# The Visible Hand of Firms: Consequences for Efficiency and Incidence of Payroll Taxation

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**Abstract**

This paper studies a historically large payroll tax cut that affected a subset of Brazilian firms. Difference-in-differences estimates based on plausibly arbitrary legal variation, report 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. In terms of mechanisms, two-thirds of the employment effect arises from plant size expansion rather than input substitution alone. In a novel finding relying on a full-incidence framework, the study reveals that consumers pay 75% of payroll taxes, while firm owners and workers pay 25%. I develop, identify and estimate a model of factor demand that quantifies the role of firms' (product and labor) market power on the efficiency costs of taxation, as well as its distributional implications. Market power shifts the incidence away from workers towards firm owners and consumers. The model quantifies that a targeted alternative tax policy focusing on small firms could amplify efficiency gains by 36% while enhancing workers' welfare gains by 95%.

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# 1 Introduction

Payroll taxes are responsible for 30% of total Government revenue (OECD 2019). Recognizing the fiscal burden on employment decisions, many countries have adopted targeted payroll tax cut programs. Such policies prompt a pressing question in the public debate regarding who benefits from payroll tax cuts. I argue the answer depends on market power. Predicated on the basic idea that the most elastic side bears more of the tax, the traditional Public Finance view has focused on comparing aggregate labor demand versus supply. Underlying this logic is the assumption that incidence is determined by market forces, often referred as the "invisible hand" (Smith 1776).

Despite mounting evidence documenting firms' wage setting power (see Card et al. 2018 for a review), economists haven't incorporated yet the role of firms into the study of tax incidence. Upon documenting clear evidence of market power in Brazil, this work aims to fill the following gap: How does labor and product market power affect tax efficiency and incidence?

In this paper, I provide novel evidence on the incidence of payroll taxation by studying a quasi-experimental historically large labor cost reduction of 15p.p., in the context of a payroll tax cut program in Brazil. Due to plausibly exogenous sector-specific legal requirements, the policy affected only a small (even within local labor markets) share of firms. This setting alleviates general equilibrium concerns, enabling precise estimation of firm and worker-level responses. To exploit this variation, I take advantage of novel, anonymized administrative tax data encompassing the universe of formal firms. The richness of the data, coupled with the unique tax reform, enables me to conduct a thorough evaluation among a broad range of outcomes, across different types of workers and firms.

The main empirical strategy leverages an event study instrumented by sector eligibility. As eligible and non-eligible firms are remarkably similar and, not surprisingly, exhibited similar trends before the reform, they provide a compelling basis for comparison. I find that the tax cut caused a 9% employment increase, phenomenon even more pronounced among small firms. Given the underlying payroll tax variation induced by the reform, the implied elasticity of employment with respect to labor cost is -0.71. I find that the employment growth does not lead to an upgrade on the workforce composition. However, I do note that the earnings pass-through exacerbates within-firm inequality. Whereas workers at the bottom percentiles of the within-firm distribution do not experience any pay increase, there are substantial gains (of 14%) at the top.

To examine the pass through to earnings and minimize the contamination from workers' turn over, I follow the displacement literature (Jacobson, LaLonde, and Sullivan 1993, Lachowska, Mas, and Woodbury 2020) to build a sample of stable incumbent workers, who are assigned to treatment based on their pre-reform employers. At the worker-level, I find an average 1.8% increase in earnings, with notable dynamics. In particular, there is no immediate effect, but a significant 3% increase three years after implementation. There is no significant difference across gender. However, consistent with the unequal pass-through inside firms, there are significant differences across occupations. Specifically, high-skilled workers benefit from a 6% pass-through, while low-skilled workers witness no gains from the same tax cut.

The identifying assumption is that conditional on fixed effects, eligibility is uncorrelated with time-varying unobserved determinants of employment and wage growth. There are two threats to the validity of this design. The first relates to selection on eligibility, i.e., that Congress did not anticipate sector-specific trends when defining the eligibility rules. I address this concern in several ways. First, I show not only that the 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 status, as illustrated by the cases of hotels and motels. Additionally, as a robustness, I recover determinants of eligibility using a logit model and apply the associated propensity scores in a matching difference-in-differences procedure, which relies on the conditional independence assumption (CIA), as opposed to the policy's legal variation. Results from both methods are qualitatively similar.

The second threat relates to manipulation of sectoral choice. Identification would be compromised if firms strategically select into eligible sectors, after the reform has been announced. In order 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 where I do observe sector changes, there is not a trend of switching towards eligible sectors. As an additional robustness check, I restrict the sample to firms that have never changed sectors and results remain similar. All of these, confirms that results are not driven by firms self selecting into eligible sectors.

Once the employment effect is established, one might suspect that the employment increase could reflect a mere shift from existing informal into formal

jobs. This margin of response is particularly relevant in the landscape of developing countries (Ulyssea 2018; Haanwinckel and Soares 2021). Nevertheless, I conduct several tests indicating that informality is not playing a major role in response to the payroll tax variation. First, I leverage the panel structure of the data to show that the reform doesn't affect the share of formal new hires transitioning from non-employment or informality. Second, I exploit variation on the geography of informality to show that effects aren't amplified in high informality areas. Taken together, these results are consistent with the fact that the informal sector in Brazil is predominantly characterized by self-employment, being prominently susceptible to corporate costs, regulations and legal liabilities (Maloney 2004).

While the payroll tax literature has been focused on employment and wages responses, I incorporate understudied margins of adjustment such as capital, profits and revenue. Interpreting the capital response is not straightforward, given that substitution and scale forces operate in opposite directions. Consistent with an optimal behavior of substituting towards 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 consumers. Importantly, while numerous previous studies extrapolate profit outcomes using structural assumptions (e.g., Suárez Serrato and Zidar 2016; Suarez Serrato and Zidar 2023), I directly observe the tax incidence borne by firm owners, revealing a 12% profit increase in response to the tax reform.

The positive earnings effect after a firm-level shock provides evidence of labor market power. However, there are many theoretical frameworks consistent with imperfect labor competition that would offer divergent explanations for the underlying mechanisms. Empirically, the wage pass-through for incumbent workers and new hires is indistinguishable, which is inconsistent with standard bargaining (Hall and Milgrom 2008; Pissarides 2009), models with imperfect substitutability between incumbents and new hires (Kline et al. 2019), and dynamic wage posting (Burdett and Mortensen 1998; Postel–Vinay and Robin 2002).

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 for incidence analysis: wage pass-through is determined not just by the slope of the labor supply and product demand curves, but also hinges on be-

havioral responses that guide 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 industrial policy empirical studies that document similar findings (Bronzini and Iachini 2014; Howell 2017; Zwick and Mahon 2017; Criscuolo et al. 2019).

To identify the model, I analytically derive closed form solutions that match structural parameters to the data, which I fit through a seemingly unrelated regression. This approach directly estimates the labor supply elasticity faced by the firm ($\epsilon = 4.15$), capital-labor elasticity of substitution ($\sigma_{KL} = 1.72$), and output demand elasticity ($\eta = 1.43$). 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 (Suárez Serrato and Zidar 2016; Card et al. 2018; Kroft et al. 2020; Lamadon, Mogstad, and Setzler 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 alludes to the presence of product market power, with an estimated markup leaning towards the upper range of existing estimates in the literature, but still between the values found in Haraszstosi and Lindner 2019, and Suárez Serrato and Zidar 2016.

Using the model, I show that 65% of the employment response is due to firms boosting their scale, and the remaining 35% due to capital-labor substitution. I further highlight that for smaller firms, the scale effect moves from 6% to 11%. The greater expansion is primarily a consequence of their limited market power, which permits them to expand more without exerting excessive pressure on prices. Additionally, a full-incidence analysis indicates that consumers pay 75% of payroll taxes, workers 14%, and firm owners 11%. I find a 44% efficiency gain associated with the payroll tax cut, which relates to the deadweight loss of taxation. The *"Marginal Value of Public Funds"* (MVPF) associated with Brazilian tax policy is 1.44, reflecting 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.

To illuminate the role of market power in driving tax incidence and distorting the efficiency costs of taxation, I propose an alternative policy that targets the payroll tax cut only to small firms. In this alternative scenario, the benefits to workers are amplified by 95%. This additional gain stems from greater ex-

pansion, 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 to the baseline. Given that small firm owners possess negligible power to secure rents, the alternative policy channels less welfare towards entrepreneurs. In terms of efficiency, the proposed policy generates a 36% welfare increase.

This paper elucidates the driving forces that dictate how market power shifts the incidence towards workers, away from consumers and firm owners. It emphasizes that tax cuts for firms possessing greater market power result in diminished efficiency gains, aligning with the notion that the taxation of economic rents induces fewer distortions. Echoing the principle of the "second-best", these insights suggest that distortions introduced by market power can indeed mitigate tax-induced distortions.

**Literature and Contributions.** The main contribution of this paper is to offer theoretical and empirical evidence on the role of market power in the incidence and efficiency costs of taxation. Concurrently, the study extends the payroll tax literature by incorporating consumers, who turn out to bear three quarters of payroll taxes in Brazil.[1] This paper contributes to fours strands of literature. First, it builds on a large body of work that finds mixed evidence for the effects of payroll tax cuts on employment and wages. Part of the literature claims that workers bear the incidence based on empirical analysis that shows zero employment, and positive wage effect (Gruber 1997; Anderson and Meyer 2000). At the other extreme, recent studies find positive employment and zero earnings response (Saez, Schoefer, and Seim 2019; Kugler, Kugler, and Prada 2017). A third strand of work is in between, reporting partial pass through to earnings (Hamermesh 1979; Holmlund 1983; Cruces, Galiani, and Kidyba 2010; Kugler and Kugler 2009; Saez, Matsaganis, and Tsakloglou 2012). Relatedly, recent studies found mixed employment effect of the "Paycheck Protection Program (PPP)".[2] This paper is able to reconcile the debate by adding a key element: market power.

From a theoretical perspective, this work contributes to the literature by delving into the underlying mechanisms that dictate how firms respond to taxes. It also establishes a framework to propose and quantify the impacts of welfare-

[1] Although the payroll tax literature hasn't explored the incidence to consumers yet, my estimate precisely aligns with the minimum wage literature (Harasztosi and Lindner 2019).

[2] This strand of work studies the COVID-19 relief plan, and rely on several interesting variations: discontinuity in firm size eligibility (Chetty et al. 2020; Autor et al. 2022), exposure to bank system (Granja et al. 2020; Li, Watson, and LaJoie 2020), and timing of loans (Autor et al. 2020; Faulkender, Jackman, and Miran 2020; Bartik et al. 2020; Doniger and Kay 2021).

improving alternative policies. Turning to the empirical side, the uniqueness of the Brazilian reform provides two noteworthy contributions. First, the policy is targeted at the firm, as opposed to the worker-level, mitigating pay equity confounding concerns (Dube, Giuliano, and Leonard 2019; Breza, Kaur, and Shamdasani 2018). Second, policy induced payroll tax variation is usually modest, in the US for example is less than 1p.p. (Guo 2023), whereas in Brazil is 20 p.p.

In the specific context of the Brazilian reform, there is important ongoing work, that employs aggregated sector data (Dallava 2014, Scherer 2015, Baumgartner, Corbi, and Narita 2022). I contribute to this strand of literature in several ways. This paper stands as the first to conduct analysis using anonymized firm-level tax data, enabling me to observe and adjust for all aspects of imperfect compliance, such as take-up and NCM eligibility. The data availability broadens the scope of my examination, moving beyond employment and wages to encompass capital, revenue, and profits, thereby supporting a pioneer full-incidence analysis of the Brazilian economy. In terms of heterogeneities, I break new ground by examining the consequences of this tax variation across diverse types of workers and firms. While existing studies restrict their sample to a limited share of sectors, I differ by including the vast majority of economic sectors in Brazil and extending the sample until recent years. I also contribute to this strand of literature, by developing a model that elucidates the mechanisms driving Brazilian firms' responses. The novel model allows me to leverage the tax variation to estimate parameters of interest, compute the MVPF of the Brazilian payroll tax system, and address counterfactual policies for the first time.

This paper documents clear evidence that Brazilian firms detain labor market power, which is in line with a burgeoning strand of frontier research (Card et al. 2018; Berger, Herkenhoff, and Mongey 2022; Lamadon, Mogstad, and Setzler 2022; Lagos 2019; Jäger and Heining 2022; Kline et al. 2019; Garin and Silvério 2019; Benmelech, Bergman, and Kim 2022; Burdett and Mortensen 1998; Azar, Berry, and Marinescu 2022) that has established firm's ability to markdown wages. Card et al. 2018 underscores the need for continued research on implications of labor market power on tax policy.[3] This is precisely the gap that this paper aims to fulfill. Berger, Herkenhoff, and Mongey 2022 uses Monte Carlo simulations to predict the welfare and output impact of corporate tax in concentrated labor markets. My work differs since it integrates product market power, and combines model with heterogeneous firm-level empirics. As argued

[3] "Finally, the idea that even highly advanced labor markets, like that of the United States, might be better characterized as imperfectly competitive opens a host of questions about the welfare implications of **industrial policies** (...)"

by Manning 2021, few papers[4] aim to directly estimate the labor supply curve faced by the firm. I contribute to this scarce literature by exploiting an unique quasi-experimental setting.

Concurrent work by Kroft et al. 2020 incorporates labor and product market power within a procurement setting in the construction sector. Their paper is focused on the role of "double market power" on wage markdown, price markup, and firms' decisions. Its consequences to tax pass-through is not immediately evident. This paper explores the interplay between market power and tax pass-through, revealing implications to the the deadweight loss and incidence of taxation. Besides including consumers to the setup, I also differ in terms of model assumptions, structural identification, and counterfactuals.

This paper also advances the literature estimating elasticities of substitution between capital and labor (Karabarbounis and Neiman 2014; Raval 2019; Doraszelski and Jaumandreu 2018; Chirinko, Fazzari, and Meyer 2011; Caballero et al. 1995; Oberfield and Raval 2021). In a meta-analysis, Gechert et al. 2022 criticizes 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 (Haraszstosi 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, measuring a 33% bias.

Finally, there is an important industrial policy literature that has studied Government subsidies for R&D (Bronzini and Iachini 2014; Howell 2017), equipments (Zwick and Mahon 2017) and investments (Criscuolo et al. 2019). This body of work has unanimously reported that subsidies are more effective in boosting employment in small businesses. While documenting the same pattern in Brazil, this paper posits that market power is a key ingredient to illuminate the mechanism underlying this empirical regularity.

**Structure.** 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. Section 4 develops the model. Section 5 estimates the model, discusses mechanisms, and examines alternative policies. Section 6 concludes.

[4]Existing evidence are primarily focused on public sector workers (Falch 2010; Staiger, Spetz, and Phibbs 2010), Uber drivers (Caldwell and Oehlsen 2022); and experimental settings (Dal Bo, Finan, and Rossi 2013; Dube et al. 2020; Belot, Kircher, and Muller 2019).

# 2 Institutional Background and Data

## 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 OECD's (refer to figure 25 for cross-country comparison). However, the Brazilian payroll tax cut program has a few unprecedented features that are advantageous from an empirical perspective. First, the targeting was at the firm-level. Second, the Brazilian reform offered a large tax reduction for benefited firms. Third, only a few firms were affected, isolating general equilibrium effects. Finally, the reform lasted for many years, which allows us to decompose short and long term effects.

**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 to 3%. The last layer of contribution is a 8 to 11% tax on wages, which is employee specific and can vary within workers of the same firm. All of these tax components are deposited in a social security fund that pools resources together. This implies that the public social security system does not provide individual savings accounts, where resources are traceable and mapped to specific workers' benefits.

