new hires proxied by their pre-hiring earnings. Column (7) displays the impact on a quality index, computed through 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. Table 3 suggests no evidence of skill upgrading in response to the reform.

Finally, I explore whether the tax cut affects the types of occupations firms employ. I exploit the detailed CBO occupational codes[14], which contains 2,300 oc-

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

cupations. After ranking occupations based on pre-reform earnings and grouping them into percentiles, I determine each firm's average occupation percentile. Table A.8 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 that there is no major shift in worker composition, nor technology-induced labor demand.

Table 3: 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
Controls	×	×	×	×	×	✓	✓
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

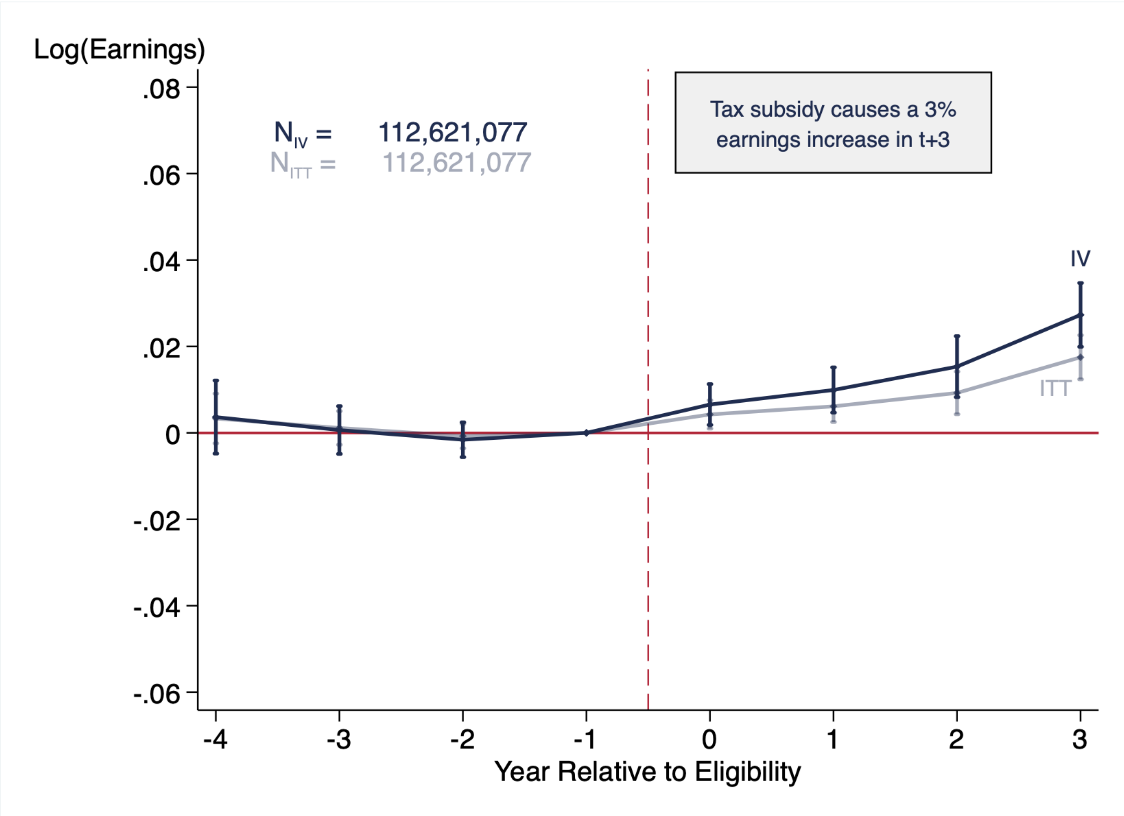
Note: This table reports difference-in-differences coefficients to assess the effect of the reform on the firm's labor composition. The empirical specification is the same as presented in equations 3 and 4. The regression is estimated in the balanced sample of firms to isolate any noise due to firm entry and exit. The goal is to depict the firm-level compositional effect. In columns (1)-(5) controls are not included to avoid over-controlling given that in these regressions the outcome is part of the set of variables typically used as control. 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 with firm fixed effects as additional controls. Standard errors are reported in parentheses.

3.5.1 Worker-level Analysis

An alternative approach to evaluating earnings pass-through involves tracking workers, as opposed to firms. This strategy offers two main advantages. First, it enables the assessment of whether firm-level earnings response results from 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 of the labor force. Second, it yields insights into how tax variation impacts workers' career paths, particularly among various types of workers. To conduct the worker-level analysis, I fit the empirical specification outlined in Section 3.1 to the worker sample, which allows for the inclusion of worker fixed effects.

Workers' Earnings. Figure A.6 depicts a pronounced drop in the gross earnings paid by firms, mostly attributed to the mechanical reduction in payroll tax rates. 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.

Figure 4: Event Study Estimates on Workers' Earnings



Note: This figure presents the event study estimates for average earnings (net of payroll taxes) for stable workers. I normalize the 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. The blue markers report the IV estimates, while the gray markers are the intention-to-treat. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Occupation. Consistent with 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, whereas the remaining 85% of lower positions are evaluated separately. Figure A.7 shows that the pass-through to highly skilled workers is 6%, and it is almost zero to low-skilled workers. Appendix E provides an extension of the model, including two types of labor, able to rationalize the findings in a setting where low-skilled workers have higher labor supply elasticity. This is consistent with a more concentrated local labor market for high-skill labor, and suggests that low-skill labor market operates closer to perfect competition.

Racial Wage Gap. The payroll tax program does not distinguish workers based on background characteristics, such as race. It offers a flat 20 percentage point cut that remains constant across all income levels, suggesting 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 utilize the policy-induced tax variation to find that white workers benefit significantly more from the reform in comparison to non-white workers. This intriguing result holds even after controlling for firm fixed effects, suggesting that the racially unequal pass-through is not attributed to firm sorting. I also evaluated heterogeneous pass-through according to gender but found zero statistical difference. Figure A.8 summarizes the analysis across different types of workers.

Unintended Discrimination. This paper introduces another critical aspect to the public debate. Despite racial discrimination being a pressing social issue in modern society, it has not been incorporated into the tax literature.[15] This might be because modern tax codes do not contain any explicit elements of racial discrimination that could be classified as either statistical or taste-based discrimination. However, taxes can exacerbate racial inequality through indirect channels substantiated in existing frictions. This paper provides novel evidence that behavioral responses to tax changes can lead to unintended consequences on racial inequality.

4 Model

The empirical evidence provided so far (particularly, Table 2) 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 that the payroll tax literature has not studied yet.

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

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 Marshall-Hicks framework to incorporate imperfect competition in both product and labor markets. By combining these two competitive friction, often modeled separately, this model can explain how heterogeneity in market power rationalizes all the empirical findings presented so far. The actual degree of market power in the economy is an empirical question uncovered by firms' response to the tax shock. The model delivers key identifying equations that directly connect the reduced form estimates to structural parameters of easy interpretation. Using the model, I can quantify mechanisms of response, and isolate the role of market power in dictating 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, where firms operate as monopolists in the product market and monopsonists in the labor market. The model has a single period, where firms choose their input mix and output level. After selling the 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, 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 the 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 justifies constant elasticity, and 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 on 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 to 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 on 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:

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

I denote this new term as the average incumbent's rent because it relates 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 non-linearity in the cost function will be key to understanding the pass-through responses to payroll tax reforms.

4.3 Pass-Through

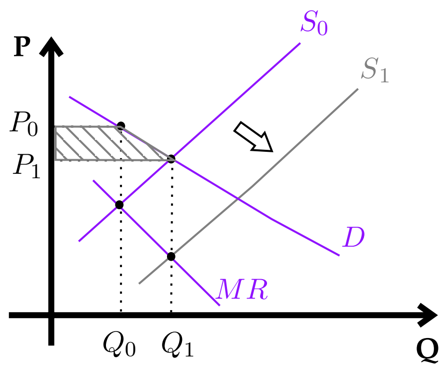
Thus far, we have presented the framework for firms' decisions in both output and input markets. This section develops intuition on the interaction between payroll tax variation and firms' choices. In particular, it sheds light on the role of market power in shaping the pass-through, which ultimately drives the incidence and efficiency of the payroll tax system.

4.3.1 Output Market

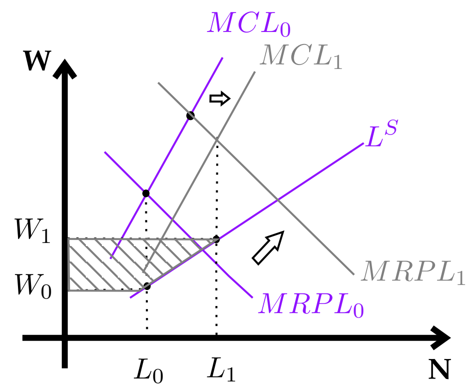
In the output market, a payroll tax reduction shifts the supply of goods. The consequences for output depend on two factors: (i) the behavioral response, which determines the magnitude of the shift, and (ii) the slope of the demand curve. Figure 5 illustrates how the price effect increases with market power. To quantify this effect, I totally differentiate the pass-through equations to compute the 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 the 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 to 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 on the 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 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 equate 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 in two mechanisms through which imperfect competition affects tax incidence. First, competition in the output market flattens the demand curve ($\frac{-1}{\eta}$), enhancing the scale effect. Second, competition increases the pass-through to the marginal cost, amplifying the shift in the supply curve and thereby the scale effect. It is important to highlight 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)-(18).

4.3.2 Labor Market

Labor market forces at work 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. Like in a perfectly competitive labor market, the first component directly 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. The 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. The pass-through to the marginal revenue depends on the direct and inframarginal effects of the firm-specific labor cost variation. Note that the 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, offsetting part of the initial cost reduction. Equation (9) relies on evelope arguments to quantify these responses.

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

The interaction between labor market power, and pass-through to the marginal cost is unambiguous. The higher the market power, the higher the direct pass-through to the incumbent's rent, and it also amplifies the indirect effect on the incumbent's rent due to labor responses. In Appendix F, I allow perturbations in the capital tax to study the role of monopsony power on the incidence of capital taxation.

5 Structural Estimation

This section connects the model and data. Studying this relationship allows me to credibly estimate parameters of interest, understand mechanisms, and design counterfactual policies.

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 depends on parameters. Solving this system, I find a clear connection between the structural parameters and the 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 the 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 subjected to this tax.[16] Once we account for product and labor market power, the effects of the Brazilian tax reform on employment, capital, earnings, and revenue can be expressed as a function of observables and three parameters to be estimated (ϵ, η, ρ):

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

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

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

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

where s_L is the labor share, $\epsilon^L_{1+\tau}$ is the empirically estimated elasticity of employment with respect to the labor cost, ϕ_1 measures the first stage associated with the policy, i.e., the percentage variation on tax rates induced by the reform. Using anonymized tax data, I precisely estimate ϕ_1. The pass-through formulae developed here is more general to the ones employed in recent studies that assume perfect labor competition. My framework can accommodates perfect labor competition as a particular case, where ϵ goes to infinity. Taking the limit of the 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,

[16] 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.

substitution and scale effects are separable, as illustrated below.

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

Identification. The pass-through expressions can be solved for the structural parameters as a function of the reduced form estimates. Equation (18) 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, it is an indication that firms are substituting capital for labor. Also 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 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 η can be identified based of the ratio between the scale and price responses to the tax reform. This ratio determines the slope of the demand curve in the product market.

Estimation Methods. The point estimates for each parameter are directly estimated by the combination of reduced form effects outlined in equations 14-16. I fit a seemingly unrelated regression to jointly estimate equations 10-18 and obtain a covariance matrix. I use the Delta method to estimate standard errors for each structural parameter.

Even though this method provides clear and intuitive evidence of the identifying variations for the structural procedure, it is not the most efficient one.

For this reason, I also estimate parameters using the Classical Minimum Distance (CMD) approach. The point estimates align under the two procedures, but CMD offers lower standard errors, as reported in Table H.4. The CMD approach 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.

Parameter Estimates. Table 4 presents the estimates for the three parameters of interest. Column (1) presents the baseline results for all firms, and columns (2) and (3) break down the estimates based on firm size. There are several reasons to break down the estimates per firm size. First, they are highly correlated with measures of market concentration that we can observe, such as market share in the local labor market. Second, it is policy informative, given that firm size is a characteristic easy to target policy on. Third, there is a large literature finding that small firms react more to industrial policies, which contributes to a 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 the baseline. This result is similar to Karabarbounis and Neiman 2014 and implies that capital and labor are substitutable, supporting the view that lowering the cost of capital may increase income inequality (Piketty and Zucman 2014).[17] Interestingly, I find that capital and labor are more substitutes in small firms (5.01, se 0.34), as opposed to in large firms (1.25, se 0.08). This result is valuable because most of the literature dedicated to capital-labor elasticities is focused on large firms in manufacturing, as opposed to my study, which encompasses a wide range of firm sizes and sectors. Greater substitutability depicted among smaller firms can reconcile findings of capital-labor complementarity in manufacturing.

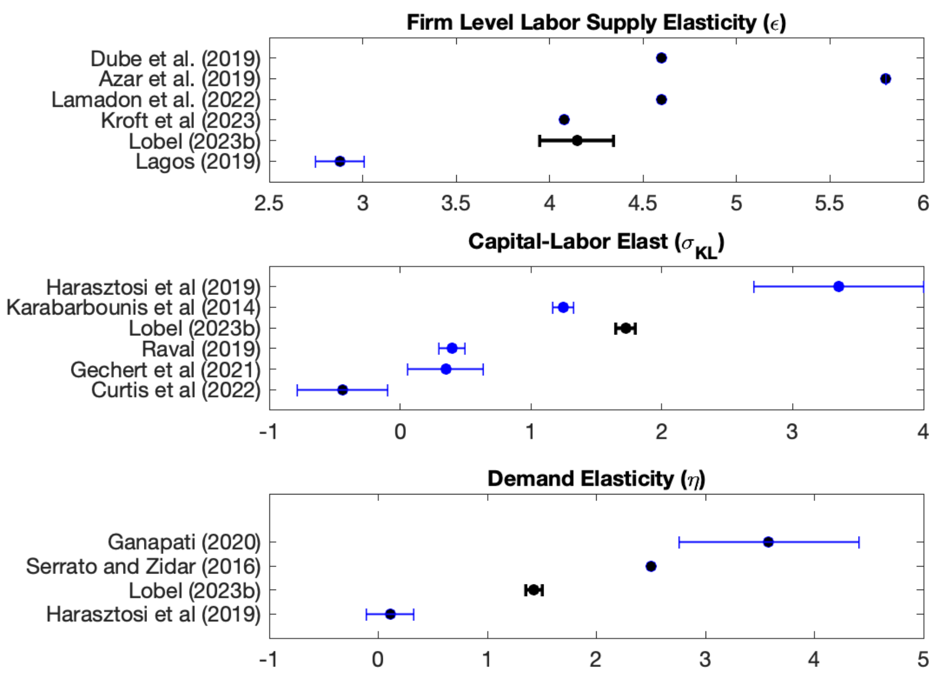
Labor Supply. The labor supply elasticity faced by the firm ϵ is 4.15 (se 0.20), which is remarkably close to recent estimates: 4.08 (Kroft et al. 2020), 4.0 (Card et al. 2018), 4.6 (Lamadon et al. 2022). I also do not reject the 2.88 estimate that Lagos 2019 found for Brazilian firms. Figure 6 summarizes the literature, and points out several studies reporting labor supply elasticity between 3 and 5. My baseline estimate implies a wage markdown of 0.81 ($\mu = \frac{\epsilon}{1+\epsilon}$), suggesting that

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

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. The 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 are 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.

Figure 6: Literature Benchmark



Note: This figure places my estimates with respect to existing estimates in the literature. The 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.

Over-identification Test. To assess the validity of the model, I compare the reduced form estimates with their corresponding model predictions, based on equations 10-18 and the estimated structural parameters. Note that there are 4 moments and only three unknown parameters (ϵ, σ, η), enabling me to test for over-identifying restrictions. The p-value for the J-test, reported in the last row of Table 4, indicates that the restriction is not rejected. This finding provides evidence that the model fits the data well and is appropriately specified. To further validate the model, I conducted the J-test for both small and large firms. In all cases, the over-identifying restrictions are not rejected. The p-values are comfortably above the standard confidence level of 5%, as evident from columns (2)-(3).

Mechanisms. Large firms, due to their market power, have a relatively greater influence on prices. This power paradoxically limits their ability to scale up their plant size in response to tax relief. As Table 4 demonstrates, the scale effect for large firms is modest at 6%, accompanied by a similar 6% reduction in prices. Column (1) indicates that after a 15p.p drop in the labor cost, employment increases by 12%[18], with the scale margin accounting for 65% of this effect. The baseline result hides interesting heterogeneity. Column (2) points out that for small firms, 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 is 6% due to scale and only 3% substitution. The interplay between scale and substitution has unique implications for analyzing tax incidence and efficiency. A more prominent scaling effect leads to larger price reductions, which ultimately benefit consumers.

[18]The empirical moments refer to the long-diff regressions, i.e., responses measured in t+3. This is the preferred timing because some responses such as workers' earnings take time to manifest. Therefore, it is logical to evaluate firm responses after an initial transition period. However, using the difference-in-differences coefficients do not cause abrupt changes to the structural estimates.

Table 4: Structural Estimation

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)
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
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
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. 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, as well as the cost shares. At the bottom, the table displays the p-values associated with the J-test for overidentification.

5.2 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 allows me to compute the efficiency gains from tax reductions or, from a different perspective, the deadweight loss associated with payroll taxation. The goal is to connect empirically observed policy responses to the incidence framework, to deliver three key insights. First, a novel payroll tax examination that accounts for the role of consumers in the tax pass-through. Second, the assessment of distortionary costs arising from payroll taxation. Third, the development of a credible framework able to measure the role of market power in shaping the efficiency and distributional consequences of payroll taxation. As market power limits firms' responses to the tax variation, it mitigates the tax-induced distortions.

Incidence Framework. The tax base is determined by the total wage bill:

$$B = wL = AL^{1+\frac{1}{\epsilon}}$$

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 the 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, resulting in a 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. 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 assumptions regarding optimizing behavior to infer them. 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, appropriately weighing the welfare attributed to each stakeholder.

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 relates 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, as workers have equally attractive opportunities elsewhere.

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 consumer 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, production inputs, and a well-specified demand curve (refer to the overid tests), allows us to back out the price response.

Efficiency Gains. The incidence formulae serve as a foundation for two interrelated measures of efficiency. The first measure involves a direct comparison between the welfare gains and the revenue costs triggered by tax reform. In mathematical terms, the efficiency gains derived from a tax cut can be written as

$$dW = dC + d\pi + dB + dR$$

The second measure is 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 cost (Mayshar 1990; Slemrod and Yitzhaki 2001; Kleven and Kreiner 2006; Hendren 2016; Bailey et al. 2020; Hendren and Sprung-Keyser 2020).

Estimates. Upon establishing the theoretical incidence and efficiency formulae as functions of the reduced-form coefficients, I proceed with the structural estimation, as shown in Table 5. Panel B reveals that consumers bear 75% of payroll taxes, while firm owners and workers bear 11% and 14% respectively. The aggregate welfare gains experienced by these stakeholders surpass the decrease in Government revenue, resulting in an efficiency gain of 44% as reflected in the MVPF value of 1.44.

Discussion. The analysis conducted herein reveals insightful findings 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 a more distortionary tax. 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 reviewed in Hendren and Sprung-Keyser 2020.

Table 5: 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	0.14
Firm Owner, $d\pi$	$\frac{\beta_\pi s_\pi}{s_L}$	0.11
Consumer, dp	$\frac{\beta_R}{s_L(\eta - 1)}$	0.75
Government, dT	$\Delta\tau + \tau_0\beta_L\frac{\epsilon + 1}{\epsilon}$	-0.70
Welfare, dW	$\frac{dw + d\pi + dp + dT}{dT}$	0.44
MVPF	$\frac{dw + d\pi + dp}{dT}$	1.44

Note: This table bridges reduced form and structural estimation. Panel A identifies and estimates structural parameters. Panel B identifies and estimates payroll tax incidence to workers, firm owners, and consumers. Panel B also reports the connection between welfare and MVPF measures to the reduced form estimates.

5.3 Market Power and the Distortionary Costs of Taxation

The efficiency gain induced by a discrete payroll tax cut can be computed following the steps outlined in section 5.2:

$$\Delta W = B\left[\underbrace{\beta_w}_{\text{worker, dw}} + \underbrace{\frac{\beta_\pi s_\pi}{s_L}}_{\text{firm owner, } d\pi} + \underbrace{\frac{\beta_R}{s_L(\eta-1)}}_{\text{consumer, dp}} + \underbrace{\overbrace{(\tau-\tau_0)}^{<0 \text{ (tax cut)}} + \tau_0 \frac{\beta_L(\epsilon+1)}{\epsilon}}_{\text{Governement, dT}} \right] \tag{17}$$

Taking Equation 17 to the data, we obtain a precise measure of the dead-weight loss associated with the payroll taxes. Appendix D provides theoretical discipline to study the role of market power in shaping the efficiency costs taxation. Equation 18 decomposes the deadweight loss into three terms, showing their impact on the distortionary costs of taxation, and their interaction with behavioral responses ($\frac{\partial L}{\partial \tau}$ and $\frac{\partial Q}{\partial \tau}$).

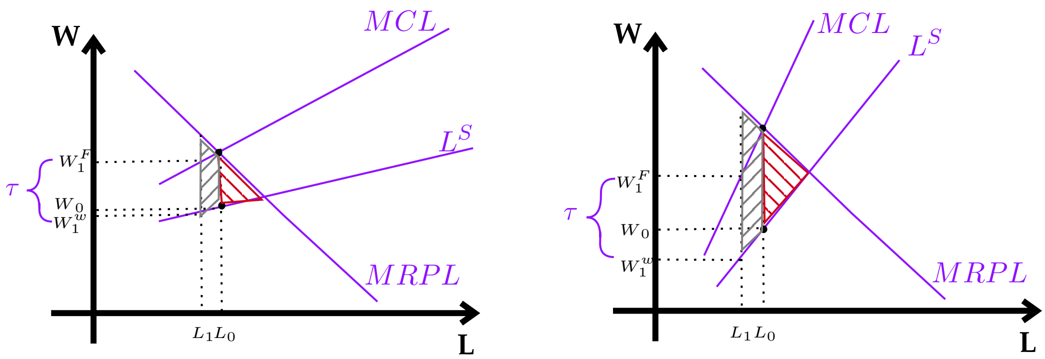
$$dW = \frac{\partial L}{\partial \tau}\left[\frac{w}{\epsilon} + w\left(\frac{\epsilon+1}{\epsilon}\right)\tau\right] + \frac{\partial Q}{\partial \tau}\left[\frac{Q^{\frac{-1}{\eta}}}{\eta}\right] \tag{18}$$

The third term in Equation 18 captures the product wedge due to product market power. The first two terms inside the brackets focus on labor market distortions, where the first term addresses the distortion induced by monopsony and the second addresses the tax wedge on labor cost. These terms are captured by the slope of labor supply and product demand curves, and illustrate that increased market power leads to higher deadweight loss. The intuition is that monopsony distorts the original labor choice, and the tax wedge exacerbates this distortion. This result ties back to the standard Public Finance view that the deadweight losses rise with the square of the tax.

Importantly, market power also affects tax distortion through behavioral responses, as represented by the derivatives outside the brackets in equation 18. Intuitively, the behavioral responses are captured by the shifts in labor demand and product supply. This idea finds support in the employment responses estimated from the Brazilian policy variation (Equation 10), and the mechanisms discussed in Section 5.1. The theoretical and empirical evidence so far shows that when firms have (product and labor) market power, they don't reduce inputs and output as much in response to an increase in taxes. Therefore, through this channel, market power mitigates the tax-induced distortions.

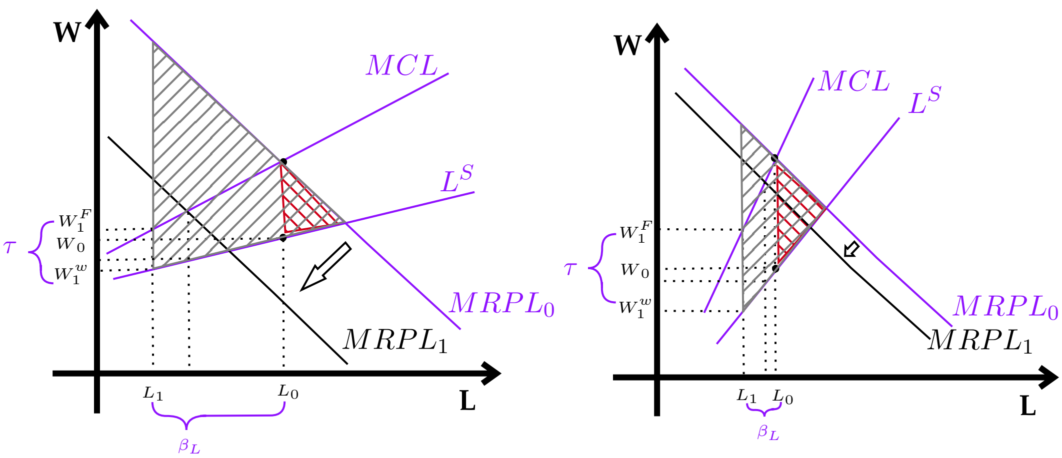
These theoretical insights can be summarized in a simple graphical framework (Figure 7) that illustrate the two opposing effects of market power on Harberger triangles. Panels (a) and (b) depict that the slope of the labor supply curve captures the standard channel. Steeper curves mean that firms have suboptimal size, and taxing them lead to greater efficiency loss. Panels (c) and (d) illustrate that once the behavioral responses are accounted for, tax distortions can be greater for firms with less market power. The shift in the marginal revenue product of labor captures the behavioral channel. The magnitude of the distortion increases with the magnitude of the shift. Equation (8) quantifies the extent through which market power shapes the tax pass-through to the marginal product of labor.

Figure 7: Market Power and Tax Efficiency



(a) Less Market Power

(b) More Market Power



(c) Less Market Power (with behavioral)

(d) More market power (with behavioral)

Note: This figure provides intuition on how market power influences the distortionary impacts of taxation. It highlights that the deadweight loss is determined not just by the steepness of the curves, but also by the behavioral shifts in the marginal revenue product of labor.

5.4 Policy Implications

The results discussed thus far provide insights for policymakers on the distributional and efficiency impacts of taxing firms with more market power. This section aims to quantify these effects by combining model and data to evaluate alternative policies. Additionally, it offers guidance on the revenue maximizing payroll tax rate.

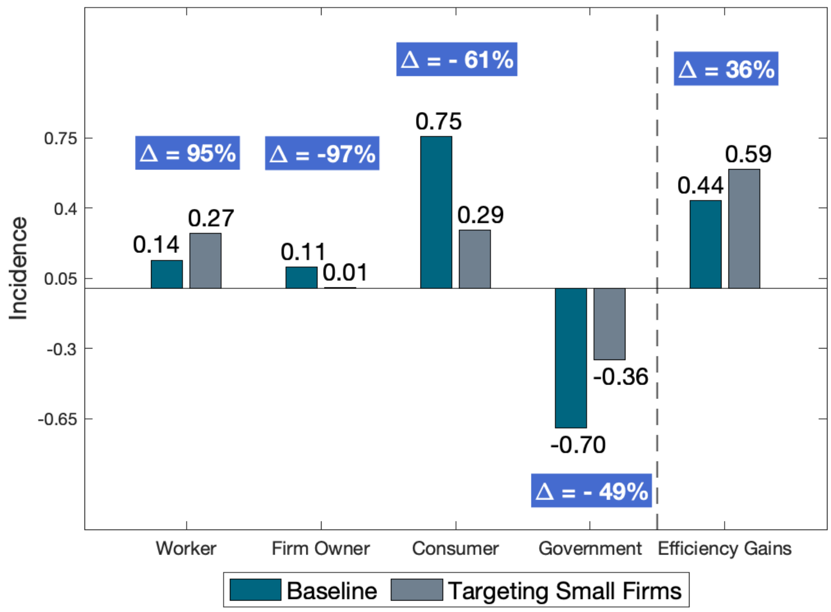
5.4.1 Targeting Small Firms

Given that small firms possess less market power, as evidenced by Section 5.1, a natural question is what would be the consequences of targetting the payroll tax cuts only to small firms. To make the alternative policy comparable to the baseline case observed in the data, consider a policy that originally covers the same amount of workers, but only those employed in small firms. Important to note that the precise measure of deadweight loss in Equation 17 accounts for potential differences in firms' initial allocation, reflected in the total wage bill "B".