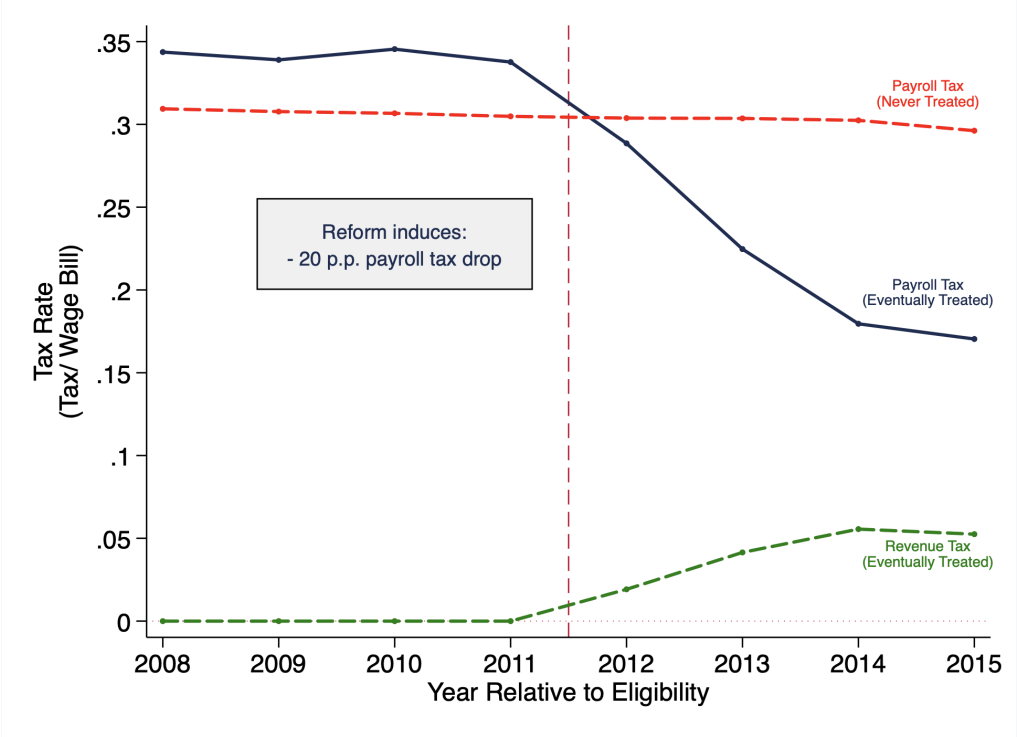
**Tax Variation.** On $14^{th}$ December, 2011 the Brazilian Congress enacted the payroll tax cut reform that waived the main component of the payroll taxation, to a small share of sectors and products.[5] Treated firms faced a uniform decrease in payroll tax rates, from 30 to 10 percentage points of the total wage bill, without any cap for high income earners. To provide slight compensation to the Government budget in face of this large drop in tax collection, the benefited firms were imposed to pay a small 1 to 2.5% taxes on net of exports gross revenue. Figure 1 compares the payroll and revenue tax variation. In the figure, taxes are divided by the total wage bill, so that all tax rates are comparable. It shows that the payroll tax drop is considerably larger than the revenue tax increase. This evidence showcases that the reform should be interpreted as a tax cut, as opposed to a tax substitution.[6] Importantly, the reform didn't affect individuals' perception on

[5] Less than 1.5% of formal firms in the country are affected by this reform.

[6] The graph reports averages, but even when we look at outliers in the labor share, only in 1% of cases it wouldn't be advantageous taking-up the benefit.

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

Figure 1: Tax Implication of the Reform




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

**Motivation.** The tax reform was motivated on the economic urge to increase 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. As part of these 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 rely on propensity scores to predict eligibility. Overall, the suggestive evidence indicates that the process to define eligible sectors was a complex political decision, but this did not compromise the identifying assumption. This research doesn't 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 tests trends and balance across eligibility status.

**Eligibility.** The policy established a sector and product specific eligibility criteria for the payroll tax exemption. Additionally, the payroll tax cut did not affect firms in the "Simples" tax tier, which is a regime for small firms. This alternative tax system was created in 1996 and had two main goals: simplify tax rules and reduce tax burden to small corporations. Given that said, firms in the "Simples" tax tier were not subjected to payroll taxes in the first place, and therefore were not affected by the payroll tax cut program.

**Timing.** The first tax bill outlining the policies and the eligible sectors were 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 nowadays, and there is no expectation of being eliminated in the near future. There were several other tax bills including more sectors to the reform in 2013 and 2014.[7] To understand the relevance of the reform from a macro perspective, table 7 presents the number of firms, workers and sectors affected by the reform, and its respective share relative to the Brazilian economy. Table 7 is also useful to characterize the staggered roll-out aspect of the program. It reports the cumulative values as all the cohorts (2012-2014) phase-in the program. At the peak of its implementation in 2014, the payroll tax cut has affected a relatively small share of Brazilian sectors (9%), firms (1.7%), and workers (6%).[8]

**Industry Variation.** Within broad defined industries, the reform did not provide eligibility to all sectors. For example, when the media industry became eligible, open television was contemplated while cable television was not. In the lodging industry, hotels are eligible, but motels are not. Similarly, table 6 provides multiple examples of similar subsectors in broad defined industries, where one of them became eligible and the other not. This finely detailed level of eligibility assignment contributes for identification in many obvious ways, and

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

[8] 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. Full list of eligible sectors can be found on table 23.

mitigates confounding concerns from concurrent policies. Other policies in place during the period, such as those under the umbrella of "Plano Brasil Maior", 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.

**Product Eligibility.** 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.[9] Treatment due to the NCM eligibility criterion only allows for partial payroll tax waive, according to the share of eligible products in the firms' gross income.

**Additional Changes.** Over the years, 5 other tax bills[10] were passed promoting marginal changes to the program, such as modifying the revenue tax rates, or adding new sectors to the policy. One of the most relevant changes happened in December 2015 when the policy became less generous as the revenue tax rates increased from 1-2.5% to 1.5-4.5%. At that moment, treatment assignment also became optional, which in practice was not a relevant change in the regime, since even in the early years of the reform take-up in eligible sectors was far from perfect. Indeed, the imperfect take-up rate is a central aspect of the reform that deserves more discussion.

**Take-up.** It may be puzzling that numerous eligible firms are not taking advantage of the generous Government benefits. Figure 12 shows that a substantial share of eligible firms do not take-up the benefit. To interpret this observational fact it is important to have in mind that the increase in revenue tax would surpass the payroll tax decrease for only 1% of eligible firms. Thus, the 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 to 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 don't opt in a supposedly beneficial tax system. Second, enrollment in the program was not automatic as in the Swedish case studied by Saez, Schoefer, and Seim 2019.

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

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

In Brazil, firms have to self-report eligibility on Government provided software to enable tax exemptions[11], through separate tax forms. Figure 13 illustrates tax forms instructions and the set of information requested in the tax platform. Even though the tax substitution implied a net tax cut in most cases, empirical findings in other countries (Kleven and Waseem 2013; Janet, David, John, et al. 2006; Zwick 2021; Moffitt 2007) suggest that the operational filling process can lead to non responsiveness even in dominated tax regions[12]. In line with this view, figure 14 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.

**Spillover.** Theoretical predictions on the consequences of tax variations, depend on whether the shock affects the market, or it is firm-specific. 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.[13] 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 27 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 22 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

[11] Firms inform eligibility on block 0, enabling block P where details are input.

[12] It relates to an extensive body of work dedicated to understand rational attention (Hoopes, Reck, and Slemrod 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, Looney, and Kroft 2009; Chetty, Friedman, and Saez 2013; Finkelstein 2009 )

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

shock, then we should expect to see a negative employment effect in these firms compared to other "Simples" firms in non-eligible sectors. Figure 15 shows that this is not the case. It reports a small and not statistically different than zero spillover effect. It also shows that "Simples" firms in eligible vs ineligible sectors follow similar trends in the pre-reform period. In contrast, it shows that among "non-Simples" firms, there are substantial effects of being in 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.[14] The reasoning is that spillovers affect worker's outside options, therefore generating more wage hikes. Figure 26 shows that the wage effect for workers in high versus low intensively treated markets are 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.

## 2.2 Data and Descriptive Statistics

I constructed two samples, one at the firm and other at the worker-level, by combining tax and labor administrative data on the universe of formal firms operating in Brazil between 2008 and 2017. The final dataset, which was anonymized, is arranged in a panel structure. This format enables the tracking of both firms and workers before and after the implementation of the tax reform. Below, I describe each data source separately.

**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 compilled 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, municipality, as well as hiring and termination dates. The main shortcoming is the lack of information about informal and non-employed workers (see Dix-Carneiro 2014 for an overview). 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

[14] 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.

5,300 Brazilian municipalities.

**Anonymized Tax Records.** To conduct a comprehensive analysis of the tax reform, we 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, capital, and profits[15]. The data structure is a panel at the firm-by-year level, ranging the years from 2008 to 2017. 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 firms tend to consolidate their tax planning efforts. It's worth noting that this should not impact the empirical analysis, as 95% of firms consist of a single establishment.

**Firm Level 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 period of analysis have not participated in the "Simples Nacional", which is a special tax tier not subjected to payroll taxes[16]. 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[17] 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.

While payroll taxes are administered at the federal level in Brazil, states are responsible for the value-added tax (VAT), known as ICMS. This tax is predom-

[15]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.

[16]Firms in the "Simples" regime face a different tax tier which consolidates all tax liability in a single tax form with simplified and lower rates.

[17]Industries are defined according to Classificação Nacional de Atividades Econômicas (CNAE), administered by the Brazilian Census Bureau (IBGE).

inantly concentrated in the retail industry, where over 85% of the tax collected stems from VAT (Naritomi 2019). During the period of analysis, states have engaged in multiple VAT tax reforms. These reforms included sector and product-specific exemptions, rate changes, as well as variations in withholding policies and auditing programs, recently studied. Unfortunately, I do not observe state-level tax data to control for all of these changes and potential confounding forces. For this reason, I exclude the retail industry from the sample. The main sample is not winsorized or balanced, but results are robust to these procedures (refer to appendix E).

**Worker Level 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, LaLonde, and Sullivan 1993; Lachowska, Mas, and Woodbury 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. This approach guarantees that results reflect stable employer-employee matches. This is important for two reasons. First, by minimizing the influence of turn over, it allows us to concentrate on the pass-through interpretation. Second, given that I assign workers to treatment based on the pre-reform period, emphasizing stable employer-employee relationships enables a more meaningful categorization into treatment groups.

**Descriptive Statistics.** Table 8 provides descriptive statistics for the worker-level 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 9 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 statis-

tics for treated firms within eligible sectors. As figure 14 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.

# 3 Empirical Analysis

The payroll tax cut causes a sharp expansion on 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 an event study instrumented by sector eligibility. The design explores the staggered implementation of the program, and the fact that there is a large share of firms never eligible or treated. The IV is necessary to adjust for two margins of imperfect compliance: the imperfect take-up in eligible sectors; and the treatment in non-eligible sectors due to the product eligibility criteria.

Concretely, the main specification combines the 2SLS framework with the event study set up, which provides two main advantages. First, it validates the identifying assumption by showing that the pre-reform coefficients of interest are not statistically different from zero. Second, it provides intuition about the dynamics of the treatment effect relative to the year prior to the policy implementation. This is particularly important as adjustment costs and inertia can potentially restrict immediate responses (Chetty et al. 2011).

**Firm level.** The first set of analysis rely 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 (e.g., education, gender, race, age and its square); $\xi_{I(j),t}$ is industry (broader than sector) interacted with year fixed effect; $\alpha_j$ 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,

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

where, $e_{s(j)}$ is the event date, in which firm j's sector becomes eligible; $L_{s(j)}$ indicates if firm j's sector is eventually eligible; and the remaining coefficients are the same as described before. Standard errors are conservatively clustered at the 5-digit industry-by-state level. Appendix A provides more details on the empirical model, underlying assumptions and reduced form equations. I also estimate an IV difference-in-differences model, where all the periods after the policy implementation are pooled to a single post period indicator. The first stage and structural equations are outlined in equations 3 and 4, respectively.

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

where, $D_{jt}$ indicates that firm j is treated in year t; $L_{s(j)t}$ indicates that firm j belongs to a sector that is eligible for treatment and that period t is after the starting eligibility date for sector s(j); $X_{jt}$ are set of controls (e.g., education, gender, race, age and its square); $\alpha_j$ is the firm fixed effect; $\xi_{I,t}$ is industry (broader than sector) interacted with year fixed effect. I cluster the standard errors at the level of the treatment variation (Bertrand, Duflo, and Mullainathan 2004 ; Cameron and Miller 2015). Because eligibility is defined at the sector level (mostly at the 7-digit), I conservatively cluster standard errors at the 5-digit industry-by-state level.

The first stage coefficient $\pi$ increases as the take-up rate on treated sectors increases, and deflates as there are more treatments occurring in non-treated sectors due to the NCM criteria. The associated reduced form is expressed in equation 4,

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

**Worker level.** The firm-level sample can impose challenges to evaluate the earnings effect. For example, one might be concerned that at the firm-level, the

average earnings can be affected by compositional changes in the labor force. To address this concern, I take advantage of the granularity of the micro data, to estimate a similar model at the worker-level. I assign workers' eligibility status ({0,1}) according to their pre-reform employer, and then evaluate individuals' outcomes regardless of the firms that they end up working for. Thus, $L_{s(i,t_0)}$ is equal to one if firm j's pre-reform sector eventually becomes eligible. Similarly, to the firm-level specification, the pooled difference-in-differences model at the worker–level is given by,

$$D_{it} = \pi L_{s(j_0)t} + \iota_i + \alpha_{j(i,t)} + \xi_{I(i,t_0),t} + X_{it} + u_{it} \tag{5}$$

$$Y_{it} = \delta L_{s(j_0)t} + \iota_i + \alpha_{j(i,t)} + \xi_{I(i,t_0),t} + X_{it} + u_{it} \tag{6}$$

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

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

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

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

**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.

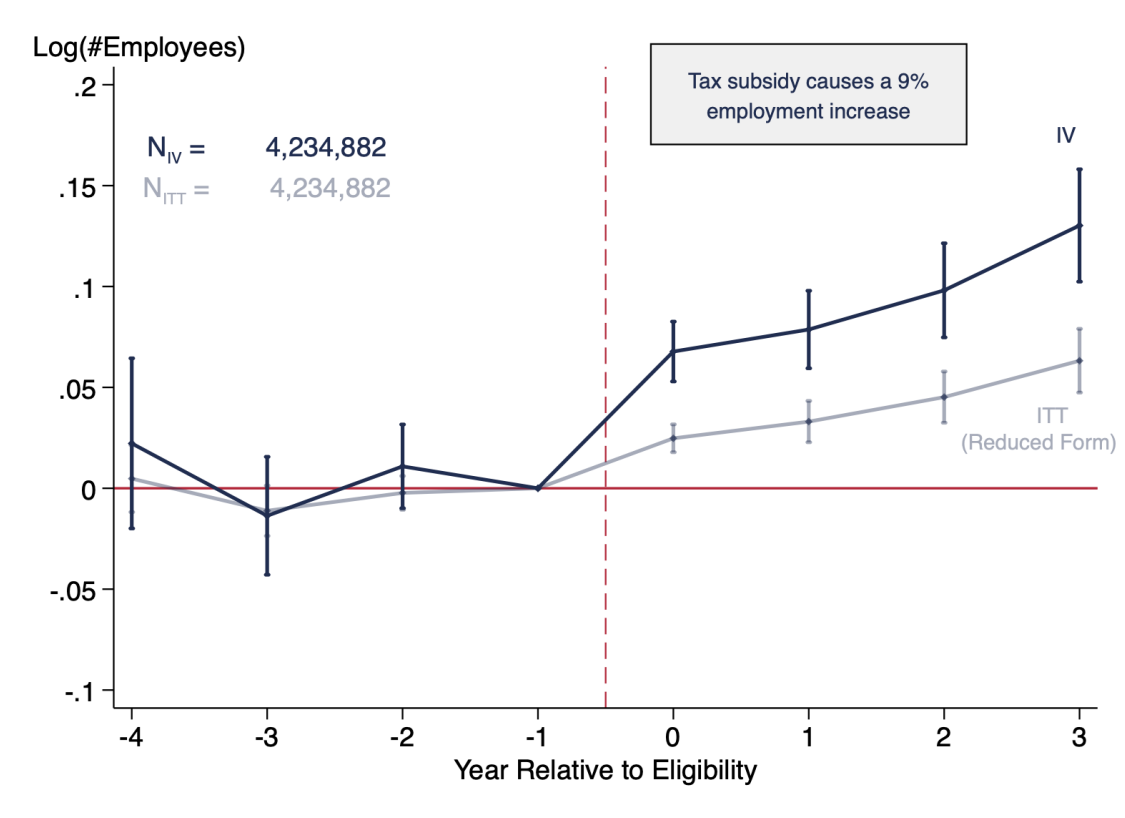
## 3.2 Labor Market Outcomes

In this section, I report causal effects of the payroll tax reform on a comprehensive set of labor market outcomes. The findings indicate that employment and wages rise when the payroll tax is reduced. However, it takes time for workers' earnings to respond. These results are confirmed in several analysis, coupled with novel heterogeneity findings at both the firm and worker-level.