To operationalize this exercise, I compute the incidence and efficiency using small firms' empirical moments, structural parameters, and initial allocations. The alternative policy generates winners and losers. Workers experience a 95% gain in the counterfactual scenario, suggesting that moving further along the supply curve more than compensates for the less steep curve, thereby triggering a more pronounced wage response. Conversely, in the product market, shifting further along the demand curve does not compensate for the less steep demand curve. Therefore, the price decrease is relatively less pronounced for small firms. As a result, consumers find themselves worse off by 61% under the alternative targeting. Lastly, firm owners retain almost negligible tax benefits, primarily because small entrepreneurs lack the power to capture rents.

Figure 8 reports that efficiency gains increase from 44% to 59%, representing a 36% variation. Part of this efficiency boost is justified by the behavioral response on Government revenue, which offsets 49% of the subsidy cost. In simpler terms, smaller firms tend to expand employment relatively more, which makes it cheaper to subsidize them. This alternative policy stands out as more efficient, even in a revenue-preserving scenario, which is evidenced by the 11% rise in MVPF. This result suggests that, in contrast to the perspective of a policymaker assuming perfect competition, payroll taxes offers a more efficient means to raise revenues.

Figure 8: Baseline vs Targeting Small Firms



Note: This figure presents the baseline incidence analysis (blue bars), and counterfactual incidence in case of alternative targeting (gray bars). The alternative policy, relies on a policy that targets only small firms, which are estimated to have less (product and labor) market power. The figure also plots the percentage difference in the incidence and efficiency measures across the the baseline and alternative policies.

5.4.2 Revenue Maximizing Tax Rate

To compute the behavioral effect we rely on the elasticity of labor supply with respect to the labor cost that was empirically estimated from the quasi-experimental variation. Note that this elasticity is fixed around the observed tax rate. To extrapolate the counterfactual elasticity at hypothetical tax rates far from the observed level, I undertake a Taylor expansion, with rates varying from τ_0 to τ_1:

$$\begin{aligned}\frac{\partial L}{\partial 1+\tau}(1+\tau_1) = \frac{\partial L}{\partial 1+\tau}(1+\tau)\Big|_{\tau=\tau_0} + \frac{\partial L}{\partial^2 1+\tau}(1+\tau)\Big|_{\tau=\tau_0}(\tau_1-\tau_0) \\ + \frac{1}{2}\frac{\partial L}{\partial^3 1+\tau}(1+\tau)\Big|_{\tau=\tau_0}(\tau_1-\tau_0)^2 + \dots \quad (19)\end{aligned}$$

$$\frac{\partial L}{\partial 1+\tau}(1+\tau_1) = \frac{L\epsilon_{L,1+\tau}}{1+\tau}\Big|_{\tau=\tau_0}\Bigg[1+\frac{(\epsilon_{L,1+\tau}-1)}{1+\tau}\Big|_{\tau=\tau_0}(\tau_1-\tau_0) + \frac{1}{2(1+\tau)^2}\Big|_{\tau=\tau_0}(\epsilon_{L,1+\tau}(\epsilon_{L,1+\tau}-1)+2)(\tau_1-\tau_0)^2\Bigg] \quad (20)$$

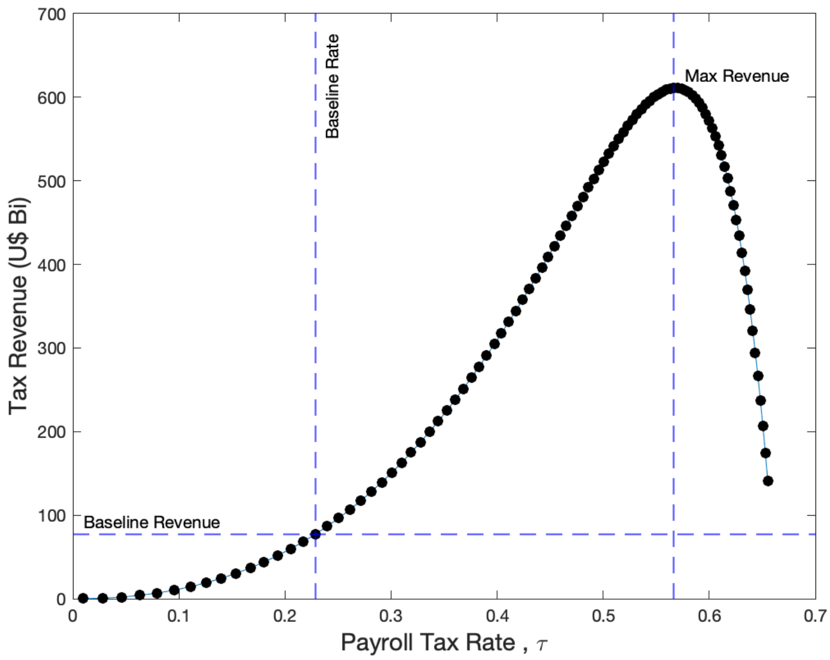
In counterfactual scenarios, where the payroll tax rate moves to τ_1, I compute the behavioral response by evaluating $dH = \tau B(\frac{\epsilon+1}{\epsilon})\frac{\partial L}{\partial 1+\tau}\frac{1}{L}$ at the counterfactual elasticity delineated in 20. With this framework, I simulate the revenue impact of perturbing the labor tax rate. Figure 9 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%.[19]

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 consequence 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 5.

[19] 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 are:

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

Figure 9: 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.

6 Conclusion

In this paper, I study a payroll tax reform that promoted unprecedentedly large tax reductions to a small share of firms in Brazil. The setting allowed fewer assumptions on general equilibrium effects, to estimate firm and worker-level responses. While capital decreases, a payroll tax reduction causes an increase in employment, wages, and profits. These effects exhibit heterogeneity across firms, with small firms responding relatively more. Interpreted through the lenses of a model of factor demand that incorporates product and labor market power, the empirical estimates inform that consumers pay most of the payroll tax burden.

The efficiency cost of taxation hinges on two opposing forces: the slope of product demand/ labor supply curves, and the associated behavioral responses. While market power induces steeper curves, it also restricts the behavioral responses. Therefore, the role of market power in determining the distortionary costs of taxation is not obvious. I precisely measure that raising taxes to large firms, estimated to possess more market power, not only reduces the efficiency cost of taxation but also shifts the incidence away from workers and toward firm owners and consumers. This result lends support to proposals advocating for relatively higher tax rates for larger firms.

Industrial policies that target specific sectors and firms are pervasive in the developing world, and have attracted renewed attention from scholars. To understand the forces at play in such policies, I aim to convey a simple yet essential message: the role of market power must be taken into account.

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

Appendix

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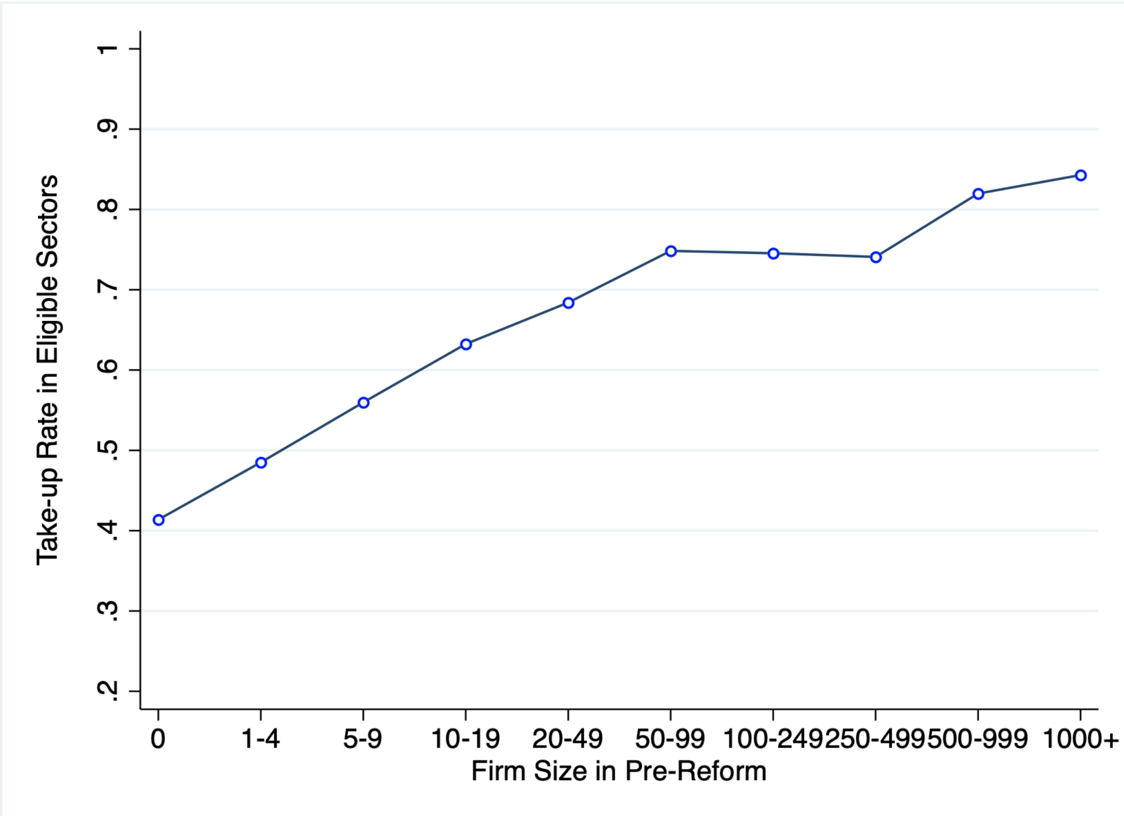
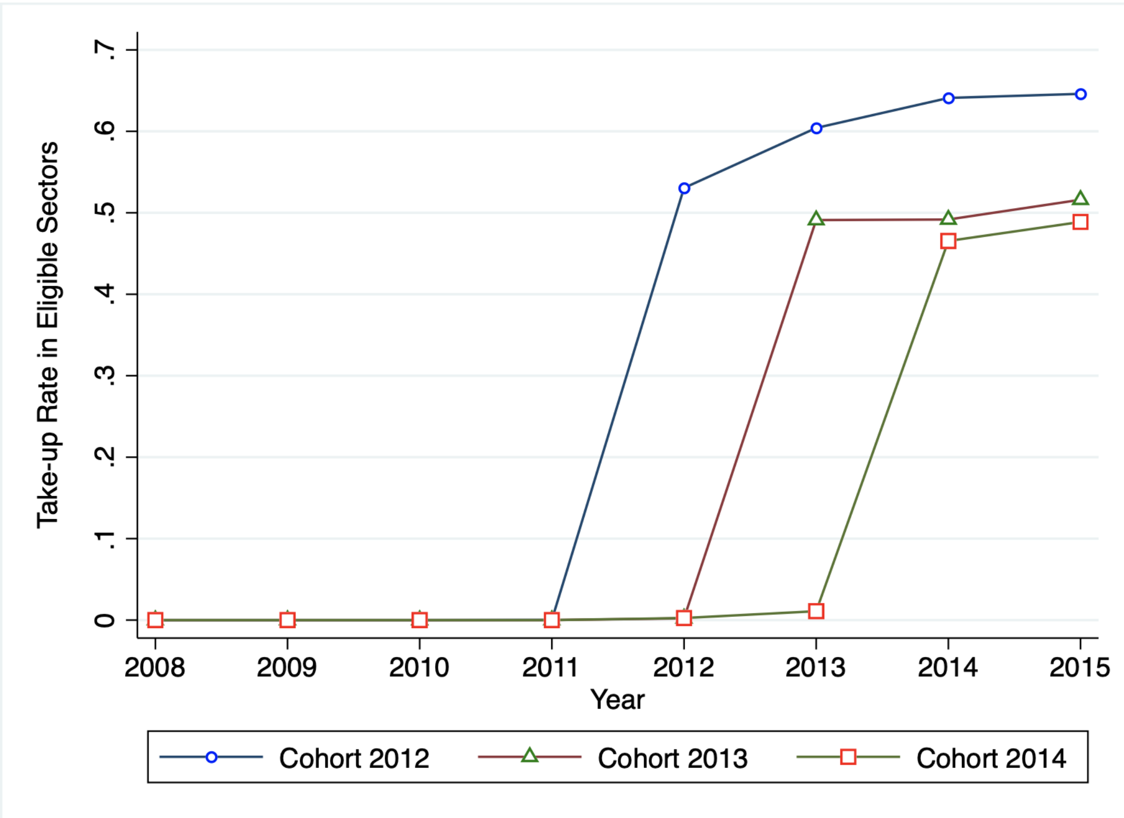
A Figures and Tables

Figure A.1: Take-up per Firm Size



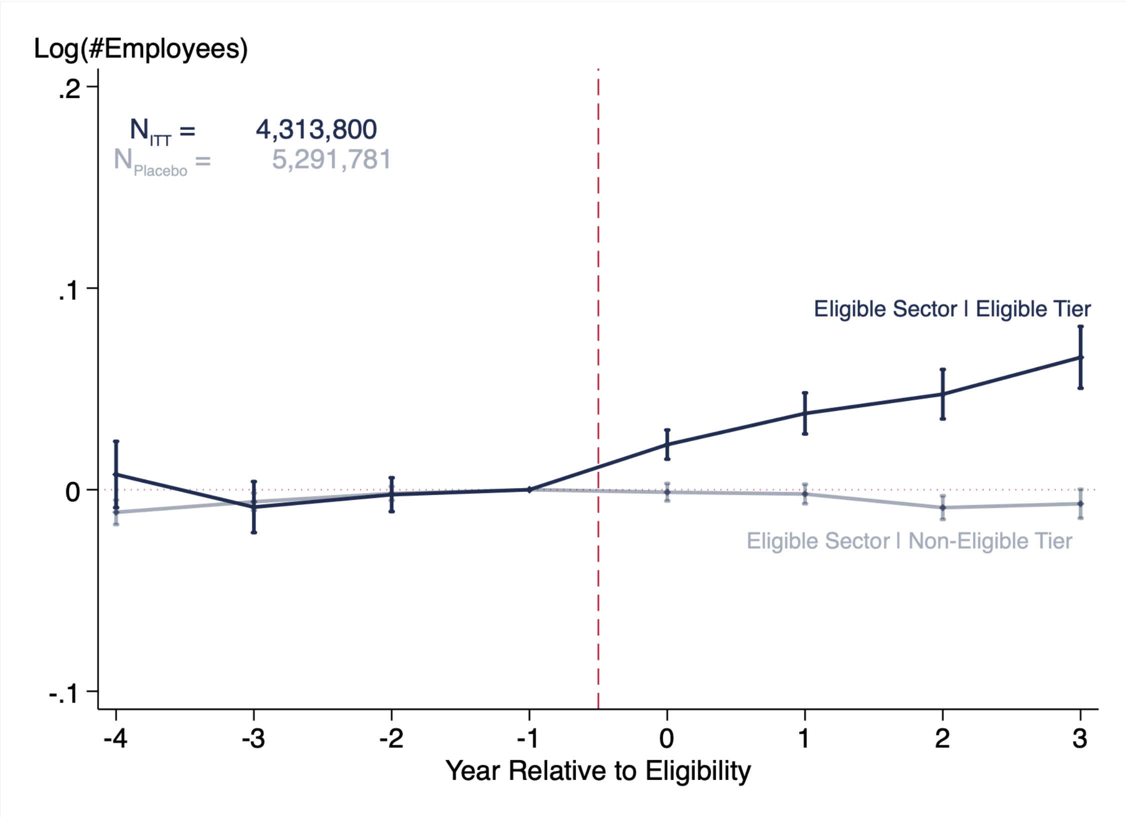
Note: This figure plots cohort-specific take-up rates among eligible firms. Eligibility is based on the firm's 7-digit sector and its observed tax tier. Firms in the "Simples" tax regime are not eligible for the reform, even if they belong to eligible sectors. Firms that have ever participated in the "Simples" regime are not included in this analysis. The figure is computed in the year 2015 after all cohorts have gained eligibility. Firm size buckets are constructed in the pre-reform years. As can be seen, eligibility is monotonically increasing with firm size.

Figure A.2: Take-up



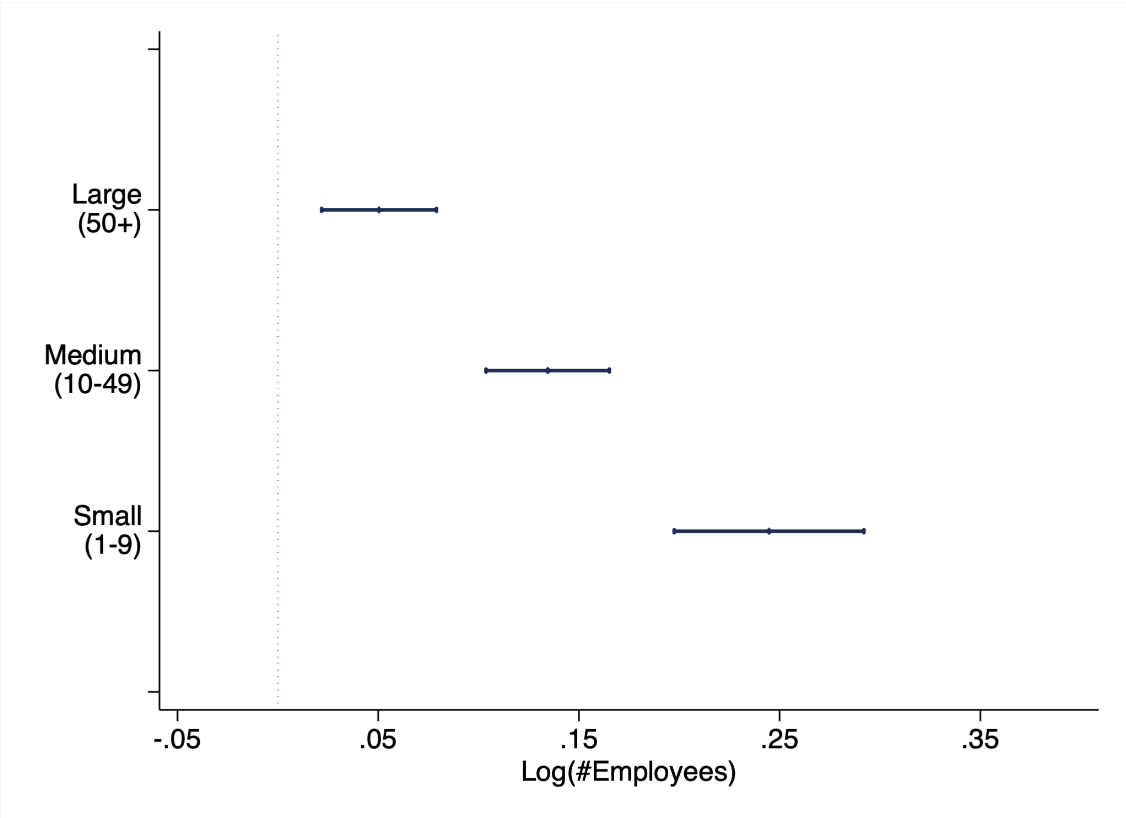
Note: This figure plots cohort-specific take-up rates among eligible firms. Eligibility is based on the firm's 7-digit sector and its observed tax tier. Firms in the "Simples" tax regime are not eligible for the reform, even if they belong to eligible sectors. Firms that have ever participated in the "Simples" regime are not included in this analysis. As expected, take-up rates are zero in the years prior to the implementation of the reform to each cohort.

Figure A.3: Spillover Test



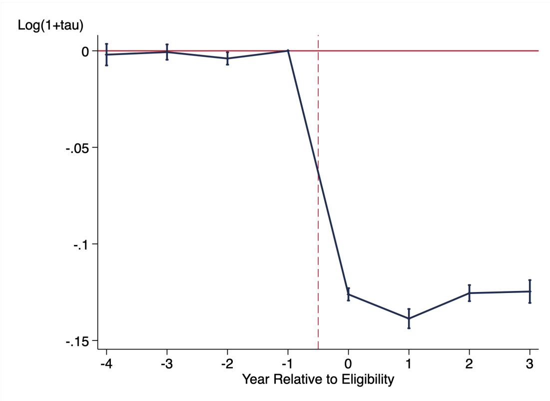
Note: The gray line plots event study coefficients that show non-statistically significant spillover effect to firms in eligible sectors, but ineligible tax tiers. The gray line is estimated on a sample that is restricted to firms in non-eligible tax tiers ("Simples" regime) and depicts a comparison 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.4: Employment by Firm Size

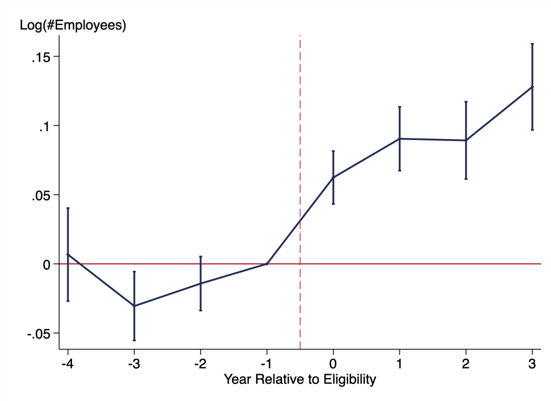


Note: This figure presents the event study estimates for the firm-level estimates, for three firm size groups (small, medium, and large firms). Size categories are defined in the pre-reform period. Firms are classified as small if they had less than nine employees, medium if they had between 10 and 49 workers, and large if they had more than 50 workers. The blue marks plot the employment difference-in-differences coefficient from the IV specification. Standard errors are reported and conservatively clustered at the 5-digit industry-by-state level.

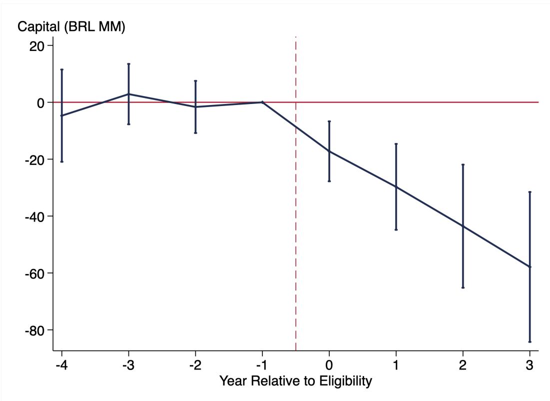
Figure A.5: Firms' Margins of Adjustment



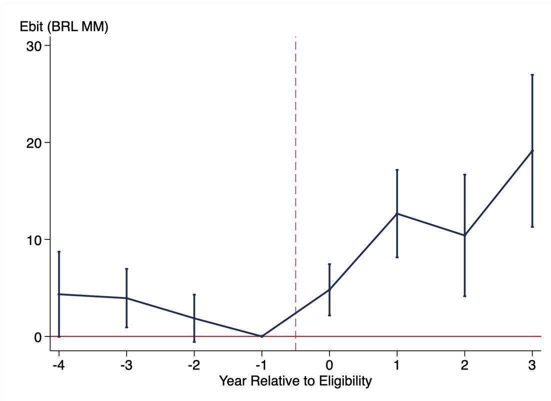
(a) Effects on payroll tax rates



(b) Effects on employment



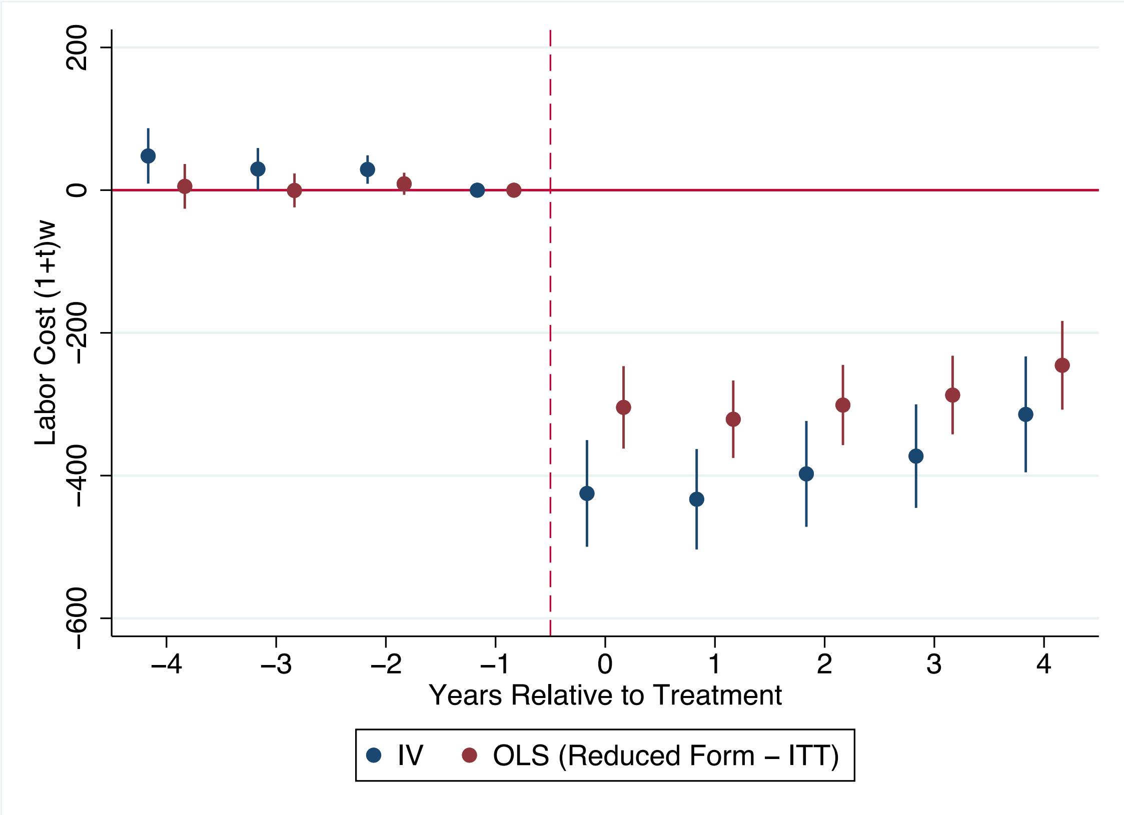
(c) Effects on capital



(d) Effects on profits

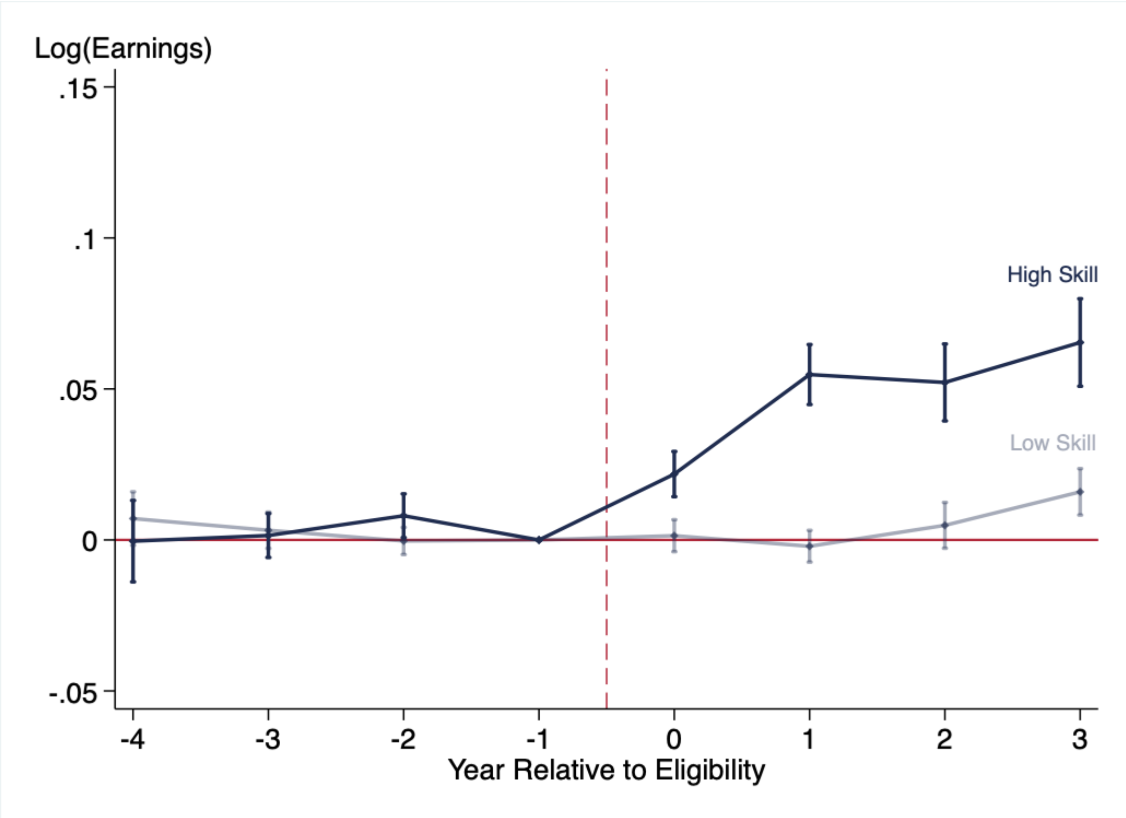
Note: This figure plots event study coefficients for multiple of the firms' margins of adjustment after the payroll tax cut. First, at the top left plot it shows the first stage, i.e., the reform induced a reduction in payroll tax liability. On the top right plot, it depicts the employment increase that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