### 3.2.1 Firm-level Analysis

The difference-in-difference estimation, from equations 3 and 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 (refer to appendix E), 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[18]. Figure 17 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 eliminates the *"mean reversal"* channel that could potentially explain the heterogenous

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

pattern. Important to mention that this heterogeneity is not mechanically driven by differences in the first stage. Figure 28 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+$\tau$)** | **Log(#Employees)** | **Log(#Employees)** | | |
|---|---|---|---|---|---|
| | All Sample (1) | All Sample (2) | Small (3) | Medium (4) | Large (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 the 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 had less than 9 workers in the pre-period. Medium if they had 10-49, and large if they had more than 50 workers. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

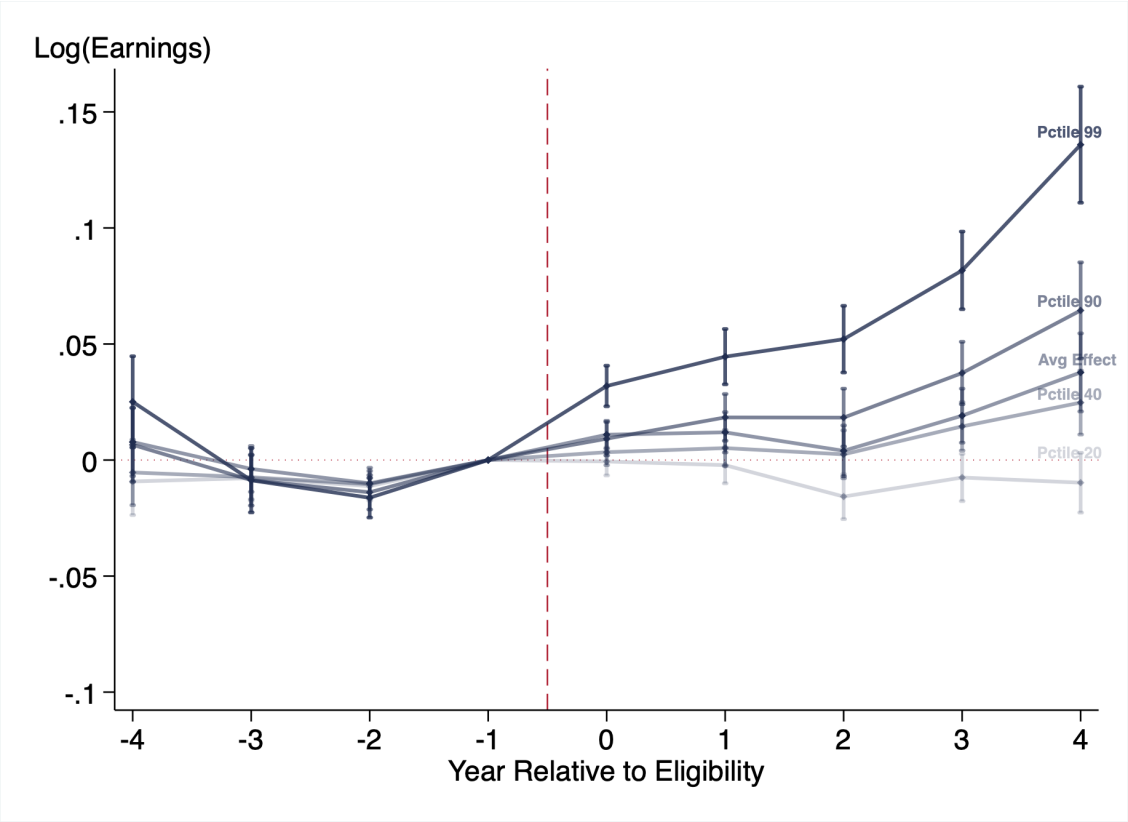
**Liquidity constraints.** The observed heterogeneity in firm size is consistent with previous studies (Zwick and Mahon 2017; Criscuolo et al. 2019; Howell

2017; Bronzini and Iachini 2014) which found that tax subsidies yield greater employment responses by small firms in different contexts. Due to lack of data and appropriate variation, the literature hasn't 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 12. 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.

**Earnings.** Several key 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 on 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 raises up to more than 4% at the firm's $99^{th}$ percentile. The absence of pre-trends across the entire range of the earnings distribution further corroborates these findings. These results shed light to 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.

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.

**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 2 demonstrates that the tax reform does not significantly impact the composition of employed workers across various dimensions. The only exception is gender, where the reform induces a marginal but statistically significant effect of a 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 is 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 2 suggest 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 occupation data, which contains 2,300 occupation codes. These occupations are ranked based on pre-reform average earnings and grouped into percentiles. Then, for each firm, I calculate the average occupation percentile they employ from. Table 11 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 result reinforces the idea that the reform didn't change the composition of the employed workforce. Another interpretation is that the reform does not induce a structural change in technology, and consequently, in the type of labor demanded.

Table 2: Effect on Labor Composition

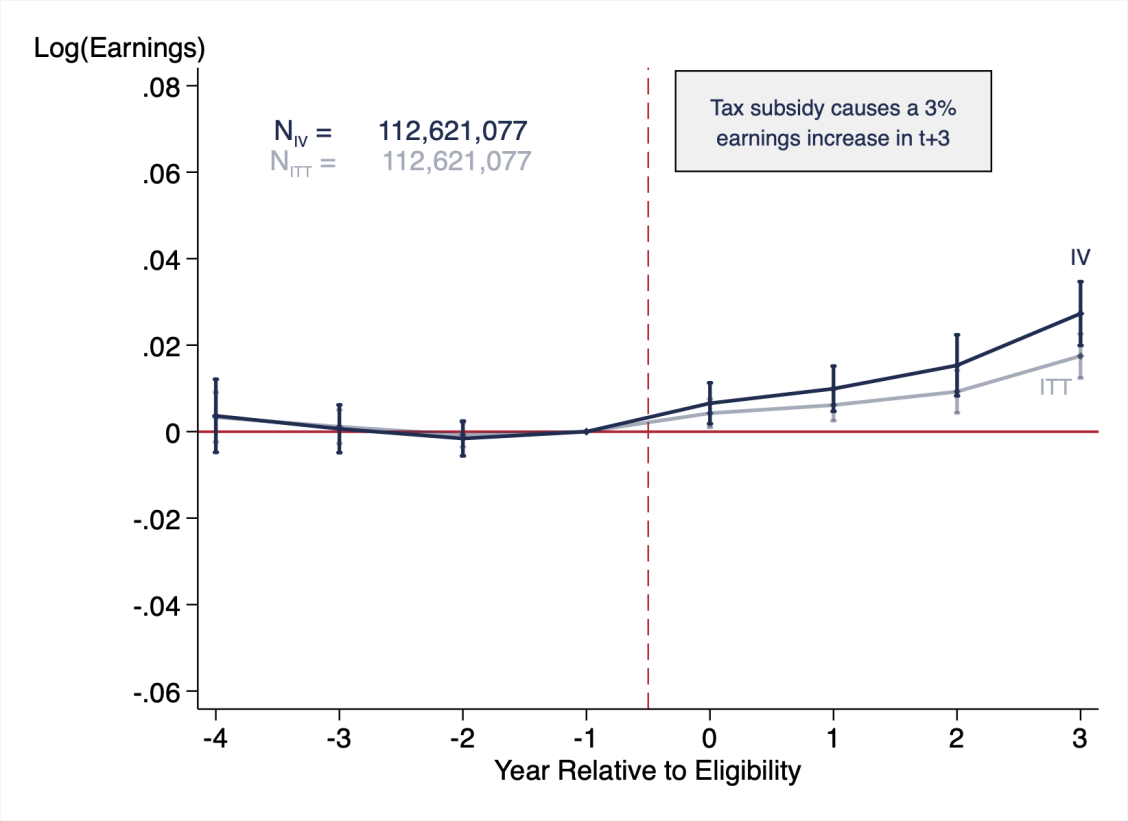
| | (1) Share High School + | (2) Share College + | (3) Share Male | (4) Share White | (5) Avg Worker's Age | (6) Log Earnings New Hires (bf hired) | (7) Log Quality |
|---|---|---|---|---|---|---|---|
| Post × Treatment | .0091 | .0099 | .0132*** | .0005 | -.1343 | -.0014 | -.0005 |
| | (.0085) | (.0061) | (.0034) | (.0045) | (.1497) | (.0116) | (.005) |
| Mean | .52 | .11 | .59 | .67 | 39.72 | 7 | 1 |
| 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 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 parenthesis.

### 3.2.2 Worker-level Analysis

An alternative approach to evaluate the pass-through to workers' earnings involves tracking workers over time. This strategy offers two main advantages. First, it provides further validation that the firm-level earnings results were not influenced by shifts in labor force composition. Second, it yields insights into how tax variation impacts workers' career paths, particularly among various types of workers. Important to clarify that throughout this analysis our focus is on the net earnings received by workers, after the deduction of payroll taxes. As shown in Figure 18, the reform causes a sharp decrease in gross earnings paid by firms, mostly due to the mechanical reduction on payroll tax rates. However, figure 4 demonstrates that net earnings pocketed by workers (after payroll taxes have been deducted) actually increase by an average of 1.8%. This effect intensifies to a 3% increase three years following the tax cut.

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 prior to the treatment event. The analysis spans four years prior to 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 at high skill and managing positions benefit relatively more from the reform. To implement this analysis, I rely on the CBO occupational code[19] 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 19 shows that the pass-through to high skilled workers is 6%, and it is almost zero to low skilled workers. In the appendix, I provide 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 suggests that the lower skill labor market operates closer to perfect competition, as a commodity type of labor.

**Minimum wage.** One might wonder if the small earnings effect presented in figure 4 is driven by the minimum wage constraint. To address this question, I classify workers into the minimum wage categories based on the modal pre-reform minimum wage status. On average 20% of workers in the sample are constrained by the minimum wage. Figure 29 confirms that results are not driven by a muted response of minimum wage workers. If anything, these workers present an event greater response, which can be rationalized by minimum wage being likely to bind even before the reform. Therefore, the labor demand expansion generates earnings response above the minimum constraint.

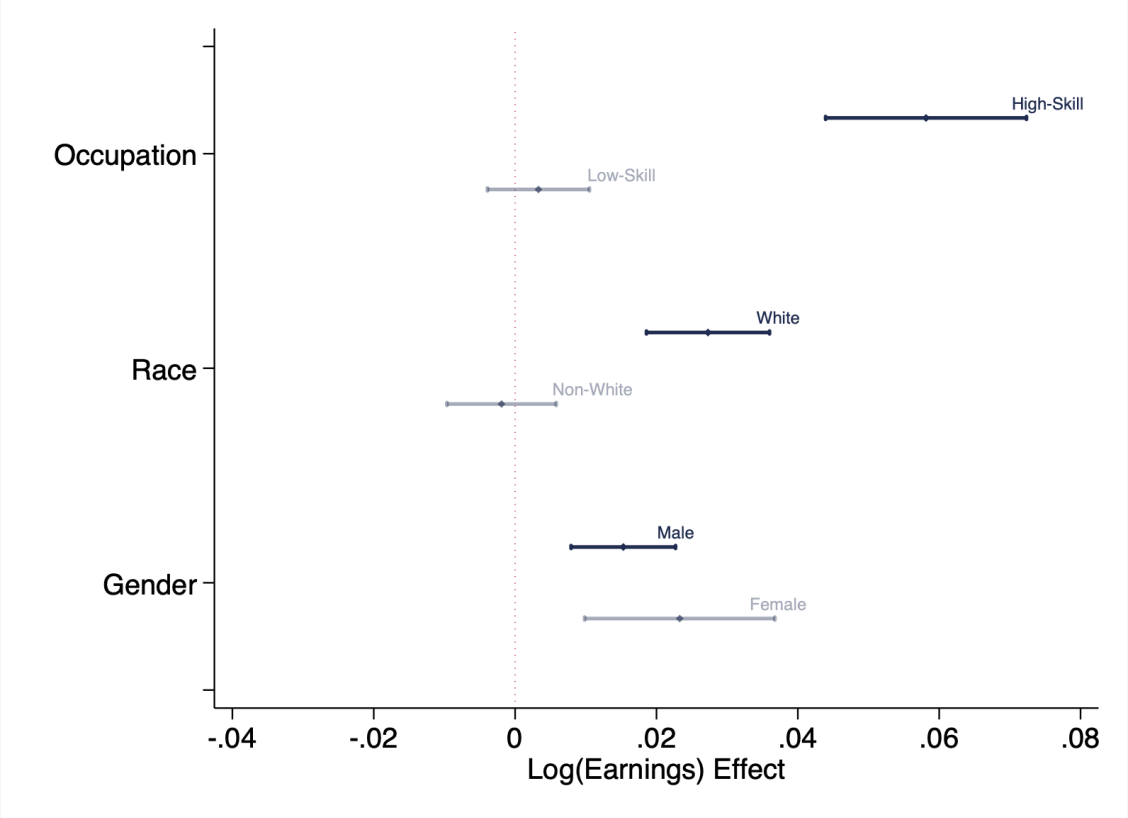
**Racial wage gap.** The payroll tax program does not distinguish workers based on background characteristics, such as race. It offers a flat 20 percentage point cut that remains constant across all income levels, suggesting no explicit intention to disadvantage workers of a specific race. However, it doesn't mean that existing racial wage differences were not exacerbated. As discussed throughout this paper, equal tax cuts can lead to a higher pass-through to high-wage occupations. 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 is estimated using regressions 5 and 6, which include firm fixed effects. Therefore, the expansion of the racial gap should not be attributed to firm sorting. I also evaluate heterogeneous pass-through according

[19]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.

to gender, but found zero statistical difference. Figure 5 summarizes the analysis across different types of workers.

Figure 5: Heterogeneities by Type of Worker




*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.

**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[20]. 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, which calls for action in future tax policy.

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

## 3.3 Validity of Results

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 8 and 9. 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:

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

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 24 reports 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.

**Anecdotal evidence.** The striking similarities in trends and levels across eligibility groups are not surprising. The political process that assigned eligibility

was mostly based on the finest sector definition (7 digits), and placed plausibly similar sectors on opposite sides of the eligibility status. For instance, the sector of trains were eligible, but touristic trains were not. The sector of open television is eligible, but cable television is not. Table 6 provides a non-exhaustive list of numerous additional examples, further illustrating this point.

**Alternative Identification.** The reason why specific sectors were chosen was not disclosure, nor was there an objective criteria to determine eligibility. From an econometric stand point 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 E.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. A detailed analysis and the corresponding results are provided in Appendix E.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 towards 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 towards eligible sectors. This low manip-

ulation response aligns with the bureaucratic challenges of changing sectors.

**Bureaucratic process.** Firms in the regular tax tiers (object of this study) face a long and costly process to change sectors. In order 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.

**Extra 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 as much intended to provide further validation of the empirical design, but aimed at exploring the economic interpretations of the results. Given the identified causal employment response to the tax cut, it's 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 from 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 30 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 prior to 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 31 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 13). 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.** As Ulyssea 2018 notes, informal employment is concentrated in firms with lower average education. The labor data provides information on workers' educational level, allowing us to observe 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 13).

**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[21] (see Perry 2007 for discussion).

[21] Alcaraz, Chiquiar, and Salcedo 2015; Dalberto and Cirino 2018

## 3.4 Other Margins of Adjustment

We have empirically established that upon a sizable tax cut, firms present substantial employment growth and moderate increases to workers' earnings. However, these two response dimensions alone don't provide a comprehensive understanding of the tax incidence. For instance, it isn't immediately clear whether the increase in employment arises from scale or substitution. These alternative channels influence prices and revenue differently, and thus have disparate impacts on the incidence to consumers. Furthermore, drawing conclusions on the incidence to firm owners, which requires knowledge of the causal effect on profits, is not feasible based on employment and earnings effects alone.

To measure 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[22] are obligated to file those information. Table 3 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 20 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 towards 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 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.

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

Relying solely on employment responses requires the use of structural assumptions to map input choices to profit outcomes, an approach often employed in tax incidence literature (e.g., Suárez Serrato and Zidar 2016; Suarez Serrato and Zidar 2023). The comprehensive nature of the administrative data used in this study enables a clean empirical approach to document the tax incidence to firm owners (column 6). Given that profits can plausibly be negative or zero, I opted not to rely on logs for this specific outcome. Instead, I leverage the inverse hyperbolic sine transformation, which accommodates all observations. Results are further corroborated using the level counterparts expressed in Figure 20.