Figure A.6: Worker Level: Gross Earnings Effect



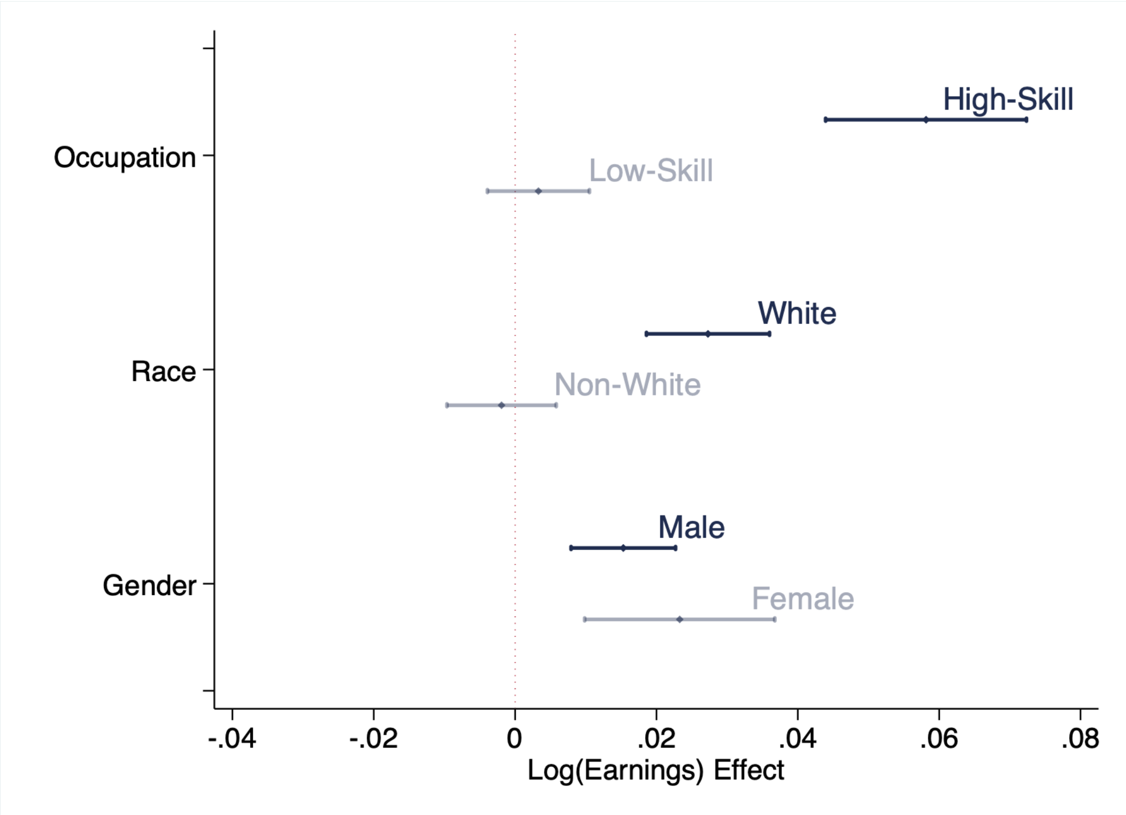
Note: This figure presents the event study estimates for average gross earnings paid workers that were employed for at least three years in the same firm during the pre-reform period. The labor cost is computed using firm-level tax data, and worker-level earnings data. I apply the firm payroll tax rate in year t, to all employees in that firm in year t. I normalize the results with respect to one year prior to the treatment event. The analysis spans four years prior to the payroll tax cut program and three years after. The plot shows an average decrease of $400 on the gross earnings, which has an approximate average of $2,300 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.7: Worker Level: Earnings per Occupation



Note: This figure presents the event study estimates for the log of 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.8: 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: Eligible vs Non-Eligible Sectors

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

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

Table A.2: 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.3: Descriptive Statistics (Worker Level Sample)

	(1) Non-Eligible (pre)	(2) Eligible (pre)	(3) Avg (pre)
Descriptive Statistics			
Earnings	2,326.54 (3,060.70)	2,166.05 (2,837.04)	2,315.46 (3,046.06)
Employees Age	39.30 (10.82)	37.20 (10.51)	39.16 (10.81)
Share White	0.67 (0.47)	0.65 (0.48)	0.67 (0.47)
Gender	0.54 (0.50)	0.79 (0.41)	0.55 (0.50)
High School +	0.70 (0.46)	0.62 (0.49)	0.70 (0.46)
College +	0.27 (0.45)	0.18 (0.38)	0.27 (0.44)
N	54,373,025	4,031,321	58,404,346

Note: This table presents descriptive statistics of the baseline worker-level sample in the pre-reform period (2008 to 2011). Each observation is a worker x year, restricted by the sample restriction of stable workers. Column (1) and (2) reports the pre-reform values for non-eligible and eligible workers, respectively. Column (3) pools these two groups together. The variable "Earnings" reports monthly earnings in December for employed workers. The currency used is the Brazilian Reais (BRL). "Employees Age" reports the age of employed workers in the sample, in December of each year. "Share White" reports the share of white workers in the sample. "High School +" reports the share of workers that achieved high school education or higher. "College +" reports the share of workers that achieved college education or higher. Standard deviations are presented in parentheses.

Table A.4: Descriptive Statistics (Firm Level Sample)

	(1) Non-Eligible	(2) Eligible	(3) Pooled	(4) Take-up
Pre-Reform Characteristics				
Employment	54.86	57.68	55.03	108.74
	(1,055.81)	(339.52)	(1,026.82)	(493.98)
Payroll Tax Rate	31.79	31.63	31.78	34.84
	(13.18)	(14.55)	(13.26)	(9.97)
Share Male	0.55	0.77	0.56	0.78
	(0.40)	(0.29)	(0.40)	(0.24)
Age	37.34	36.16	37.27	35.35
	(8.95)	(7.85)	(8.90)	(6.24)
Share High School +	0.52	0.58	0.52	0.60
	(0.41)	(0.38)	(0.41)	(0.34)
Share White	0.68	0.74	0.68	0.76
	(0.37)	(0.33)	(0.37)	(0.29)
Share Blue Collar	0.89	0.85	0.89	0.83
	(0.24)	(0.27)	(0.24)	(0.27)
N	1,775,601	114,153	1,889,754	47,315

Note: This table presents descriptive statistics of the baseline firm-level sample in the pre-reform period (2008 to 2011). Each observation is a firm x year. The descriptive statistics are presented for different groups of interest. Column (1) and (2) reports the pre-reform values for non-eligible and eligible firms, respectively. Column (3) pools these two groups together. Column (4) reports the value for eligible firms that eventually take-up the treatment. The variable "Payroll Tax Rate" informs the average payroll tax rates in (%). The variable "High School" reports the share of workers that achieved high school education or higher. The variable "Gender Composition" reports the share of male workers. The variable "Share White" informs the average share of white workers per firm. Standard deviations are presented in parentheses.

Table A.5: 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.6: 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.7: 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.8: 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	.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. 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, except for the share of male workers, a variable which we control for in all specifications. 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	Treated	Always-Takers	Never-Takers
	(1)	(2)	(3)	(4)
Employment	108.54	103.69	188.94	27.1
	(29.592)	(16.237)	(16.86)	(2.692)
Payroll Tax Rate	.33	.35	.35	.29
	(.011)	(.002)	(.002)	(.004)
Share Male	.73	.73	.71	.74
	(.02)	(.02)	(.017)	(.022)
Age	35.17	35.17	33.36	36.68
	(.326)	(.326)	(.087)	(.354)
High School +	.58	.6	.57	.58
	(.026)	(.022)	(.007)	(.022)
White	.75	.75	.76	.71
	(.025)	(.025)	(.007)	(.027)
Blue Collar	.8	.8	.92	.86
	(.018)	(.018)	(.003)	(.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. I start by studying the shape of the labor supply curve faced by the firm, based on workers' choices.

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:

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

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,

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

□

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,

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

Note that,

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

$$\epsilon^{L}_{Q} = \frac{\epsilon^{L}_{1+\tau}}{\epsilon^{Q}_{1+\tau}} \tag{11}$$

Using 5 in 11,

$$\epsilon^{L}_{Q} = \frac{-\epsilon^{L}_{1+\tau}(\frac{1}{\eta}+\epsilon^{\lambda}_{Q})}{\epsilon^{\lambda}_{1+\tau}} \tag{12}$$

Now, 12 and 9 in 10,

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

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

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

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

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

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:

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

Recall, that the elasticity of employment with respect to labor cost $\epsilon^L_{1+\tau}$ 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,

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

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. This result makes intuitive sense, as most of the action in this reform is on 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 in a money metric approach that aggregates the net benefit and costs of payroll taxes.

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

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

The efficiency gain induced by a discrete payroll tax cut can be computed following the steps outlined in section 5.2:

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

Taking Equation 2 to the data, we obtain a precise measure of the deadweight loss associated with payroll taxation. To obtain further theoretical intuition of the role of market power on tax efficiency, I totally differente 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:

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

Theoretically, the impact of market power on the deadweight loss of taxation is dictated by two opposing forces. By one hand, market power reduces the distortions through behavioral responses. Firms with less market power are more responsive to taxation. By the other hand, equation 8 shows that market power increases the distortions through lower ϵ and η. Using the parameters estimated in Brazil, I find that the behavioral response channel dominates, and the market power reduces the deadweight loss of taxation.

E 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 ($\epsilon_{1+\tau}^{\lambda}$, ϵ_{Q}^{λ}, $\epsilon_{1+\tau}^{Q}$):

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

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

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.

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

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

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:

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

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

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 E.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 E.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 E.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.

F Capital Taxation with Monopsony

The model developed in this paper to study payroll taxation can be easily extended to analyze capital taxes, in which case it becomes similar to the model developed by Curtis et al. 2021, except that I allow for monopsony power in the labor market. The derivation is analogous to the one presented in Section C, but since the tax applies to capital, the firm minimizes the following cost function:

$$\min_{K,L} \quad A_0 L^{\frac{1}{\epsilon}+1} + rK(1+\tau)$$
$$\text{s.t.} \quad (s_L L^\rho + s_K K^\rho)^{\frac{1}{\rho}} \geq Q$$

Capital Tax in Monopsony vs Perfect Competition. Applying similar envelope arguments as in the main model, I obtain the following predictions for the pass-through of capital taxes on employment, capital, and revenue:

$$\epsilon^L_{1+\tau} = \frac{(1-(1-\rho)\eta)\epsilon^2 s_K}{(1-\epsilon\rho+\epsilon)(\epsilon+s_L) - (1-(1-\rho)\eta)(\epsilon+1)s_L}$$

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

$$\epsilon^R_{1+\tau} = \frac{(1-\eta)}{(\epsilon+s_L)\epsilon}(\epsilon^2 s_K + (\epsilon+1)s_L\epsilon^L_{1+\tau})$$

To obtain analytical solutions to directly compare the pass-through formulae with previous studies that relied on perfectly competitive labor markets, I take the limit of each of the model's predictions when $\epsilon \to \infty$. Applying L'Hôpital's rule, our expressions boil down precisely to the standard equations:

$$\lim_{\epsilon\to\infty} \epsilon^L_{1+\tau} = s_K(\sigma - \eta)$$

$$\lim_{\epsilon\to\infty} \epsilon^K_{1+\tau} = -\eta s_K - \sigma s_L$$

$$\lim_{\epsilon\to\infty} \epsilon^R_{1+\tau} = s_K(1-\eta)$$

Identification. From the capital and revenue equations, I can use the capital tax variation to identify the capital-labor elasticity of substitution under monopsony:

$$\sigma_{monop} = \frac{\epsilon^K_{1+\tau}(\epsilon+s_L)\epsilon + \epsilon^2 s_K + (\epsilon+1)s_L\epsilon^L_{1+\tau} - \epsilon^R_{1+\tau}(\epsilon+s_L)\epsilon}{\epsilon^2 s_K + (\epsilon+1)s_L\epsilon^L_{1+\tau} - \epsilon(\epsilon+s_L)} \tag{1}$$

$$\sigma_{comp} = \frac{\epsilon^R_{1+\tau} - \epsilon^K_{1+\tau} - s_K}{s_L} \tag{2}$$

For sake of benchmarking, I rely on the empirical findings from the capital tax literature, which examines the effect of capital taxes on employment, capital, and revenue. I incorporate their reduced-form estimates into equations 1 and 2 to yield the structural estimates for the elasticity of substitution between capital and labor under monopsony and perfect competition. To ensure full comparability, I also derive the structural parameters based on my empirical findings from the Brazilian payroll tax cuts, using a model of a perfectly competitive labor market. The results are consolidated in Table F.1.

Table F.1: Estimates Under Monopsony vs Perfect Competition

	(1) **Capital Tax** (Curtis et al. 2023)	(2) **Payroll Tax** (Lobel 2023)
Panel A: K-L Elasticity, σ_{KL}		
Perfect Competition	-0.21	1.30
Monopsony	0.24	1.72
% Difference	·	**33%**
Panel B: Empirical Moments		
Capital Elasticity	-0.57	0.27
Revenue Elasticity	-0.54	-0.4
Labor Elasticity	-0.83	-1.02

Notes: This table compares the estimates of the capital-labor elasticity of substitution from a payroll and capital tax shift. The table also compares the estimation across models of perfect competition vs monopsony. For consistency, I use the long-diff coefficients for the empirical moments on the payroll tax variation.