Table 3: Firms' Margins of Adjustment

| | (1) Log Labor Cost $(1+\tau)$ | (2) Log Employment | (3) Log Capital | (4) Log Earnings | (5) Log Revenue | (6) Asinh Ebit |
|---|---|---|---|---|---|---|
| **Panel A:** Diff-in-Diff | | | | | | |
| Baseline | -.1321*** | .1019*** | -.0967*** | .0251*** | -.073*** | .0399 |
| | (.0032) | (.0206) | (.0335) | (.0063) | (.0232) | (.0417) |
| Small Firms | -.1352*** | .2691*** | -.0858 | .0563*** | -.0216 | -.0376 |
| | (.0067) | (.0366) | (.0784) | (.016) | (.0516) | (.0448) |
| Large Firms | -.1308*** | .0573** | -.0953*** | .0152** | -.0831*** | .0723 |
| | (.0038) | (.0239) | (.0361) | (.0059) | (.0251) | (.057) |
| **Panel B:** Long-Diff (t + 3) | | | | | | |
| Baseline | -.1247*** | .1279*** | -.0337 | .0309*** | .0495 | .1223* |
| | (.0059) | (.0311) | (.0496) | (.0094) | (.0371) | (.0669) |
| Small Firms | -.1275*** | .3461*** | .0085 | .0602** | .1864** | .0035 |
| | (.0224) | (.0587) | (.1133) | (.0252) | (.0875) | (.0788) |
| Large Firms | -.1253*** | .0969*** | -.0312 | .0228*** | .0223 | .1992** |
| | (.0047) | (.0352) | (.0549) | (.0086) | (.0391) | (.0898) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Sector x Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 449, 679 | 450, 387 | 345, 217 | 450, 387 | 374, 774 | 377, 619 |

*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, gross revenue, and profits. Results are presented for the baseline sample, and separately per firm size, which is defined with respect to the median in the pre-reform period. 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 parenthesis.

## 3.5 Alternative Models of Labor Market Power

The positive earnings pass-through as a response to a firm-specific shock motivates the imperfectly competitive labor market. However, rejecting competitive markets doesn't necessarily imply that the wage setting process follows a standard monopsony model as in Manning 2011; Card et al. 2018; Kroft et al. 2020. In fact, models of bargain (Hall and Milgrom 2008; Pissarides 2009; Jarosch, Nimczik, and Sorkin 2019), dynamic wage posting (Burdett and Mortensen 1998; Postel–Vinay and Robin 2002), and static wage posting with recruiting costs (Kline et al. 2019; Manning 2006) also predict positive earnings effect, but they rely on different forces, which in turn, imply distinct policy implications. For example, while in the standard monopsony, the wage pass-through is driven by the employment expansion, in the bargain models, the underlying force is the surplus of the employee-employer match. Prior work, have faced identification challenges to disentangle these two forces.

Important to highlight that this paper doesn't have the ambition to discover an universal formula to explain how wages are set. There are several particularities on the way wages are negotiated in the real world. However, this subsection evaluates testable implications for each class of models, in order to find the most suitable framework to interpret the consequences of tax policy. I discuss and falsify potential candidates, and provide supporting empirical evidence to the standard monopsony model.

**Bargaining.** In models such as those proposed by Hall and Milgrom 2008, Pissarides 2009, and Jarosch, Nimczik, and Sorkin 2019, wages are determined by the split of the surplus from the employer-employee match. Seen through this lens, the surplus generated by the Brazilian payroll tax reform exerts additional upward pressure on wages. Although it is not feasible to empirically disentangle the extent to which wage pass-through is attributable to surplus sharing versus employment growth, other empirical predictions can be tested. For instance, the pass-through argument via bargain operates during wage (re)negotiations. Given that such renegotiations occur less frequently for existing employees compared to new hires, the bargaining model would predict a higher wage pass-through for new hires relative to incumbents. However, this hypothesis is empirically rejected, as reflected in figure 32.

**Dynamic Wage Posting.** A popular framework to motivate firms' monopsonistic power hinges on search frictions, where firms are able to poach and negotiate over workers in other companies (Burdett and Mortensen 1998; Postel–Vinay

and Robin 2002). In this model, high-productivity firms grow larger as they can attract and retain more workers, offering employees an increased likelihood of receiving matching counter offers when presented with outside opportunities. Consequently, larger (more productive) firms tend to hire more via poaching, whereas small firms rely on hiring from non-employment. The Brazilian tax reform, by reducing labor costs, increases the surplus of the employment match, leading to several testable predictions. Large firms engaging in poaching might display higher wage pass-through as the firm they are poaching from, with some probability[23], have also experienced a tax cut and will bid back to retain the worker. This argument is formalized in Biro et al. 2022. Through the lenses of this model, if anything, larger firms should present greater wage pass-through relative to small ones. I find substantially greater effects in small firms (figure 16), refuting predictions from dynamic wage posting models.

Under the logic of this model, less productive firms tend to benefit disproportionately more from the tax cut as they are more likely to hire from non-employment. This is inconsistent with the empirically estimated employment and profit effects to small and large firms. Another implication is that, if anything, incumbent workers should experience greater earnings pass-through relative to new hires. The reasoning is that the outside option of the prospective new hire does not improve, when a small share of firms in the local labor market is shocked with a labor cost reduction. Conversely, incumbent employees should experience higher wage pass-through, as the tax benefit boosts their current firm's ability to counter bid when confronted with an outside offer. However, figure 32 shows that there is not a statistically significant difference between incumbent and new hire's wage effect, serving as an additional piece of evidence against dynamic wage posting.

**Recruiting Cost.** Several papers have found empirical support for wage posting models with recruiting cost, in the context of technological firms in the US (Kline et al. 2019), and survey responses from large UK companies (Manning 2006). Applying these models to interpret the Brazilian tax reform, generates the prediction that incumbent workers would have a rent premium because they are expensive to be replaced. In Kline et al. 2019, the marginal cost of new hires is $c'(\frac{L_j}{I_j}) + w_j^m$, where $c(\cdot)$ is a convex cost function and $w_j^m$ is the market wage. Figure 32 tests the main prediction from this model, and rejects that incumbent workers experience a wage premium pass-through relative to incumbents.

[23] In the Brazilian case, this probability is small, because a small share of firms are affected. Even if this probability is zero the test still holds, as small (less productive firms) should not display greater earnings pass-through in this model.

**Standard Monopsony.** In its original form, wage posting models don't allow firms to negotiate wages differently with new hires and incumbent workers. For this reason, there should be no pass-through wedge across job tenure, which aligns with the empirical findings in this paper. This model is further supported by the fact that firms experiencing a more pronounced employment effect also share more rents with incumbent workers, in the form of earnings pass-through. In order to fully rationalize the firm-level heterogeneities (as presented in Figure 16), one must take into consideration the potential for imperfect competition in the goods market, as well. This influences the incidence on the marginal revenue product of labor (MRPL) through the scale effect. When integrated, these considerations help to rationalize all the empirical findings from the Brazilian tax reform, bringing together two standard methods of modeling frictions in both the labor and product markets.

# 4 Model

In this section, I develop a simple model of factor demand with imperfect competition in the goods and labor market, intended to shed light on the role of market power to drive the efficiency and distribution of tax reforms. The labor market power is tailored to capture the empirical fact of positive wages pass-through after an idiosyncratic shock to the firm. Additionally, the combination of model and data allows me to estimate structural parameters, such as wage markdown, capital-labor elasticity of substitution, and demand elasticity. Those are key elements to shed light on important mechanisms of adjustment to the tax policy, which inform policy makers on the design of alternative policies.

## 4.1 Preliminaries

The model has a single period, and firms operate in a partial equilibrium framework[24]. 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, $s_g$ are the inputs' cost share ($g \in \{L,K\}$). The capital market operates in perfect competition, which means that the marginal productivity of capital

[24]This is motivated by the nature of the tax reform, which impacts individual firms but not the market as a whole.

equals its cost. However, the labor market operates in imperfect competition, and the labor supply elasticity $\epsilon$ 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, as in standard monopsony models (Card et al. 2018).

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

The wage-setting rule suggests that if wages[25] rise due to firm-specific shocks, both incumbents and new hires benefit equally - 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 B, and the labor supply curve faced by the firm is derived according to workers' idiosyncratic preferences.

The output market operates in monopolistic competition, where firms choose quantity and face a constant price elasticity $\eta$ (Hamermesh 1996; Criscuolo et al. 2019). The degree of monopolistic power is dictated by the parameter $\eta$, 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 cost of labor is, $C_L = (1+\tau)wL$, where, $\tau$ is the average payroll tax rate. The government can manipulate labor cost through $\tau$-perturbations, with the percentage variation on labor cost induced by the Brazilian policy denoted by $\phi_1$. In appendix D, I allow perturbations in the capital tax to study the role of monopsony power on the incidence of capital taxation.

## 4.2 Firm's Problems

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

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

[25] The subscript j indexes a specific firm, but for ease of notation this subscript will be omitted in the rest of the paper.

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

$$\underbrace{\left(\frac{\eta-1}{\eta}\right)Q^{\frac{-1}{\eta}}}_{\text{Mg Revenue}} = \underbrace{\frac{\partial C(\tau,Q)}{\partial Q}}_{\text{Mg Cost}} \tag{9}$$

In contrast to a perfectly competitive environment, the marginal cost no longer operates as a linear function of the output level (see proof of lemma 1, in appendix B). The intuition is that there is an increasing cost to expand plant size due to inframarginal wages. As a result, the imperfect labor competition limits the pass-through to employment. Mathematically, equation 9 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 $\eta$). 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 optimal, 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 decreases 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:

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

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 ($\epsilon \to \infty$), similarly to traditional models (Hamermesh 1996). The nonlinearity in the cost function will be key to understanding the pass-through responses to payroll tax reforms.

## 4.3 Pass-Through

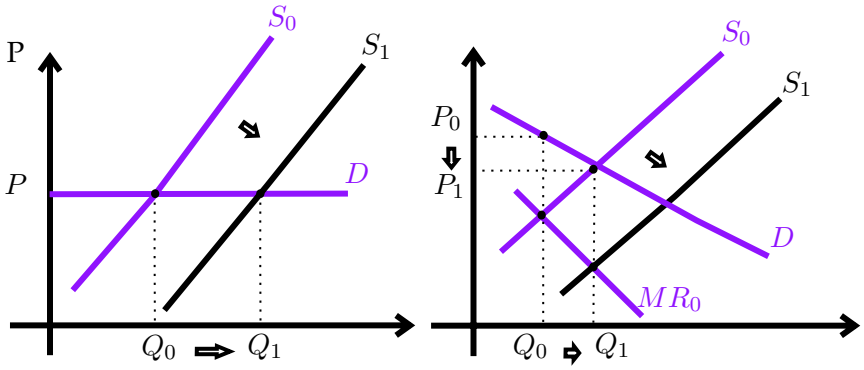
The pass-through of the payroll tax reform operates through two mechanisms. The output market, which determines the scale effect. And the input mix, which dictates the substitution across production inputs.

### 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 6 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 6: Conceptual Framework - Product Market




*Note:* This figure illustrates the pass-through in the product market from a firm-specific payroll tax cut. The left graph shows the intuition for the case of perfect competition in the output market. In this case, cost reduction shifts supply of goods, but doesn't affect prices as firms are price takers. On the right, the graph depicts the intuition for the monopolistic case. Compared to the perfectly competitive case, there is a smaller quantity (or scale) effect due to the price setting power.

The price elasticity depends not only on the market power, which is determined by the constant elasticity $\eta$, but also on the scale effect. Because of monopsony power, the scale effect cannot be evaluated solely based on the product market. Remember that now, the inframarginal rent affects the plant size decision. At the optimum, the effects of tax policy on marginal revenue should equate the effects on marginal cost:

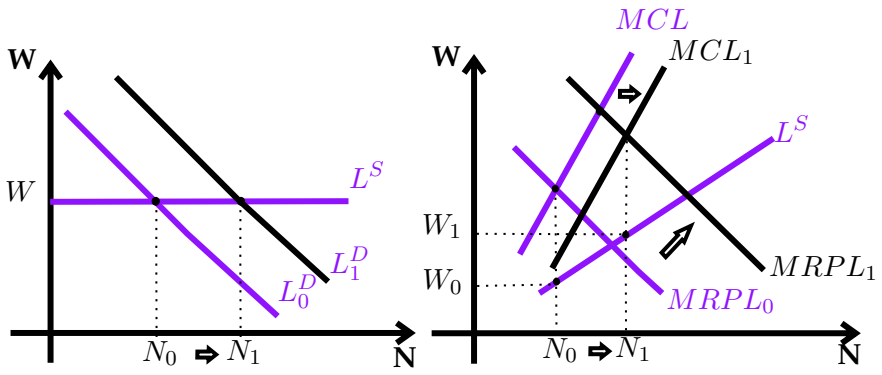
$$\underbrace{-\frac{1}{\eta}\epsilon^{Q}_{1+\tau}}_{\substack{\text{Effect on mg revenue} \\ (\eta \text{ drives slope of D})}} = \underbrace{\overbrace{\epsilon^{\lambda}_{1+\tau}}^{\text{Direct}} + \overbrace{\epsilon^{\lambda}_{Q}\epsilon^{Q}_{1+\tau}}^{\text{Inframarginal}}}_{\substack{\text{Effect on mg cost} \\ \text{(shift in supply curve)}}} \tag{10}$$

Equation 10 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 10 are endogenous to the tax system. Appendix B further develops this formula to establish a closed-form solution for the pass-through as a function of primitives, articulated in equations 14-17.

### 4.3.2 Labor Market

Labor market forces at work that determine tax pass-through are reflected on effects to the marginal cost of labor and marginal revenue product of labor. Figure 7 provides intuition on the interaction between imperfect labor competition and a firm-specific shock.

Figure 7: Conceptual Framework - Labor Market




*Note:* This figure illustrates the pass-through in the labor market from a firm-specific payroll tax cut. The left graph shows the intuition for the case of perfect labor market competition. In this case, payroll tax cut shifts right the demand for labor, but doesn't affect wages since firms have no wage setting power. On the right, the graph depicts the intuition for the monopsonistic case. In this case, the employment effect is not as large, but as the tax reform expands labor demand, it provokes a wage increase.

Equations 11 and 12 quantify the elasticity of the marginal cost of labor, and elasticity of marginal revenue product of labor with respect to labor cost, in the case of monopsonistic labor markets.

$$\frac{\partial \log MCL}{\partial \log(1+\tau)} = \underbrace{1}_{\text{Direct effect on MCL}} + \overbrace{\frac{\epsilon^L_{1+\tau}}{\epsilon}}^{\text{Inframarginal on MCL}} \tag{11}$$

$$\frac{\partial \log MRPL}{\partial \log(1+\tau)} = \underbrace{\epsilon^{\lambda}_{1+\tau} + \epsilon^{\lambda}_{Q}\epsilon^{Q}_{1+\tau}}_{\text{Direct + inframarginal on mg rev}} + \overbrace{(1-\rho)(\epsilon^{Q}_{1+\tau} - \epsilon^{L}_{1+\tau})}^{\text{Effect on MPL}} \tag{12}$$

**Marginal Cost of Labor** The pass-through to the marginal cost of labor (MCL) comprises 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 due to the upward slope of their labor supply curve. This component elevates the MCL through two channels: (i) the responsiveness of labor demand (behavioral forces), which causes the firm to move further along the labor supply curve, and (ii) the firm's labor market power, which is reflected in the steepness of the labor supply curve.

**Marginal Revenue Product of Labor** As equation 12 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 to 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 on 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 13 quantifies these mechanisms by relying on envelope arguments.

$$\overbrace{\frac{\partial \lambda(Q,\tau)}{\partial(1+\tau)}}^{\text{Effect on mg cost}} = \overbrace{\frac{AL^{1+\frac{1}{\epsilon}}}{Q}}^{\text{Effect on avg cost}} + \overbrace{\frac{AL^{1+\frac{1}{\epsilon}}}{Q\epsilon}}^{\text{Direct effect on incumbent rent}} + \overbrace{\frac{A(1+\tau)}{\epsilon}\left(\frac{\epsilon+1}{\epsilon}\right)\frac{L^{\frac{1}{\epsilon}}}{Q}\frac{\partial L}{\partial(1+\tau)}}^{\text{Indirect effect on incumbent rent from L response}} \tag{13}$$

The interaction between labor market power, and pass-through to the marginal cost is unambiguous. The higher the market power, the higher is 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.

# 5 Structural Estimation

This section connects model and data. A direct structural identification allows me to credibly estimate parameters of interest, understand mechanisms, and design counterfactual policies. To operationalize the structural estimation, I start by deriving the identifying equations.

## 5.1 Identification and Interpretation

**Pass-through formulae.** Following the derivation outlined in section 4 (and detailed in appendix B) 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[26]. Once we account, for product and labor market power, the pass-through of the Brazilian tax reform to employment, capital, earnings and revenue can be expressed as a function of observables and three parameters to be estimated $(\epsilon, \eta, \rho)$:

$$\beta_L = \left(\frac{\epsilon}{1+\epsilon(1-\rho)}\right)\left(\Omega(\epsilon,\eta,\rho)-1\right)\phi_1 \tag{14}$$

$$\beta_K = \psi(\epsilon)\left[\left(\frac{1}{1-\rho}\right)\left(\frac{(\epsilon+2\epsilon^L_{1+\tau})(1-\eta)}{\epsilon+\epsilon^L_{1+\tau}}\right)\right]\phi_1 \tag{15}$$

$$\beta_{Rev} = \psi(\epsilon)\left[(1-\eta)\left(\frac{\epsilon+2\epsilon^L_{1+\tau}}{\epsilon+\epsilon^L_{1+\tau}}\right)\right]\phi_1 \tag{16}$$

$$\beta_W = \frac{\epsilon^L_{1+\tau}}{\epsilon}\phi_1 \tag{17}$$

where, $\Omega(\epsilon,\eta,\rho)$ and $\psi(\epsilon)$ simplify a combination of terms detailed in the Appendix B; $\phi_1$ measures the first stage associated with the policy, i.e., the percentage variation on tax rates induced by the reform. Mathematically, $\phi_1 = \frac{\partial \log(1+\tau)}{\partial Reform}$. Using anonymized tax data, I precisely estimate $\phi_1$.

The incidence model developed here is more general compared to the one in previous studies that assumed perfect labor market competition. In my framework, one can think of the perfectly competitive labor market as a particular case, where $\epsilon$ goes to infinity[27]. Taking the limit of pass-through expressions 14-17, I recover the exact same expressions derived in a standard Marshall-Hicks analysis and estimated by Curtis et al. 2021; Criscuolo et al. 2019; and Harasztosi and Lindner 2019. In the standard competitive case, substitution and scale effects are separable, as illustrated in the expressions below.

[26] 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 B.3.

[27] This limit case moves the economy to a perfectly competitive labor market, where the labor supply faced by the firm is perfectly elastic.

$$\lim_{\epsilon \to \infty} \beta_L = \left( \underbrace{\frac{-s_K}{1-\rho}}_{substitution} - \underbrace{s_L \eta}_{scale} \right) \phi_1 \qquad \lim_{\epsilon \to \infty} \beta_K = \left( \underbrace{\frac{s_L}{1-\rho}}_{substitution} - \underbrace{s_L \eta}_{scale} \right) \phi_1$$

**Estimation Method.** To operationalize the structural estimation, notice that the pass-through expressions can be solved for the structural parameters as a function of the reduced form estimates. Equation 17 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.

$$\epsilon = \frac{\beta_L}{\beta_w} \tag{18}$$

From the capital and labor responses, $\sigma_{KL}$ is identified:

$$\sigma = \frac{\beta_K - \beta_L}{\beta_W + \phi_1} \tag{19}$$

The intuition is that $\sigma$ is estimated from the tension between capital and labor substitution. Equation 19 depicts the intuition that as $\beta_K$ decreases relative to $\beta_L$, it is an indication that firms are substituting capital for labor. Also interesting to notice that when $\beta_W$ goes to zero, $\sigma$ boils down to the standard expression from previous studies that assumed perfect labor market competition. Another angle to read equation 19 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:

$$\eta = \frac{\sigma \beta_R - \beta_K}{\beta_R - \beta_K} = \frac{-\beta_Q}{\beta_P} \tag{20}$$

The economics behind equation 20 is that $\eta$ 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. The point estimates for each parameter are directly estimated by the combination of reduced form effects outlined in equations 18-20. In the data, I jointly estimate equations 14-17 in order to obtain a covariance matrix, and therefore use the so called, *"Delta method"* to estimate standard errors for each structural parameter.

**Parameters estimate.** 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[28]. The labor-capital elasticity of substitution ($\sigma_{KL} = \frac{1}{1-\rho}$) is equal to 1.72 (SE 0.57) at the baseline. This result is similar to Karabarbounis and Neiman 2014 and imply that capital and labor are substitutes[29], supporting the view that lowering the cost of capital may increase income inequality (Piketty and Zucman 2014). Interestingly, I find that capital and labor are more substitutes in small firms (5.01, SE 2.95), as opposed to in large firms (1.25, SE 0.56). This result is valuable because most of the literature dedicated to capital-labor elasicities 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.

The labor supply elasticity faced by the firm $\epsilon$ is 4.15 (SE 1.63), which is more precisely estimated, but remarkably close to the 4.19 (SE 4.79) estimated by Suárez Serrato and Zidar 2016. Many other studies find similar labor supply elasticities in the order of 4.08 (Kroft et al. 2020), 4.0 (Card et al. 2018), 4.6 (Lamadon, Mogstad, and Setzler 2022). I also don't reject the 2.88 estimate that Lagos 2019 found for Brazilian firms. Figure 8 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 Brazilian workers leave on the table 19% of their marginal revenue product of labor. Columns (2) and (3) report that the labor supply elasticity for small and large firms are 5.75 (SE 2.65) and 4.25 (SE 2.23), respectively. This result is strikingly consistent with the increasing and monotonic relationship between labor market power and firm size demonstrated by Yeh, Macaluso, and Hershbein 2022.

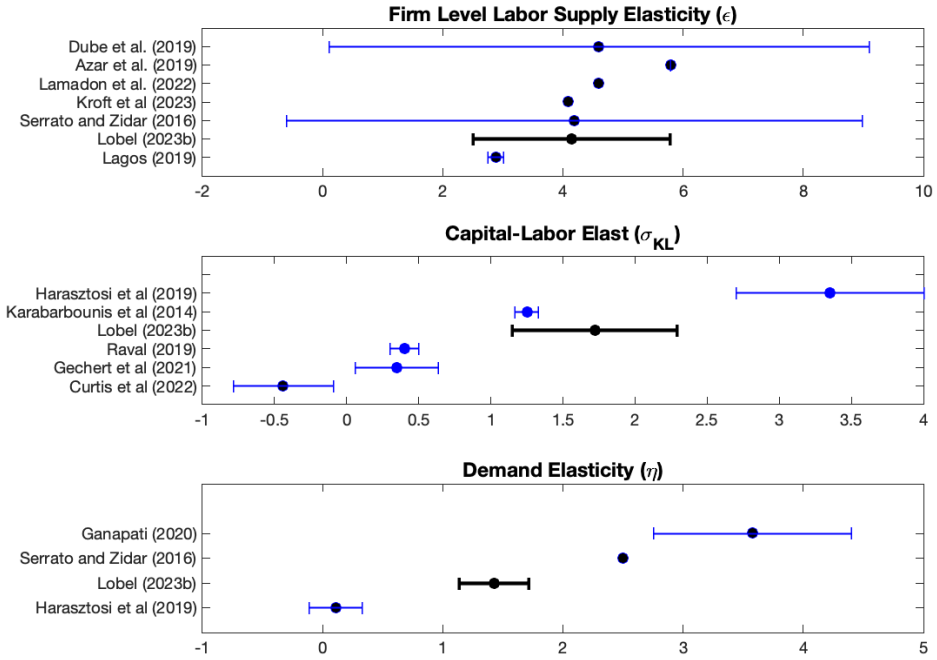
The output demand elasticity with respect to price is 1.43 (SE 0.29), a value greater than one, which aligns with the conventional notion that monopolies operate on the elastic side of the demand curve. If a firm independently decides to raise prices, the quantity loss outweighs the revenue gains from higher prices. Heterogeneous responses to the tax variation reveals that large firms have substantial more market power in the product market. The output demand elasticity for small and large firms is 5.21 (SE 2.93) and 1.10 (SE 0.22), respectively. These elasticities are key to examining the theoretical implications of market power on

[28]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.

[29]Important to highlight that this is not a consensus in the literature. Other recent studies have found that capital and labor are complementaries (Raval 2019).

the scale response to a tax cut, a phenomenon that will play a central role in the subsequent discussion of underlying mechanisms.

Figure 8: Literature Benchmark




*Note:* This figure places my estimates with respect to existing estimates in the literature. The parameters are outlined in the x-axis, and its respective estimates 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 14-17 and the estimated structural parameters. Notice that there are 4 moments and only three unknown parameters $(\epsilon, \sigma, \eta)$, 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 conduct 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 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%[30], 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 in 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.

[30]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 the firm's responses after allowing for an initial transition period. However, using the difference-in-differences coefficients don't cause abrupt changes to the structural estimates.

Table 4: Structural Estimation

| | (1) | (2) | (3) |
|---|---|---|---|
| **Structural Estimates** | **Baseline** | **Small Firms** | **Large Firms** |
| Labor Supply Elasticity, $\epsilon$ | 4.15 | 5.75 | 4.25 |
| | (1.63) | (2.65) | (2.23) |
| Labor-Capital Elasticity, $\sigma_{KL}$ | 1.72 | 5.01 | 1.25 |
| | (0.57) | (2.95) | (0.56) |
| Output Demand Elasticity, $\eta$ | 1.43 | 5.21 | 1.10 |
| | (0.29) | (2.93) | (0.22) |
| **Mechanisms** | | | |
| Price effect, $\beta_P$ | -0.06 | -0.05 | -0.06 |
| Scale effect, $\beta_Q$ | 0.08 | 0.24 | 0.06 |
| Scale / Employment, $\beta_Q/\beta_L$ | 0.65 | 0.70 | 0.64 |
| **Empirical Estimates** | | | |
| Employment effect, $\beta_L$ | 0.12 | 0.35 | 0.09 |
| Capital effect, $\beta_K$ | -0.03 | 0.01 | -0.03 |
| Earnings effect, $\beta_w$ | 0.03 | 0.06 | 0.02 |
| Revenue effect, $\beta_R$ | 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 |

*Notes:* This table presents the parameters estimated, according to the method presented in 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. In the "Mechanisms" section, the table reports effects on prices ($\beta_P$), scale ($\beta_Q$), 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 subsection, 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, in order 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.

**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, $\tau_0$ is the payroll tax rate in the pre-reform period, and $\tau_1$ 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 outlined by:

$$dH = \tau_1 dB = \tau_1 B \left( \frac{\epsilon + 1}{\epsilon} \right) \epsilon^L_{1+\tau} \overbrace{\phi_1}^{\equiv \frac{\partial \tau}{\tau_0}}$$

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

$$dR = dM + dH = B \left[ (\tau_1 - \tau_0) + \tau_1 B \left( \frac{\epsilon + 1}{\epsilon} \right) \epsilon^L_{1+\tau} \phi_1 \right]$$

where, $\epsilon^L_{1+\tau}$ is the elasticity of employment with respect to the labor cost, and $\phi_1$ measures the effect of the reform on labor cost, which has a negative sign in this case. Given that said, two things worth noting. First, a greater employment

response to the reform implies less tax revenue loss. Second, for a fixed employment response, increased labor market power leads to more 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 avoid taking stances on the social welfare function, and to ensure comparability with existing literature, I rely on a money metric approach for welfare measurement. To assess the tax incidence to firm owners, the causal effects of the tax reform on profits can be directly observed. The comprehensive dataset is an addition to related work that relied on structural models to calculate the tax incidence on firm owners in cases where profits were not observed (Suárez Serrato and Zidar 2016). Therefore, the welfare impact on firm owners is calculated as follows:

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

where, $\epsilon^{\pi}_{1+\tau}$ is the elasticity of profits with respect to the labor cost, while $s_L$ and $s_\pi$ 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 weighting the welfare attributed to each stakeholder.

The tax impact on a monopsonistic labor market equilibrium illustrates the implications for worker surplus, as follows

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

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,

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

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 $\eta$, which is estimated based on the perfect fit between model and data. Intuitively, the combination of observed effects on revenues, production inputs, and a well specified demand curve (refer to the overid tests), allow 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 a 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 accross 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

Table 5: Structural Parameters and Incidence Estimation

| | (1) Identified by | (2) Estimate |
|---|---|---|
| *Panel A. Parameters Estimate* | | |
| Labor Supply Elasticity, $\epsilon$ | $\frac{\beta_L}{\beta_W}$ | 4.14 |
| K-L Elasticity of Substituion, $\sigma$ | $\frac{\beta_K - \beta_L}{\beta_W + \phi_1}$ | 1.72 |
| Demand Elasticity, $\eta$ | $\frac{\sigma\beta_R - \beta_K}{\beta_R - \beta_K}$ | 1.43 |
| *Panel B. Incidence* | | |
| Worker, $dB$ | $\beta_W$ | 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.

Hendren and Sprung-Keyser 2020. These results enhance our understanding of tax incidence and efficiency, particularly in the context of developing economies.

## 5.3 Alternative Policies

From a policy perspective, having acknowledged the influence of market power on the efficiency and distributional outcomes of tax policy, immediate questions arise: How can we design better tax policies? Can we quantify the potential impacts of alternative policies? This subsection answers these inquiries by proposing and evaluating two alternatives: First, improving the targeting of the tax subsidy only to small firms. Second, implementing labor market regulations aimed at eliminating monopsony power.

### 5.3.1 Targeting Small Firms

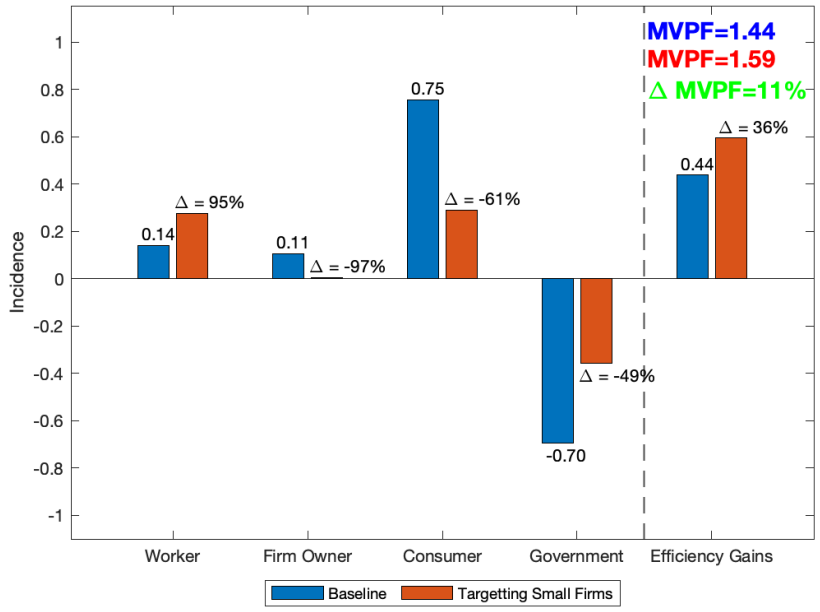
Targeting tax subsidies to small firms presents a logical proposition for two main reasons. First, firm size represents an easily observable characteristic, thereby enabling simple policy tagging. Second, section 5.1 have illustrated that small firms possess less power in both product and labor markets, which consequently makes taxation more burdensome for them. Thus, this subsection aims to quantify gains and further evaluate potential winners and losers in this alternative targeting. To operationalize this exercise, I compute the incidence and efficiency using the empirical moments and structural parameters estimated for small firms.

Figure 9 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 part of the subsidy cost. In simpler terms, smaller firms tend to expand employment relatively more, which makes it cheaper to subsidize them. Quantitatively, behavior responses counterbalance 49% of the revenue decline. This alternative policy stands out as more efficient, even in a revenue preserving scenario, which is evidenced by the 11% rise in MVPF.