A noteworthy point is a negative estimation under capital taxation and perfect competition, which contradicts cost minimization behavior. However, this is precisely what 2021 found in their alternative two-input specification. They estimated σ_{KL} to be greater than zero in their primary three-input specification. I didn't simulate their primary model under monopsony to maintain comparability with my setting, where firms select only two inputs: capital and labor. The goal of this exercise is to hold all else constant and illuminate the role of labor market power in the estimation of capital-labor substitution. As this paper has documented the existence of labor market power in the real world, neglecting this friction during structural estimation could result in a 33% bias in the estimated elasticity of substitution between capital and labor. This is a non-trivial bias, and it is important to bear in mind when interpreting benchmark estimates in the literature.

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. Tables G.1 and G.2 below showcase the estimates derived from the firm-level analysis, fitted to this balanced sample. 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*** (.015)	.025** (.013)	.015 (.011)	.007 (.01)	.001 (.003)
Long Diff	.082*** (.017)	.038*** (.013)	.016 (.011)	-.002 (.01)	.003 (.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.

Table G.2: Firm Level Estimates

	(1) Log(1+τ)	(2) Log(#Employees)	(3) Log(Earnings)
Panel A: IV			
Diff-in-Diff	-.136***	.114***	.019*
	(.003)	(.03)	(.011)
Long Diff	-.121***	.155***	.024**
	(.004)	(.03)	(.01)
Panel B: ITT			
Diff-in-Diff	-.075***	.063***	.01*
	(.003)	(.017)	(.006)
Long Diff	-.075***	.083***	.014**
	(.003)	(.019)	(.006)
Controls	✓	✓	✓
Firm FE	✓	✓	✓
Sector x Year FE	✓	✓	✓
# Clusters	7,824	7,924	7,824
N	2,422,141	2,491,523	2,422,141

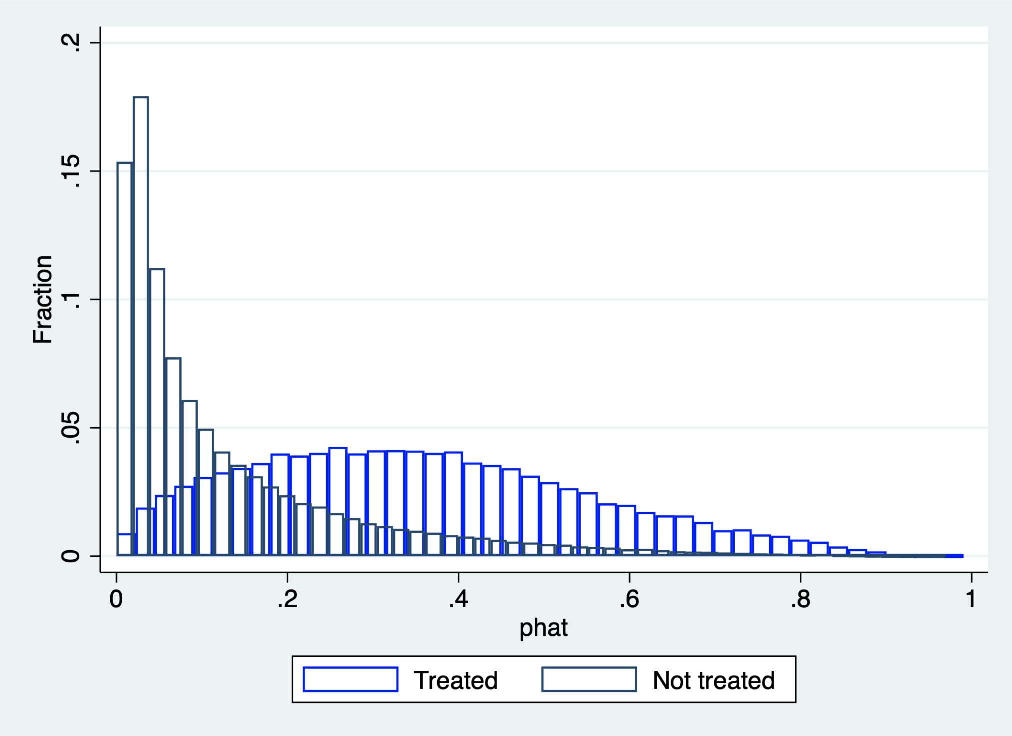
Note: This table presents IV and reduced form (ITT) estimates for the firm-level balanced sample. Difference-in-differences coefficient is estimated in equations 3 and 4, where there is only one post period. The long difference comes from the period t=+3, in the event study design. Panel A reports the IV coefficients, which adjust for imperfect compliance. Panel B reports the reduced form coefficients, which are interpreted as the intention to treat (ITT) coefficients. Column (1) reports the policy induced labor cost variation, which provides evidence on the first stage. The remaining columns have log of employment, as the dependent variable. Column (2) presents the average effect in the whole sample. Columns (3-5) present heterogeneity based on pre-reform firm size. Firms are categorized as small if they have less than 9 workers in the pre-period. Medium if they have 10-49, and large if they have more than 50 workers. 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.3 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.

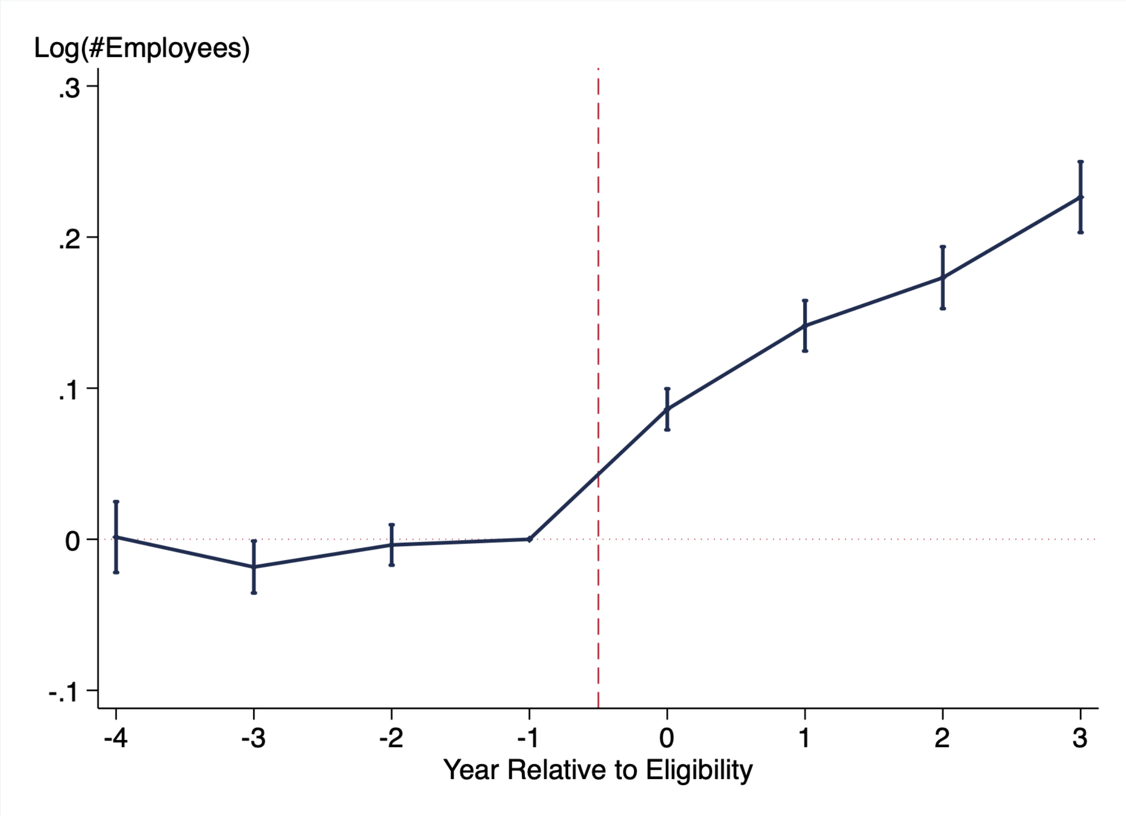
Table G.3: Balance on Matched Sample

	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.5 reports. To showcase that the matching algorithm still works in the placebo sample, Table G.4 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.4: 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.5: 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: Tax Forms Information

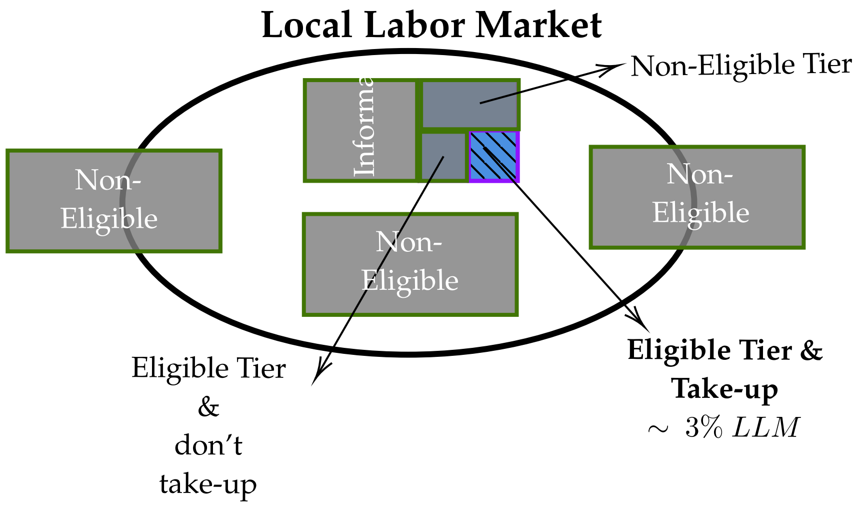
Nº	Campo	Descrição	Tipo	Tam	Dec	Obrig
01	REG	Texto fixo contendo "0145".	C	004*	-	S
02	COD_IN C_TRIB	Código indicador da incidência tributária no período: 1 – Contribuição Previdenciária apurada no período, exclusivamente com base na Receita Bruta; 2 – Contribuição Previdenciária apurada no período,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

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

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

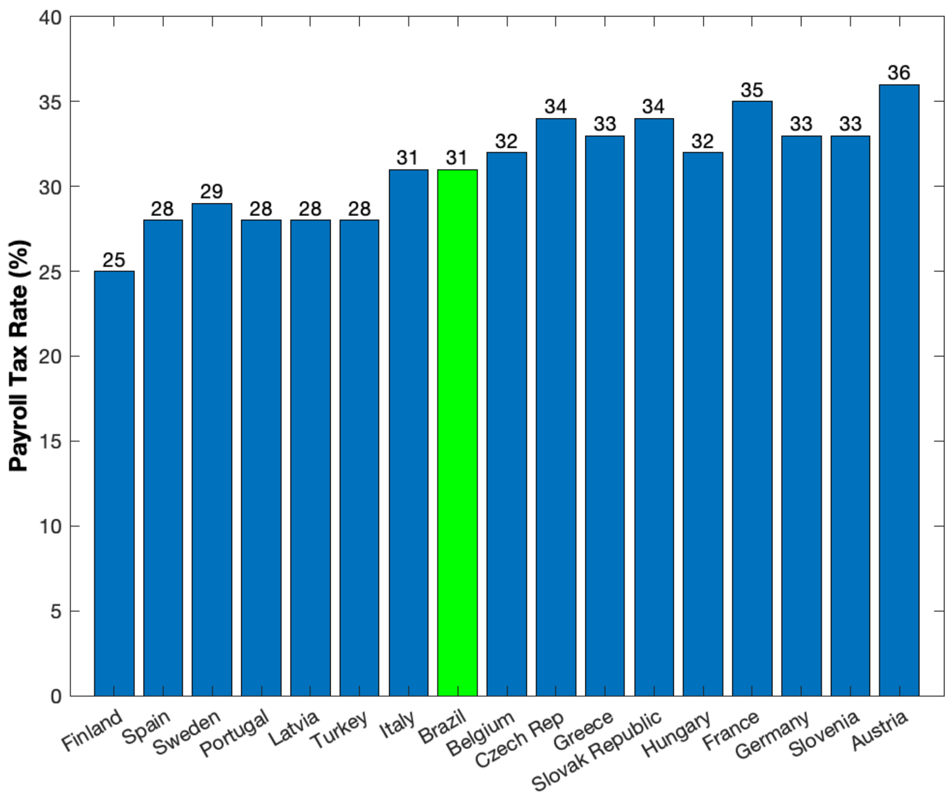
Note: This figure shows instructions for eligible firms to request the payroll tax benefit. It describes detailed information to be provided in Tax Administration software, in order to substitute part of the payroll tax by revenue taxes.

Figure H.2: Illustration of Treatment Coverage in LLM



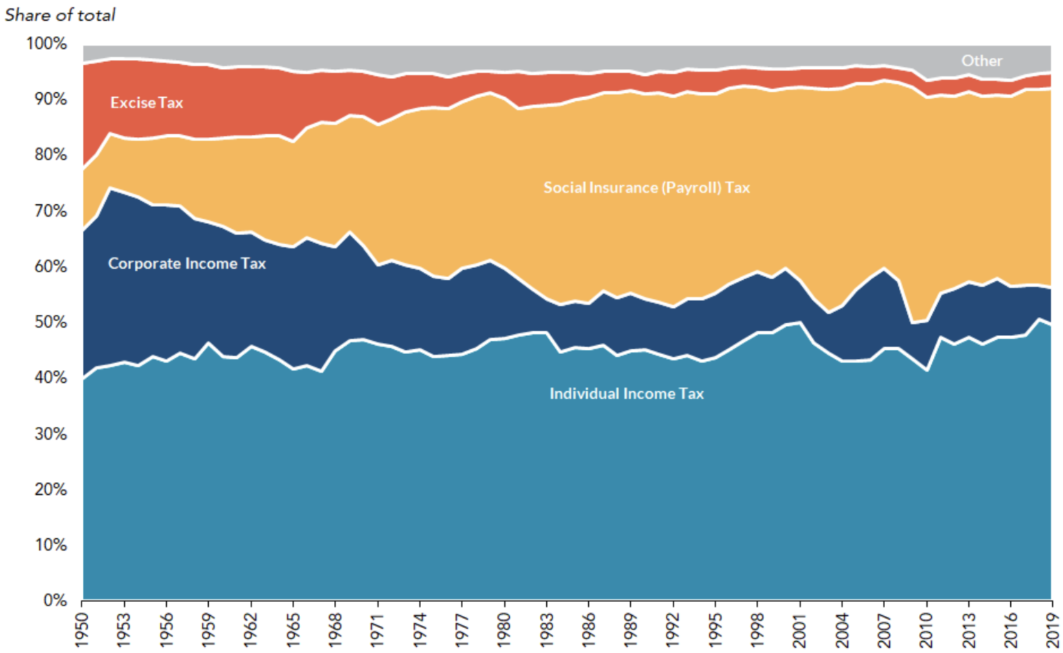
Note: This figure illustrates the policy coverage within local labor markets (LLM), which are defined as occupation x region cells according to job switching patterns. The figure shows that within LLM there are eligible and non-eligible sectors. In the eligible sectors, there is ineligibility due to informality, non-eligible tax tier ("Simples"), and imperfect take-up. All together, the figure illustrates that conditional on existence of an eligible sector in a local labor market, the share of treated firms in the LLM is approximately 3%.