In terms of distributional impacts, the alternative policy's effects on workers is not immediately clear. On one hand, smaller firms face a less steep labor supply curve; on the other, their lack of product market power allows for more prominent behavior response, reflected in their scale effect. In this exercise, I find that workers experience a 95% gain in the alternative scenario, suggesting that moving further along the supply curve more than compensates 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 9: Baseline vs Targeting Small Firms



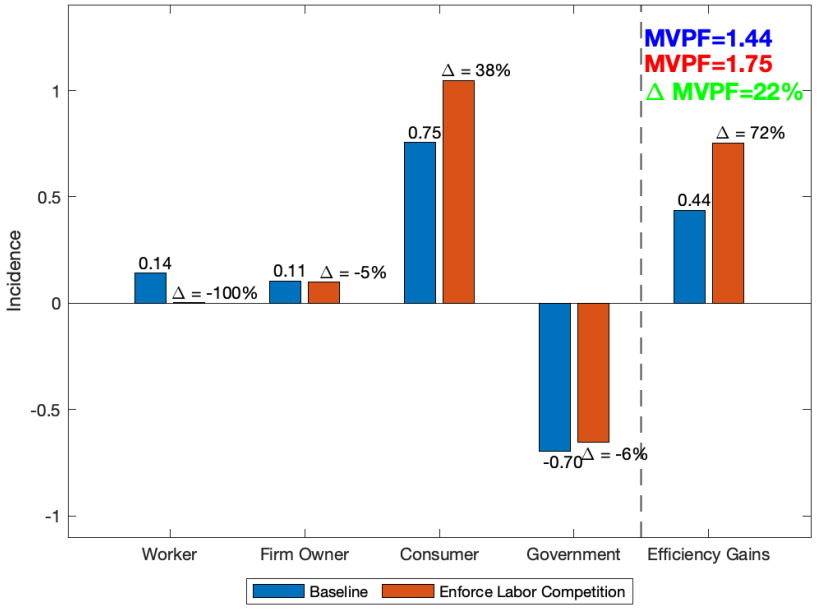

*Note:* This figure presents the baseline incidence analysis (blue bars), and counterfactual incidence in case of alternative targetting. The alternative policy, represented in the red bars, relies on a policy that targets only firms with low market power. The figure plots the differential incidence, and differential efficiency gains across the baseline and alternative policy.

### 5.3.2 Enforce Labor Market Competition

In the baseline evaluation, both labor and product market power shape firms' responses to the tax reform. However, to disentangle the role of each of these competitive frictions, the counterfactual exercise envisions a policy capable of eliminating labor market power. Such a policy could take the form of regulatory sanctions, the dismantling of entry barriers, or more broadly, any initiatives that foster labor market competition. To operationalize this evaluation, I simulate the firm's pass-through to labor, capital, and revenue, when labor supply elasticity ($\epsilon$) approaches infinity.

Figure 10 shows that when labor market power is assumed away, the efficiency of tax cuts increases remarkably by 72%. These efficiency gains come with winners and losers. Relative to baseline, workers lose their entire welfare gains because in the alternative scenario their labor supply is perfectly elastic. Competitive forces in the labor market stimulate firms to ramp up production, downward pressuring output prices, especially as firms retain product market power. Consequently, consumers are unambiguously better off, and their welfare increases by 38%. Firm owners' welfare, ex-ante, could go in either direction. On one hand, perfectly competitive labor markets allow firms to expand without sharing rents with incumbent workers. On the other, greater expansion combined with product market power leads to a more salient price decrease. At the end of this horse race between cost and revenue effects, firm owners experience a minor loss of 5% relative to receiving the same tax benefit in the baseline monopsonistic labor market.

Figure 10: Baseline vs Enforcing Labor Competition




*Note:* This figure presents the baseline incidence (blue bars), and counterfactual incidence when labor market competition is enforced. The alternative policy, represented in the red bars, assumes that the labor supply elasticity faced by the firm is infinity. The figure plots the differential incidence, and differential efficiency gains across the observed and alternative policies.

### 5.3.3 Revenue Maximizing Tax Rate

To compute the behavioral effect we rely on the elasticity of labor with respect to the labor cost that was empirically estimated from the quasi-experimental variation. Notice 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 $\tau_0$ to $\tau_1$:

$$\frac{\partial L}{\partial 1+\tau}(1+\tau_1) = \frac{\partial L}{\partial 1+\tau}(1+\tau)\Big|_{\tau=\tau_0} + \frac{\partial L}{\partial^2 1+\tau}(1+\tau)\Big|_{\tau=\tau_0}(\tau_1-\tau_0) + \frac{1}{2}\frac{\partial L}{\partial^3 1+\tau}(1+\tau)\Big|_{\tau=\tau_0}(\tau_1-\tau_0)^2 + ... \quad (21)$$

$$\frac{\partial L}{\partial 1+\tau}(1+\tau_1) = \frac{L\epsilon_{L,1+\tau}}{1+\tau}\Big|_{\tau=\tau_0}\left[1 + \frac{(\epsilon_{L,1+\tau}-1)}{1+\tau}\Big|_{\tau=\tau_0}(\tau_1-\tau_0) + \frac{1}{2(1+\tau)^2}\Big|_{\tau=\tau_0}(\epsilon_{L,1+\tau}(\epsilon_{L,1+\tau}-1)+2)(\tau_1-\tau_0)^2\right] \quad (22)$$

In counterfactual scenarios, where the payroll tax rate moves to $\tau_1$, 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 22. With this framework, I simulate the revenue impact from perturbing the labor tax rate. Figure 11 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%[31].

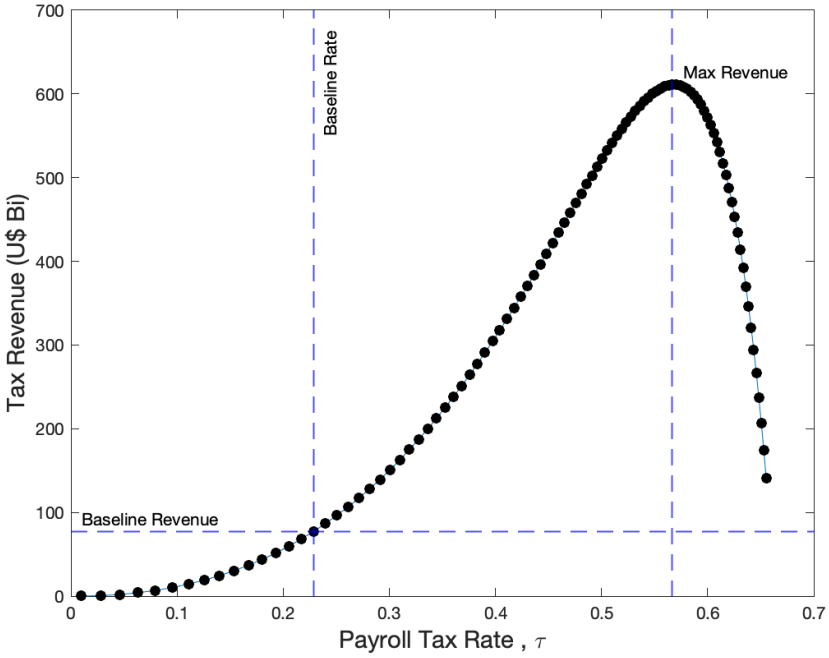
One immediate takeaway from this exercise is that payroll tax rates in Brazil are fairly far from the revenue maximizing rate, which is indicative that existing average tax rates are on the "right side of the Laffer curve". The direct consequence is that Brazilian policymakers can increase the payroll tax without fearing a decline in tax revenue. In fact, this conclusion is further supported by the

[31] Alternatively, this could be expressed as a firm's payroll tax rate of 130%. Throughout this paper, we refer to the payroll tax rate as it applies to the wage bill paid by firms. To establish a connection with prior research exploring revenue maximizing tax rates, it is helpful to express the tax rate that applies to workers' take-home payment. In this case, if the tax rate t=1, there is no incentive to work. A one-to-one relationship between the two aforementioned tax rates can be depicted as:

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

positive MVPF reported on table 5.

Figure 11: 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 reduction to a small share of firms in Brazil. The setting allowed fewer assumptions on general equilibrium effects, in order 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 novel 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 curvature of the product demand and labor supply curves, and the associated behavioral

responses. While market power leads to steeper curves, it also restricts the behavioral responses. Therefore, the role of market power in determining the distortionary costs of taxation is not obvious. The theoretical framework I provide, combined with rich data, suggests that market power reduces the efficiency cost of taxation and shifts the incidence away from workers and towards 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 due to the rise of novel firm-level datasets, these policies have attracted renewed attention from scholars. To understand the forces at play in such policies, I aim to convene a simple yet essential message: the role of market power must be taken into account. When adapting this framework to study macro tax policy, an intrinsic limitation of this paper is the inability to explore the interplay between market power and general equilibrium effects due to the nature of the examined policy. To bridge this gap, I am partnering with the United States Internal Revenue Services (IRS) to study the Tax Cuts and Jobs Act (TCJA), and incorporate the role of general equilibrium effects in future work.

This paper lays the groundwork for two additional research avenues that I am currently pursuing. First, the observed patterns of imperfect subsidy take-up have inspired a study into the role of tax preparers in such behavioral responses. To this end, I am partnering with the U.S. Treasury in a study that employs the AKM framework to identify accountants' fixed effects, and incorporate this into several tax policy applications. Second, this paper presents novel evidence of racial discrimination in tax policy. Unlike statistical or taste-based discrimination studied in Labor Economics, the issue within Public Finance is that tax policy may inadvertently amplify pre-existing market failures, leading to racially unequal pass-through effects. Finally, the insights into the substitution between capital and labor shed light on a new avenue for integrating market power considerations into the literature on capital-skill complementarity.

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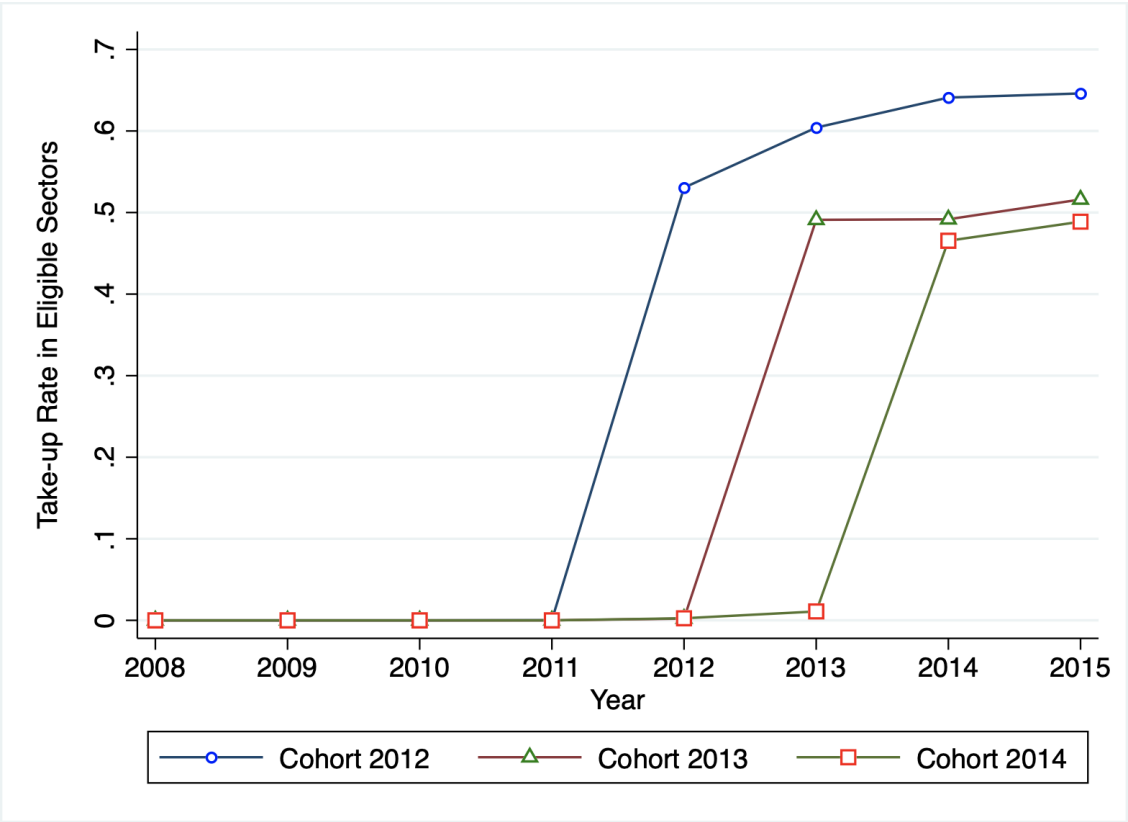
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# 7 Figures and Tables

Figure 12: 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 their 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 years prior to the implementation of the reform to each cohort.

Figure 13: Tax Forms Information

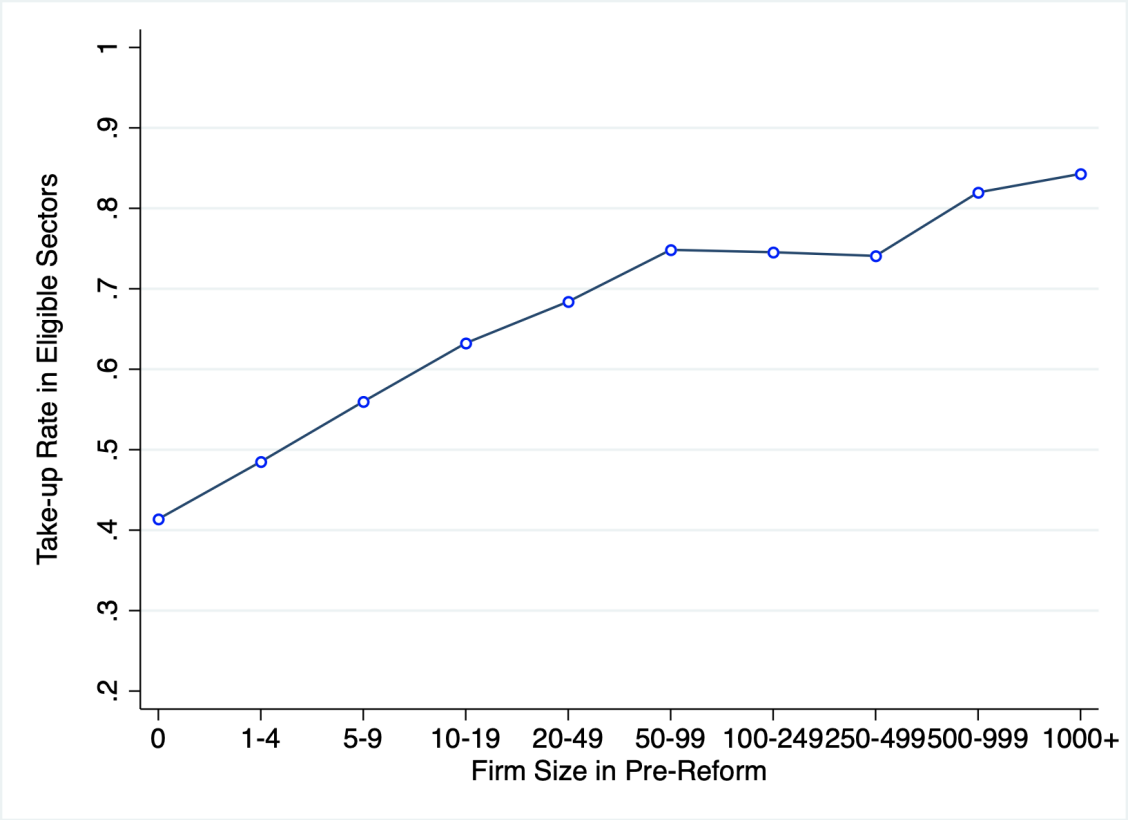
| Nº | Campo | Descrição | Tipo | Tam | Dec | Obrig |
|---|---|---|---|---|---|---|
| 01 | REG | Texto fixo contendo “0145”. | C | 004* | - | S |
| 02 | COD_INC_TRIB | Código indicador da incidência tributária no período:<br>1 – Contribuição Previdenciária apurada no período, exclusivamente com base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