Figure H.3: Payroll Tax Rates Around the World



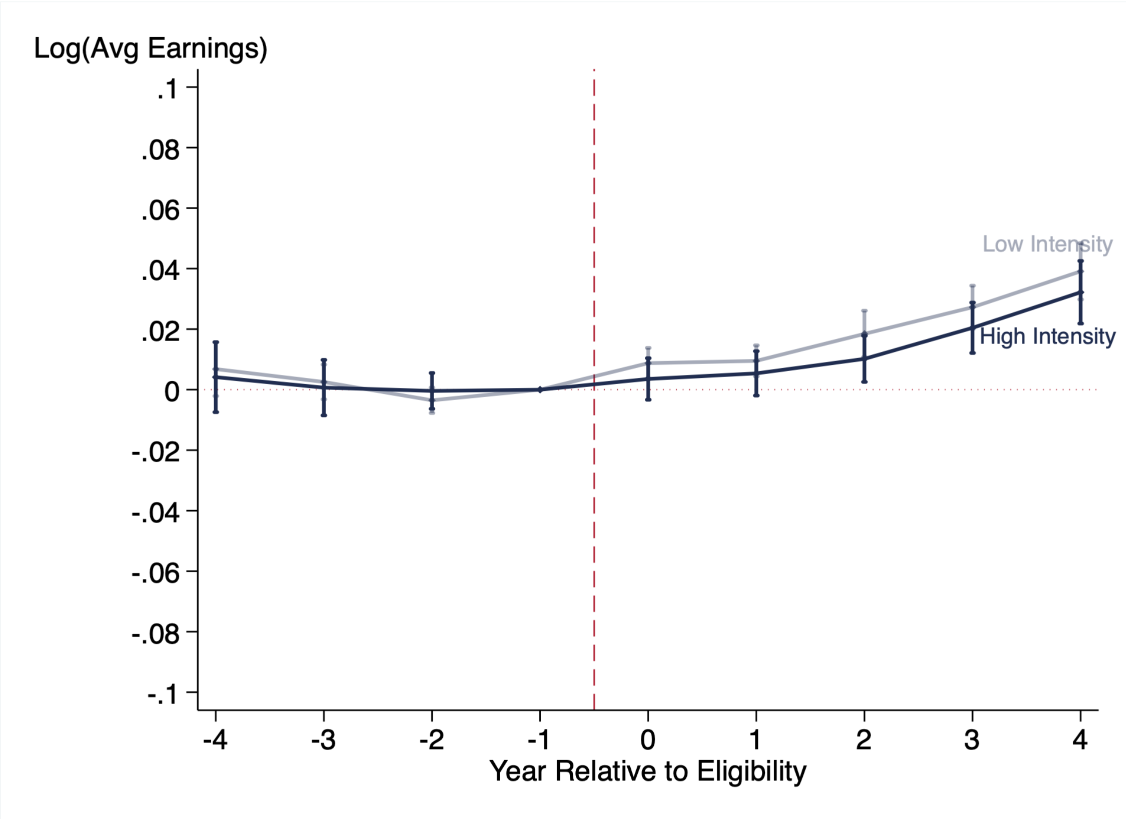
Note: This figure reports payroll tax rates around the world. The payroll tax rate is composed by the sum of employer and employee's contributions.
Source: Elaborated by author, based on information from OECD 2019.

Figure H.4: Sources of Government Revenue



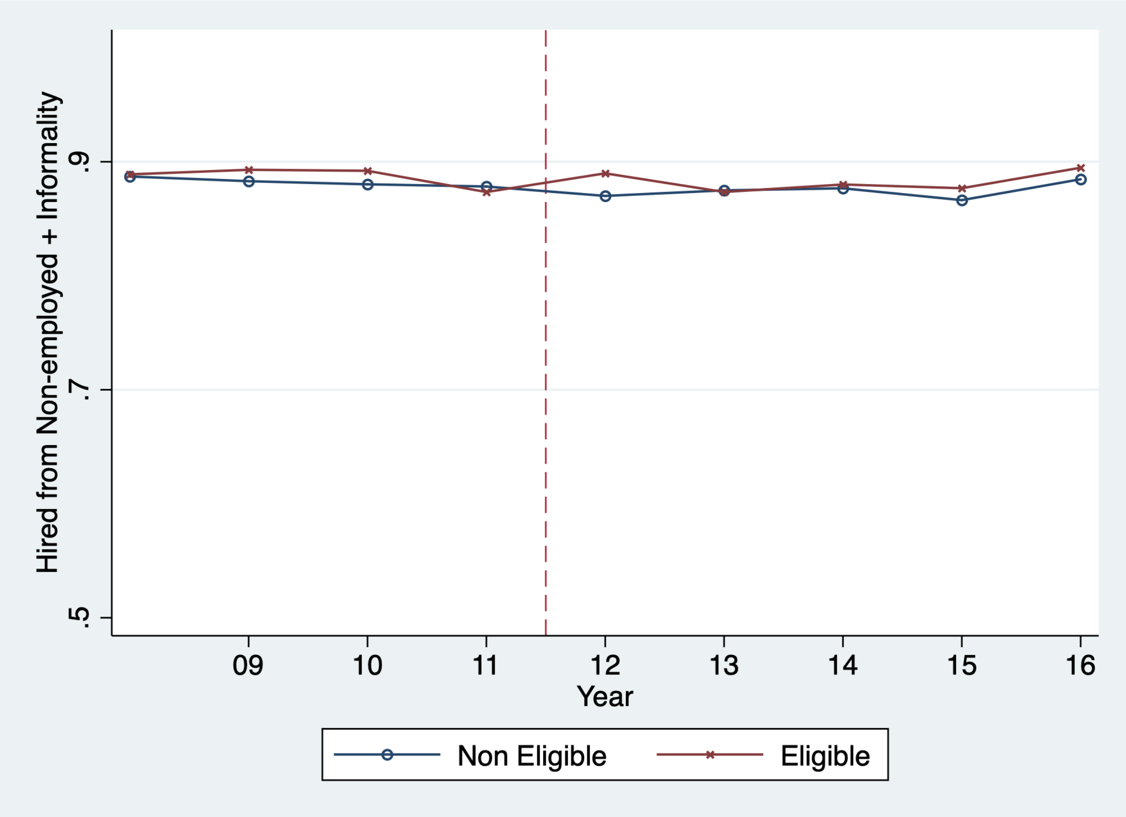
Note: This figure shows the growing importance of payroll taxes compared to other sources of revenue for the US Federal Government. Currently, payroll taxes are the second most important source, accounting for more than 30% of total revenue.
Source: Office of Management and Budget. Historical Tables. Table 2.1, "Receipts by Source: 1950-2025"

Figure H.5: Firm vs Market Level Shock



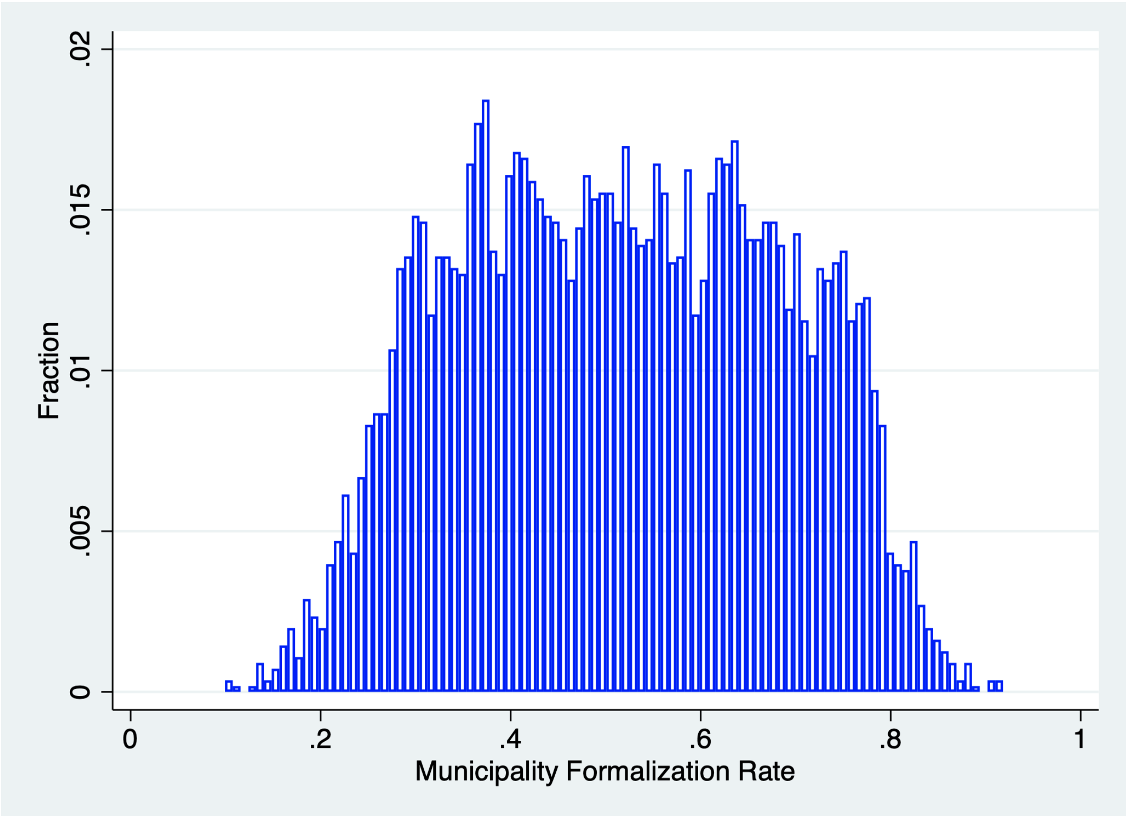
Note: This figure provides an additional test on the spillover effect. It compares the worker's earnings effect for high and low intensively treated markets. To measure market treatment intensity I compute the share of treated workers in each labor market, which are defined by the occupation x region 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.6: New Hires Origin by Eligibility Status



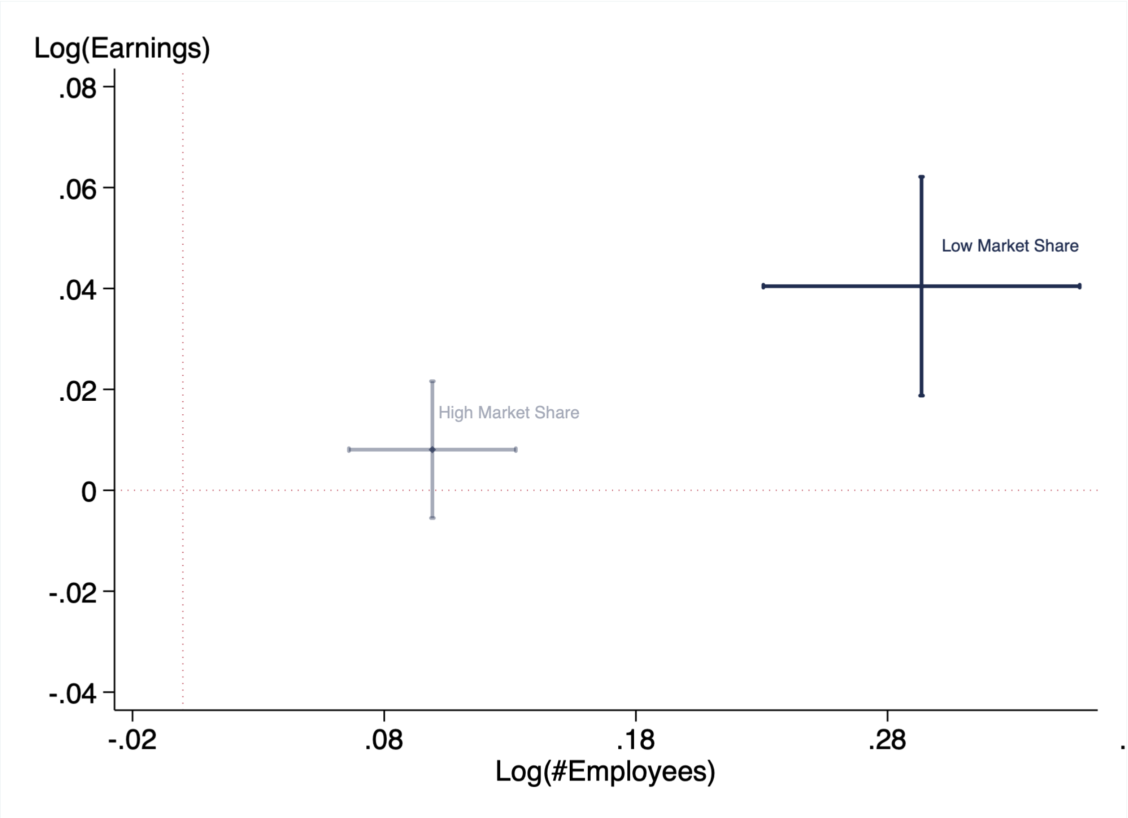
Note: This figure plots the share of new hires coming from non-employment or informality. A new hire is classified as previously informal or non-employed if she was not holding a formal job in the three months prior to being hired. Eligibility is defined based on the sector of employment.