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

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

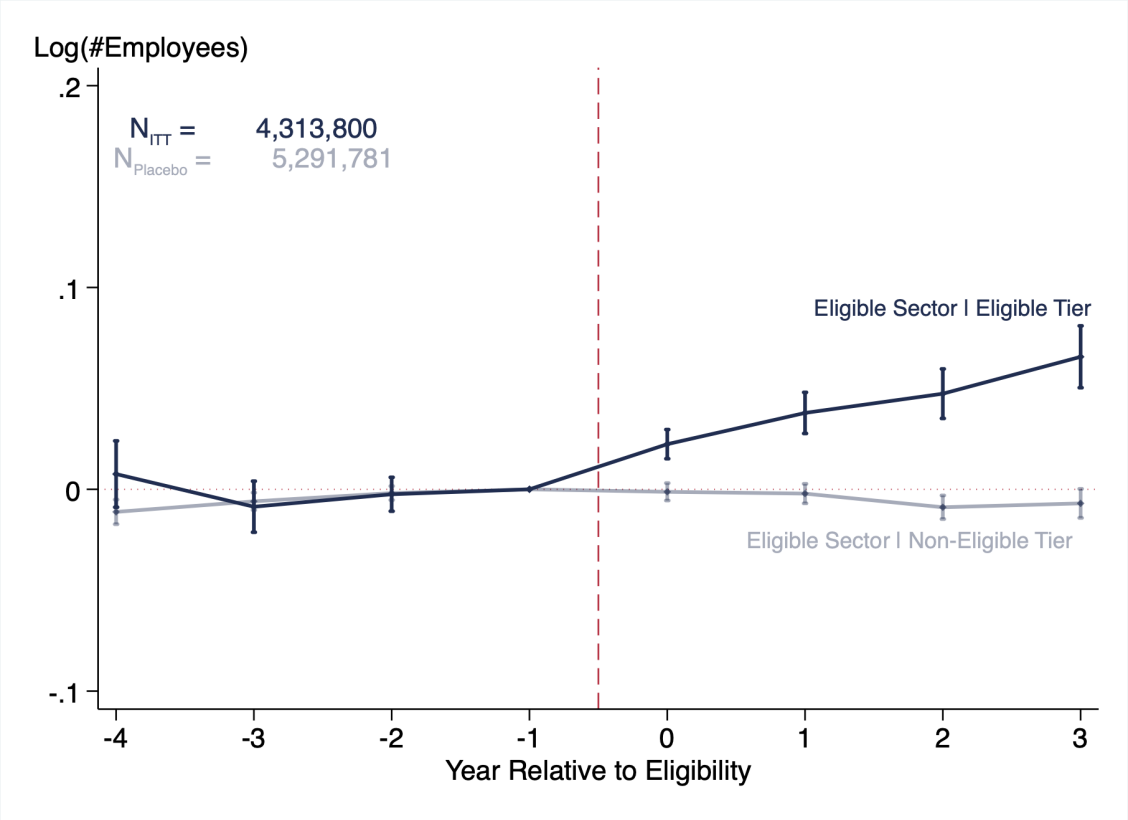
*Note:* This figure shows instructions for eligible firms to request the payroll tax benefit. It describes detailed information to be provided in Tax Administration software, in order to substitute part of the payroll tax by revenue taxes.

Figure 14: Take-up per Firm Size



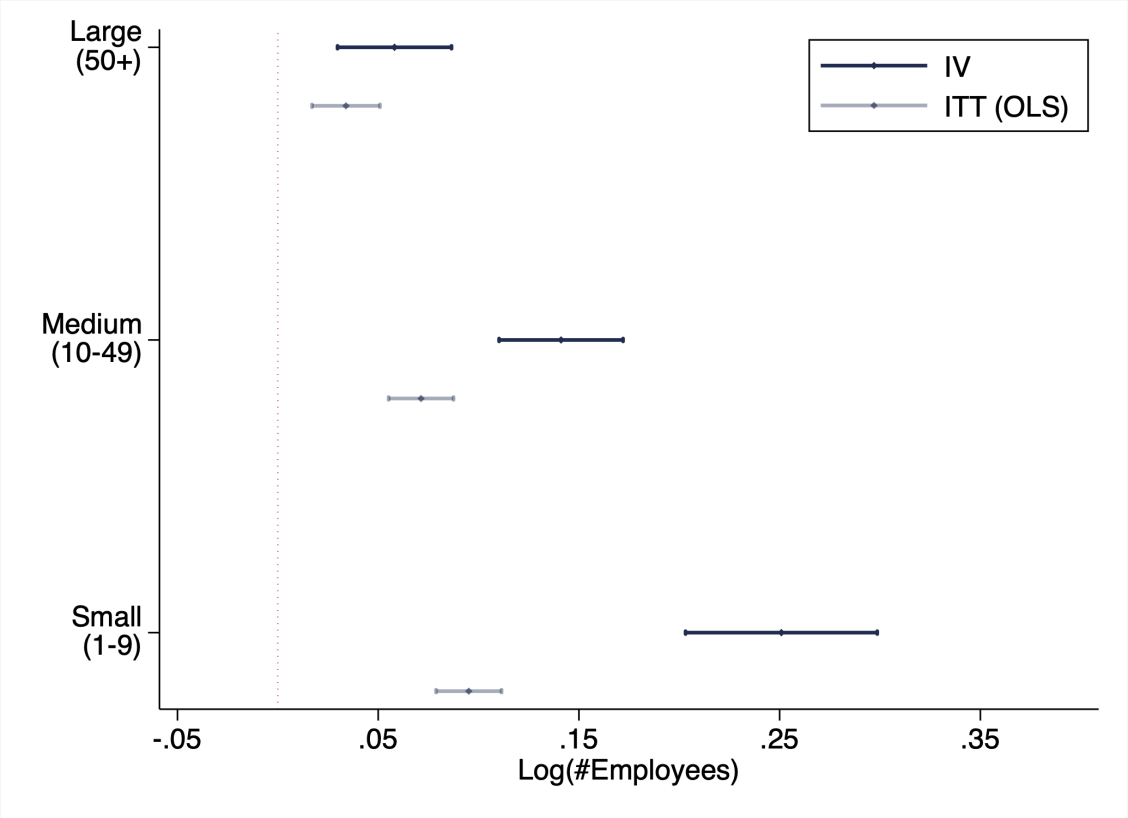

*Note:* This figure plots cohort specific take-up rates among eligible firms. Eligibility is based on the firm's 7-digit sector, and their observed tax tier. Firms in the "Simples" tax regime are not eligible for the reform, even if they belong to eligible sectors. Firms that have ever participated in 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 it can be seen, eligibility is monotonically increasing with firm size.

Figure 15: Spillover Test



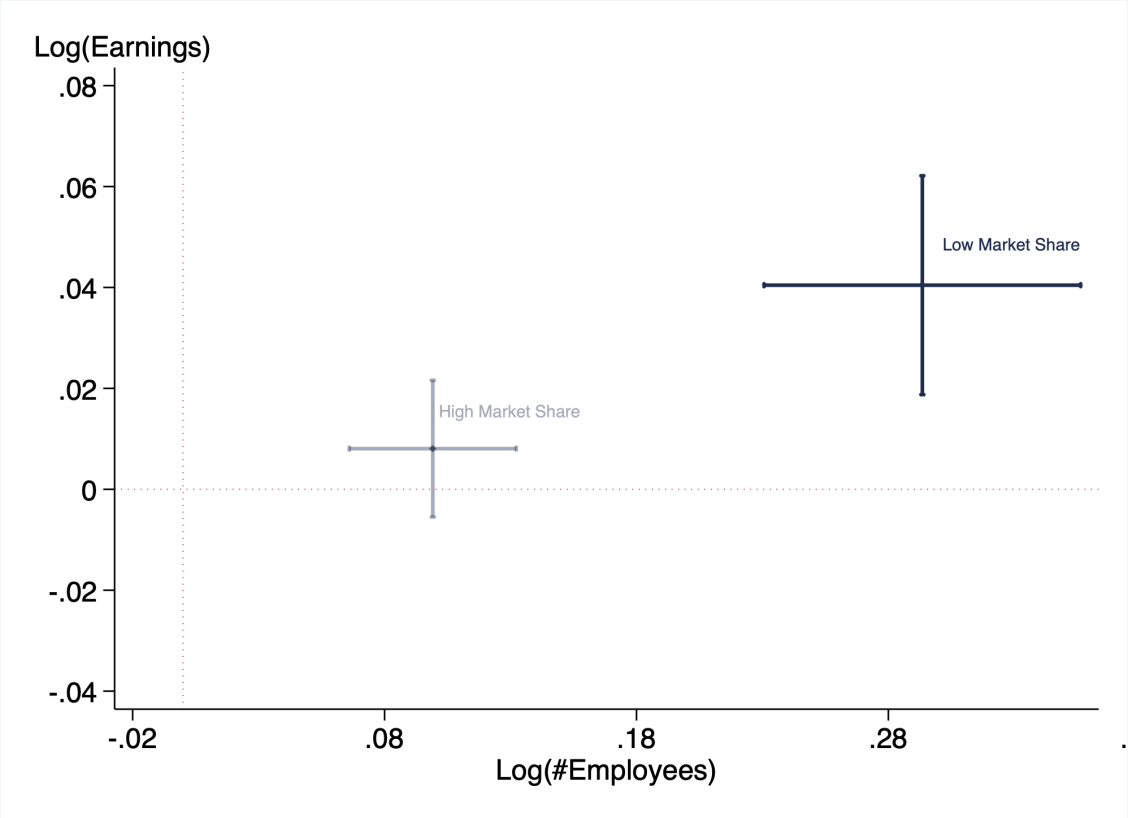

*Note:* The gray line plots event study coefficients that show non-statistically significant spillover effect to firms in eligible sectors, but ineligible tax tiers. The gray line is estimated on a sample that is restricted to firms in non-eligible tax tiers ("Simples" regime) and depicts a comparison 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 16: Earnings and Employment per Firm Size



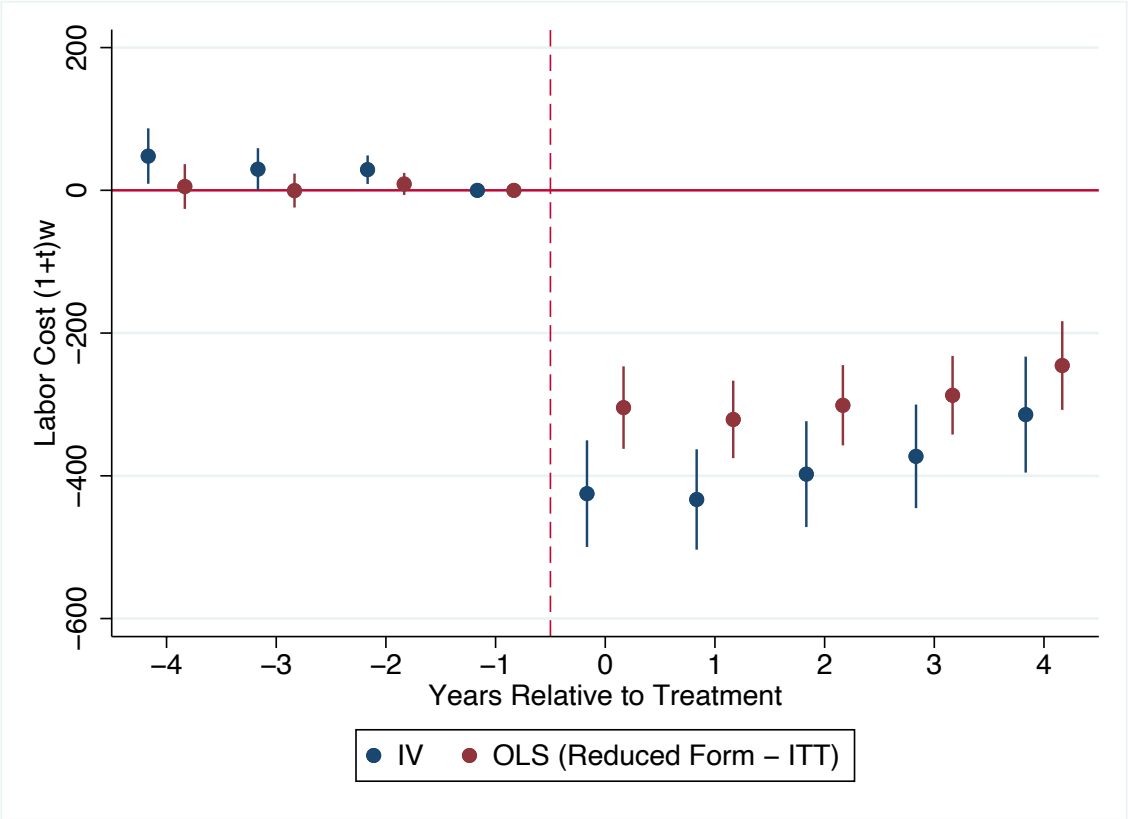

*Note:* This figure presents the event study estimates for the firm-level estimates, for three firm size groups (small, medium and large firms). Size categories are defined in the pre-reform period. Firms are classified as small if they had less than nine employees, medium if they had between 10 and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

Figure 17: Earnings and Employment per Market Power



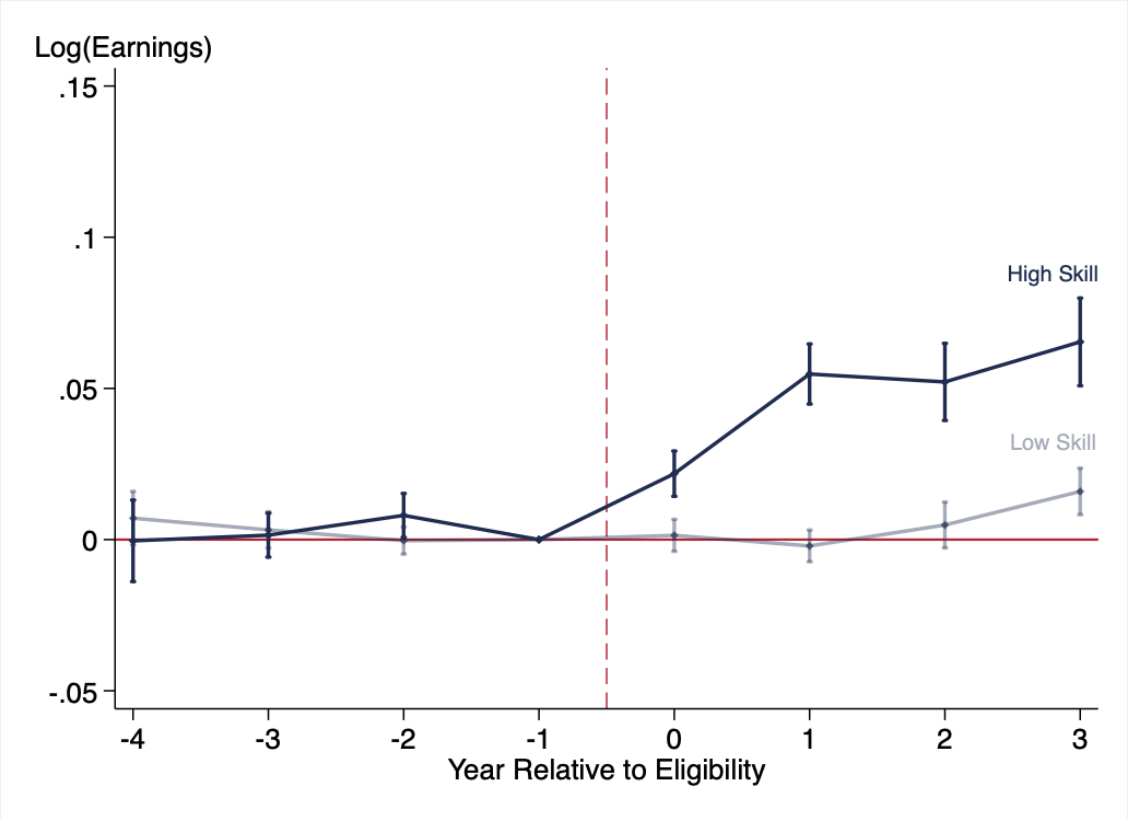

*Note:* This figure presents firm-level IV difference-in-differences coefficients for above and below the median on pre-reform employment market share within each local labor market. The outcomes are employment and earnings. The blue marker plots the effect for firms below the median (low market power), 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 18: Worker Level: Gross Earnings Effect



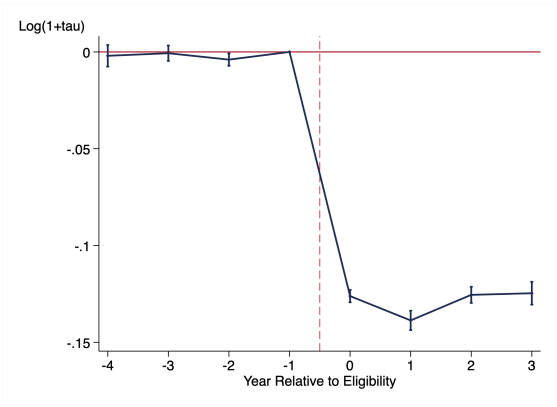

*Note:* This figure presents the event study estimates for average gross earnings paid workers that were employed for at least three years in the same firm during the pre-reform period. The labor cost is computed using firm-level tax data, and worker-level earnings data. I apply the firm payroll tax 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 19: Worker Level: Earnings per Occupation

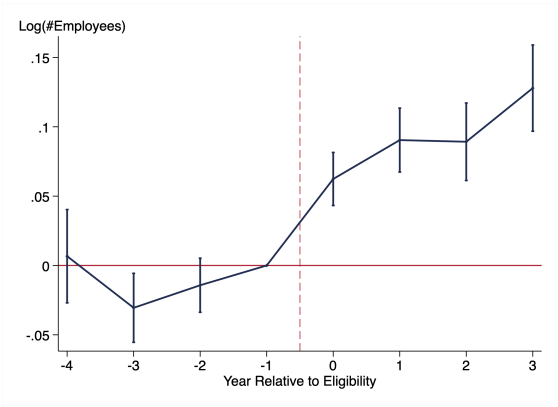



*Note:* This figure presents the event study estimates for the log of earnings per occupation group, at the worker-level based on pre-reform occcupations. Leaders are directors, managers and qualified technical positions according to the CBO classification. While leaders experience high pass-through 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.