Figure H.7: Formalization Rates per Municipality



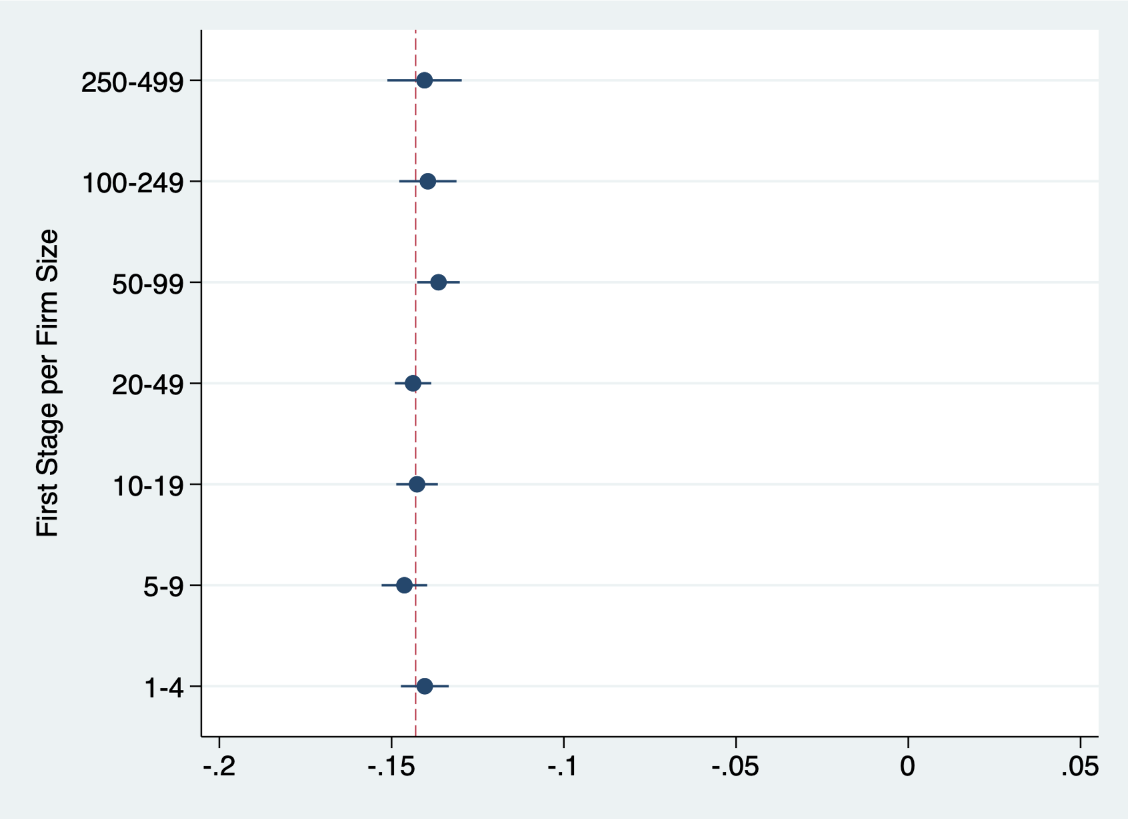
Note: This figure presents the distribution of formalization rates per municipalities in Brazil, according to the 2010 Census. There are 5,300 municipalities with heterogeneous informality rates.

Figure H.8: Earnings and Employment per Market Concentration



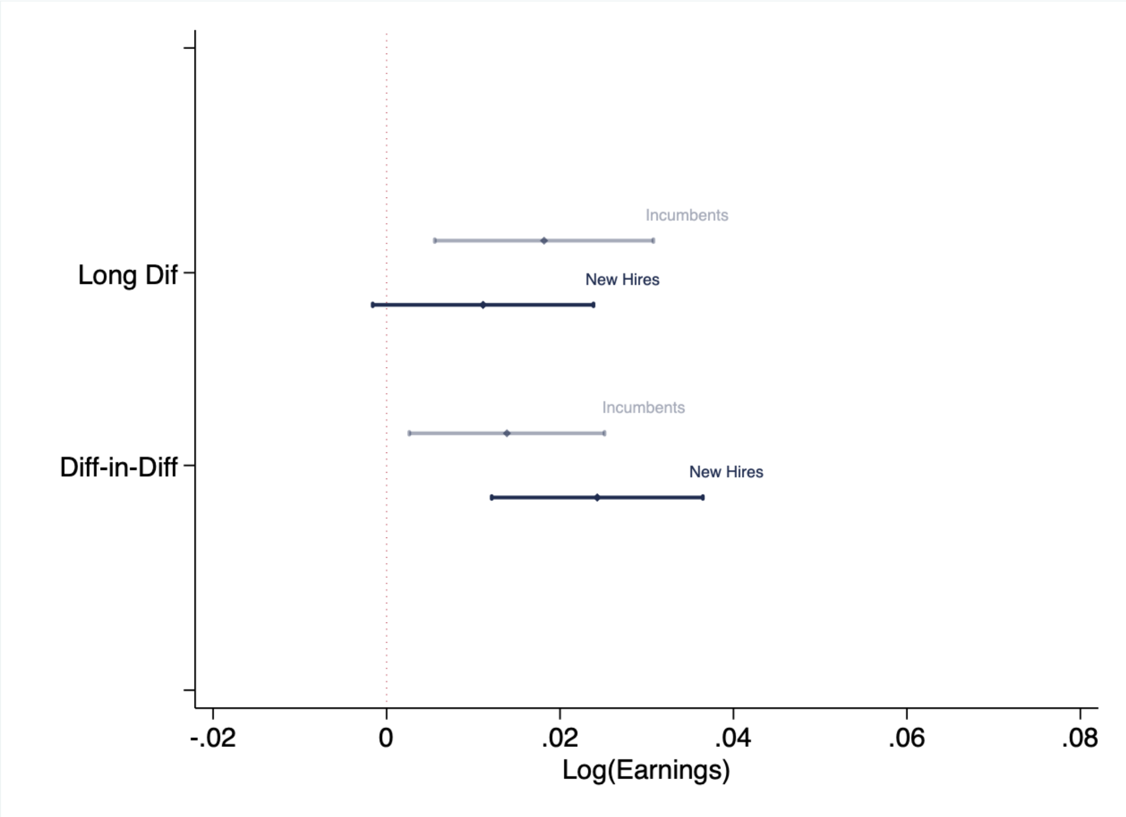
Note: This figure presents firm-level IV difference-in-differences coefficients for above and below the median on pre-reform employment market share within each local labor market. The outcomes are employment and earnings. The blue marker plots the effect for firms below the median (low market power), 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.

Figure H.9: First Stage per Firm Size



Note: This figure presents the difference-in-differences IV estimate for the effect of the tax treatment on the log of labor cost. Firms are categorized on size bins based on pre-reform levels. It shows that the effect of the reform on labor cost is similar for all size bins. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Figure H.10: Pass-Through to New Hires and Incumbent Workers



Note: This figure presents firm-level difference-in-differences coefficient and event study coefficients in t+3. The blue dots represent estimates for new hires, whereas the gray bars are the incumbents' response. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table H.1: List of Eligible Sectors

Description (7-Digit Sector)	Industry	Year
Development and licensing of customizable computer programs	IT	2012
Technical support, maintenance and other information technology services	IT	2012
Data processing, application service providers and internet hosting services	IT	2012
Development of customed computer programs	IT	2012
Web design	IT	2012
Consulting in information technology	IT	2012
Development and licensing of computer programs	IT	2012
Call center activities	Call Center	2012
Hotel	Lodging	2012
Manufacturing of eletronic games	Manufacturing	2012
Manufacturing of eletronic components	Manufacturing	2012
Aircraft maintenance and repair, except runway	Maintanance	2013
Aircraft maintenance on the runway	Maintanance	2013
Maritime support navigation	Transportation	2013
Port support navigation	Transportation	2013
Maintenance of vessels and floating structures	Transportation	2013
Maintenance and repair of boats for sport and leisure	Transportation	2013
Construction of buildings	Construction	2013
Electrical installation and maintenance	Construction	2013
Waterproofing in civil engineering works	Construction	2013
Installation of doors, windows, ceilings, partitions and built-in cabinets of any material	Construction	2013
Plaster and stucco finishing works	Construction	2013
Building painting services in general	Construction	2013
Application of coatings and resins	Construction	2013
Other construction finishing works	Construction	2013
Foundational construction work	Construction	2013
Bakery and confectionery with predominance of resale	Retail and Motovehicles	2013
Retail trade of dairy and cold products	Retail and Motovehicles	2013
Retail sale of sweets, candies, bonbons and the like	Retail and Motovehicles	2013
Specialized retail trade of computer equipment	Retail and Motovehicles	2013
Refilling cartridges for computer equipment	Retail and Motovehicles	2013
Specialized retail trade of telephony	Retail and Motovehicles	2013
Specialized retail trade of home appliances	Retail and Motovehicles	2013
Furniture retail trade	Retail and Motovehicles	2013

Fabric retail trade	Retail and Motovehicles	2013
Retail sale of haberdashery items	Retail and Motovehicles	2013
Retail trade of bed, table and bath articles	Retail and Motovehicles	2013
Retail sale of upholstery, curtains and blinds	Retail and Motovehicles	2013
Retail sale of other household articles	Retail and Motovehicles	2013
Book retail trade	Retail and Motovehicles	2013
Retail trade of newspapers and magazines	Retail and Motovehicles	2013
Stationery retail trade	Retail and Motovehicles	2013
Retail sale of records, CDs, DVDs and tapes	Retail and Motovehicles	2013
Retail sale of toys and recreational items	Retail and Motovehicles	2013
Retail trade of sporting goods	Retail and Motovehicles	2013
Retail sale of cosmetics and perfumery	Retail and Motovehicles	2013
Retail sale of clothing and accessories	Retail and Motovehicles	2013
Footwear retail trade	Retail and Motovehicles	2013
Travel goods retail trade	Retail and Motovehicles	2013
Retail sale of household cleaning products	Retail and Motovehicles	2013
Retail sale of photographic and filming articles	Retail and Motovehicles	2013
Retail sale of pharmaceutical products	Retail and Motovehicles	2013
Construction of roads and railways	Construction	2014
Painting for signs on highways and airports	Construction	2014
Construction of special works of art	Construction	2014
Urbanization works - streets, squares and sidewalks	Construction	2014
Construction of dams for power generation	Construction	2014
Construction of stations and electricity networks	Construction	2014
Maintenance of electricity distribution networks	Construction	2014
Construction of stations and telecommunications	Construction	2014
Maintenance of stations and telecommunications	Construction	2014
Construction of water supply networks	Construction	2014
irrigation works	Construction	2014
Construction of pipeline transport networks, except for water and sewage	Construction	2014
Port, maritime and river works	Construction	2014
Assembly of metal structures	Construction	2014
Industrial assembly works	Construction	2014
Demolition of buildings and other structures	Construction	2014
Site preparation and land clearing	Construction	2014
Drilling and soundings	Construction	2014
Earthworks	Construction	2014
Land preparation services not otherwise specified	Construction	2014
Collective road transport of passengers (fixed route)	Transportation	2013
Collective road transport (fixed itinerary)	Transportation	2013

Collective road transport (metropolitan region)	Transportation	2013
Collective road transport of passengers (interstate)	Transportation	2013
Public passenger transport by road (international)	Transportation	2013
Transport by inland freight navigation	Transportation	2013
Transport by inland cargo navigation	Transportation	2013
Maritime cabotage transport - Cargo	Transportation	2013
Cabotage maritime transport - Passengers	Transportation	2013
Long haul maritime transport - Cargo	Transportation	2013
Long haul maritime transport - Passengers	Transportation	2013
Regular passenger air transport	Transportation	2013
air freight transport	Transportation	2013
Transport by inland navigation (municipal)	Transportation	2013
Transport by inland navigation (interstate)	Transportation	2013
Intercity and interstate passenger rail transport	Transportation	2014
Railway passenger transport in the city	Transportation	2014
Metro transport	Transportation	2014
Loading and unloading	Transportation	2014
Port infrastructure management	Transportation	2014
Port Operator Activities	Transportation	2014
Management of waterway terminals	Transportation	2014
Road freight transport (municipal)	Transportation	2014
Cargo road transport (interstate)	Transportation	2014
Road transport of dangerous goods	Transportation	2014
Road transport of removals	Transportation	2014
Rail freight transport	Transportation	2014
Newspaper printing	Media	2014
Printing of books, magazines and other periodicals	Media	2014
Book editing	Media	2014
Editing of daily newspapers	Media	2014
Editing of non-daily newspapers	Media	2014
Magazine editing	Media	2014
Integrated edition to print daily newspapers	Media	2014
Integrated edition to the printing of non-daily newspapers	Media	2014
Editing integrated with magazine printing	Media	2014
Radio activities	Media	2014
Open television activities	Media	2014
Portals, content providers and information services	Media	2014

Note: This table list the eligible sectors at its most granular sector definition (7-digits). The sector definition used in the tax bills are the CNAE classification administred by the Brazilian Census Bureau (IBGE). The table also reports the broader 1-digit industry for each eligible sector, and the year in which they gained eligibility.

Table H.2: 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.3: Balance Test (Firm Level)

	OLS	**TWFE**
Employment	14.59	1.09
	$[-11.34, 40.52]$	$[-4.52, 6.69]$
Age	-1.24	0.07
	$[-2.72, 0.23]$	$[-0.09, 0.23]$
College +	0.02	-0.00
	$[-0.03, 0.07]$	$[-0.01, 0.00]$
Race	0.06	-0.00
	$[-0.00, 0.12]$	$[-0.01, 0.01]$
Gender	0.22	0.00
	$[0.13, 0.30]$	$[-0.00, 0.01]$
High School +	0.07	-0.00
	$[-0.03, 0.17]$	$[-0.01, 0.01]$
Firm FE	×	✓
Sector x Year FE	×	✓
N	1889754	1776214
Clusters	10516	9925

Note: This table reports the results of balance test for the firm-level sample, which consists on regressing firms' characteristics on the time-invariant eligibility dummy. The model is fitted in the pre-period (2008-2011), and the unit of observation is firm x year. The basline model is: $X_{jt} = L_{s(j)} + u_{jt}$, and the TWFE model: $X_{jt} = L_{s(j)} + \alpha_j + \gamma_t + \xi_{I(j),t} + u_{jt}$, where $Ls(j)$ is a dummy to indicate if the firm was ever eligible; X_{jt} are firms' characteristics and the fixed effects are the same used in all firm-level specifications presented before. The first column displays the results for the baseline (OLS) model. The second column reports the values for the TWFE. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table H.4: Comparison Across Methods

Structural Estimates	(1) Baseline	*Direct Estimation* (2) Small Firms	(3) Large Firms	*CMD* (4) Small Firms	(5) 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.