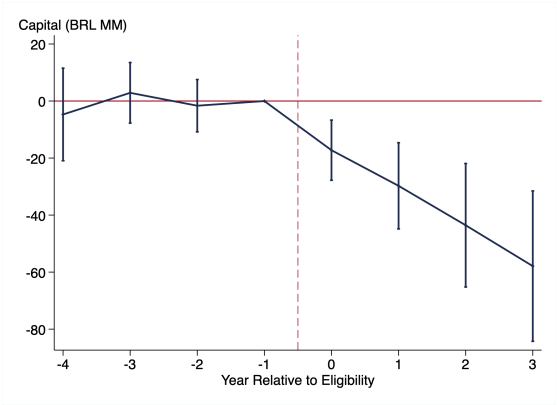
Figure 20: Firms' Margins of Adjustment



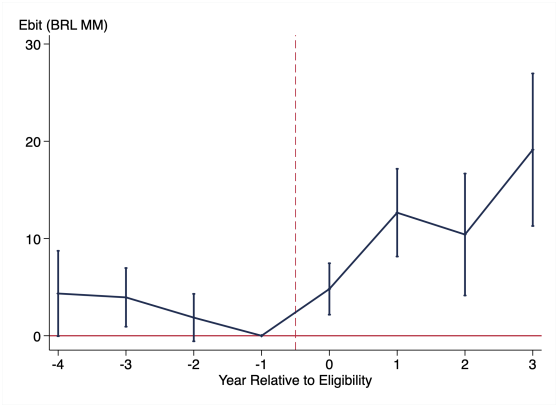

(a) Effects on payroll tax rates




(b) Effects on employment



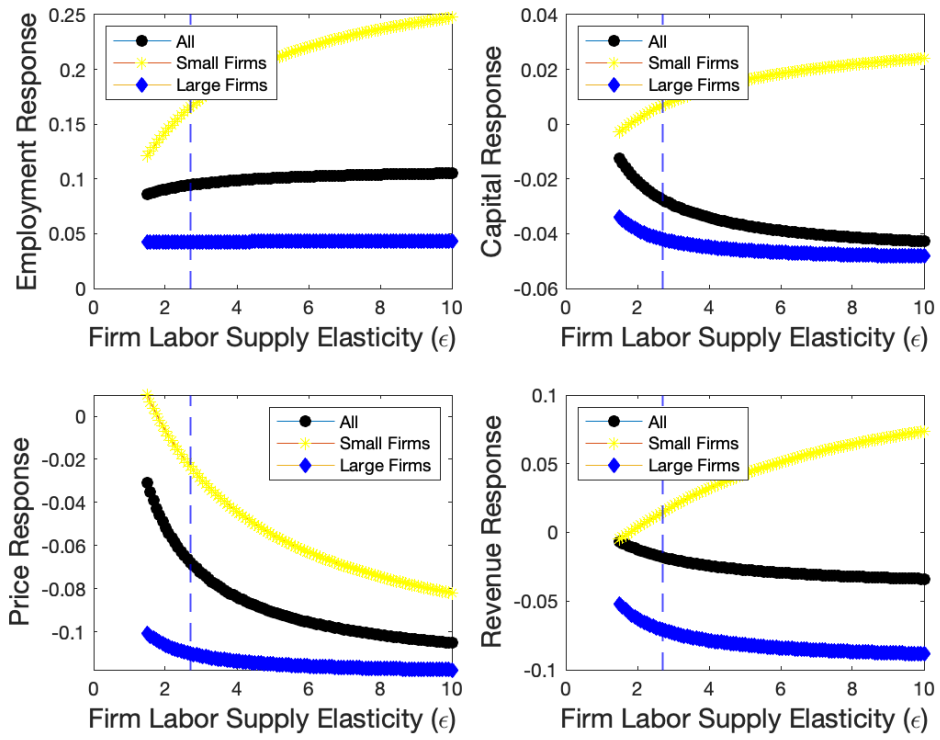

(c) Effects on capital




(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 on 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 21: Simulations




*Note:* This figure simulates the model in several dimensions. In each plot, the x-axis represents the labor supply elasticity faced by the firm. As we move from the left to the right, we are moving to more competitive labor markets. The horizontal dashed line depicts the labor supply elasticity estimated in the data. The black line simulates the effects for the overall sample. This would be analogous to fixing the targeting of the policy and perturbing the degree of labor market competition. The yellow line represents the effects to small firms, which is useful to evaluate the effects of an alternative policy that focuses its targeting on firms with low market power. The blue line represents the effects to large firms, which is useful to evaluate the effects of an alternative policy that focuses its targeting on firms with high market power. The four plots represents the counterfactual responses across the margins of employment, capital, price, and revenue.

Table 6: 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 7: 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 does 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 administred 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 dividing # of workers by the universe of Brazilian workers according to PNAD-C.

Table 8: Descriptive Statistics (Worker Level Sample)

| | (1)<br>Non-Eligible (pre) | (2)<br>Eligible (pre) | (3)<br>Avg (pre) |
|---|---|---|---|
| *Descriptive Statistics* | | | |
| Earnings | 2,326.54<br>(3,060.70) | 2,166.05<br>(2,837.04) | 2,315.46<br>(3,046.06) |
| Employees Age | 39.30<br>(10.82) | 37.20<br>(10.51) | 39.16<br>(10.81) |
| Share White | 0.67<br>(0.47) | 0.65<br>(0.48) | 0.67<br>(0.47) |
| Gender | 0.54<br>(0.50) | 0.79<br>(0.41) | 0.55<br>(0.50) |
| High School + | 0.70<br>(0.46) | 0.62<br>(0.49) | 0.70<br>(0.46) |
| College + | 0.27<br>(0.45) | 0.18<br>(0.38) | 0.27<br>(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) pool this two groups together. The variable "Earnings" reports monthly earnings in the month of December for employed workers. The currency used is the Brazilian Reais (BRL). "Employees Age" reports age of employed workers in the sample, on December of each year. "Share White" reports 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 parenthesis.

Table 9: 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) pool this 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 parenthesis.

Table 10: Worker Level Estimates

| **Worker Level** | **Log(Earnings)** | **Log(Earnings)** | |
|---|---|---|---|
| | All Sample | Blue Collar | White Collar |
| | (1) | (2) | (3) |
| ***Panel B: IV*** | | | |
| **Diff-in-Diff** | .018** | .003 | .058*** |
| | (.007) | (.007) | (.014) |
| **Long Diff** | .027*** | .016** | .064*** |
| | (.007) | (.008) | (.014) |
| ***Panel A: OLS*** | | | |
| **Diff-in-Diff** | .009** | .002 | .031*** |
| | (.004) | (.004) | (.008) |
| **Long Diff** | .017*** | .01* | .044*** |
| | (.005) | (.005) | (.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 11: 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*** (.016) | .022 (.013) | .01 (.011) | .003 (.01) | .001 (.002) |
| **Long Diff** | .068*** (.016) | .038*** (.013) | .012 (.011) | -.003 (.011) | .005 (.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 earnigns 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.

Table 12: 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 are short term bills, such as the wage bill. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table 13: Informality Analysis

| | (1) Log(1+$\tau$) | (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.

# The Visible Hands of Firms: Consequences for Efficiency and Incidence of Payroll Taxation

## Appendix

Felipe Lobel - UC Berkeley

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# A Details on the Empirical Model

## A.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 reduce form equation. Starting with the firm-level design, we obtain the reduced form by substituting all first stage equations into the second stage,

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

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; $\alpha_j$ is the firm fixed effect; $\eta_{jt}$ and $\epsilon_{jt}$ 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,

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

Notice that if K=L and diagonal is such that $\pi_{kl} = 0$ (when k $\neq$ l ), then $\rho_l = \beta_l \pi_{ll}$, and $\beta_l = \frac{\rho_l}{\pi_{ll}}$. However, if K$<$ $L$ then the system $\rho_l = \sum_{k=-4,\neq-1}^{3} \beta_k$ for l=1,...., L is a system of L equations in K<L unknowns and generally cannot be solved. The off diagonal coefficients estimated in 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.

## A.2 Characterizing Compliers

Section 3.1 stresses that the causal interpretation for the LATE is restricted to the set of compliers. Often times, compliers are not representative of the population, therefore it is useful to have deeper understanding on who the compliers are. The challenge is that differently 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 Abadie 2002 proposes a 2SLS approach to detect compliers. This method relies on the fact that *never-takers* (eligible firms that don't 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 instrumeting eventual treatment or never treatment; and $\alpha_d, \zeta_d$ are constants. The IV coefficients for d ={0,1} recovers 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* come from regressing $X_{jt}D_j(1-L_{s(j)})$ on $D_j(1-L_{s(j)})$. Table 14 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[32]. The table shows that covariates' means for treated and untreated compliers are not statistically distinguishable between each other, except for share of male workers, variable which we control for in all specifications. As Angrist, Hull, and Walters 2022 points out, the balance check accross 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.

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

Table 14: 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 parenthesis and conservatively clustered at the 5-digit industry-by-state level.

# B Model

In this appendix, I present the model derivation. For didactic purposes, I start by analyzing the revenue and payroll taxes, separately. At 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' choice.

## B.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 induce 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 derive 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 $\nu_{ij}$ is the idiosyncratic preference for worker i to be at firm j. Assuming that $\nu_{ij}$ 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 $\lambda$ 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, $\epsilon$ is the constant labor supply elasticity faced by the firm.

## B.2 Effects of Payroll Taxation

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

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

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

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

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

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

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

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

*Proof.* From FOC,

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

From constant returns,

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

□

The profit maximizing firm chooses output Q,

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

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

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

To evaluate the effect on labor costs $(1+\tau)$ induced by the payroll tax variation, I take logs and differentiate with respect to $(1+\tau)$,

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

Also notice 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 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 don't oberserve neither $\lambda$, nor Q. However, 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 $\epsilon$. It turns out that we

do observe labor share ($s_L$), and we can estimate $\epsilon$. 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, means that reducing labor cost will have greater impact on the marginal cost. Second, is the inverse mark down. The intuition for this term is that as labor market power increases, there is more rents to be shared with incumbents 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,

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

Notice 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:

$$\epsilon^{L}_{1+\tau} = \left(\frac{\epsilon}{1+\epsilon(1-\rho)}\right)\left(\Omega(\epsilon,\eta,\rho)-1\right) \tag{15}$$

where,

$$\Omega(\epsilon,\eta,\rho) = \left(\frac{(\epsilon+2\epsilon^{L}_{1+\tau})(\sigma-\eta)}{\sigma\epsilon}\right)\left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right) \tag{16}$$

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.

$$\epsilon^{K}_{1+\tau} = \left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+2\epsilon^{L}_{1+\tau}}{\epsilon}\right)\left(\frac{1}{1-\rho}-\eta\right) \tag{17}$$

$$\epsilon^{R}_{1+\tau} = (1-\eta)\left[\left(\frac{\epsilon+1}{\epsilon}\right)\left(\frac{\epsilon+\epsilon^{L}_{1+\tau}}{\epsilon}\right)\left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\right]\left(\frac{\epsilon+2\epsilon^{L}_{1+\tau}}{\epsilon+\epsilon^{L}_{1+\tau}}\right) \tag{18}$$

## B.3 Effects of Revenue Taxation

Under revenue taxation ($\tau_r$), 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 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 $\epsilon^{\lambda}_{Q}$, which we can pin down by differentiating the cost function with respect to Q,

$$\epsilon^{\lambda}_{Q} = \left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+1}{\epsilon}\right)\frac{\epsilon^{L}_{Q}}{\epsilon} \tag{22}$$

Notice 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 $\epsilon^{\lambda}_{Q}$ and $\epsilon^{Q}_{\tau_r}$ in 20 and 21, we obtain the closed form pass-through expressions for the revenue taxation,

$$\epsilon^{L}_{\tau_r} = \frac{-(1-\rho)\epsilon}{1+\epsilon(1-\rho-\chi(\epsilon,s_L))}\frac{\tau_r}{1-\tau_r}\eta \tag{24}$$

$$, \qquad \epsilon^{K}_{\tau_r} = \frac{\tau_r \eta}{1-\tau_r}\left(\frac{-\chi(\epsilon,s_L)\epsilon}{1+\epsilon(1-\rho-\chi(\epsilon,s_L))}-1\right) \tag{25}$$

where, I denote $\chi(\epsilon, s_L) = \left(\frac{1}{\frac{1}{s_L}+\frac{1}{\epsilon}}\right)\left(\frac{\epsilon+1}{\epsilon}\right)$ to simplify notation. The elasticity $\eta$ makes the model versatile to accommodate different degrees of competition in the product market. As $\eta$ increases we move to a more competitive product market. At first, we will be agnostic about its value, and let $\eta$ 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.

# C 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 Goldin and Katz 1998 and Autor, Goldin, and Katz 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; $\rho$ 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, $\epsilon_i$ represents the labor supply of worker type i $\in (l, h)$. Notice 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 am able to calculate the demand for high and low-skilled labor. More importantly, I derive the labor cost pass-through for each type of labor, as a function of the auxiliary elasticities ($\epsilon^{\lambda}_{1+\tau}$, $\epsilon^{\lambda}_{Q}$, $\epsilon^{Q}_{1+\tau}$):

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

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

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

cost pass-through as a function of observables and structural parameters.

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

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

where,

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

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

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

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

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

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

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

**Identification.** In this augmented model, it is not feasible to obtain closed-form analytical solutions for all the structural parameters as functions of the reduced-form estimates. The notable exceptions are the labor supply elasticities ($\epsilon_h$ and $\epsilon_l$), which can be directly computed from the employment and wage responses for each type of worker. To structurally estimate the parameters $\rho$, $\gamma$, and $\eta$, 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 15 reports the structural estimates.

**Structural estimation.** The elasticity of substitution between high and ow 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 at 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 15 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). 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. In fact, 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 15: Structural Estimation *(Extended Model)*

| | (1) |
|---|---|
| **Structural Elasticities** | **Baseline** |
| Low-High Skill Elasticity, $\sigma_{LH}$ | 1.27 |
| | (0.04) |
| High Skill Labor Supply, $\epsilon_H$ | 3.58 |
| | (1.32) |
| Low Skill Labor Supply, $\epsilon_L$ | 6.01 |
| | (2.54) |
| Output Demand Elasticity, $\eta$ | 1.20 |
| | (0.07) |
| **Empirical Estimates** | |
| High Skill Employment, $\beta_{L_H}$ | 0.14 |
| Low Skill Employment, $\beta_{L_L}$ | 0.12 |
| High Skill Earnings, $\beta_{W_H}$ | 0.04 |
| Low Skill Earnings, $\beta_{W_L}$ | 0.02 |
| Capital, $\beta_K$ | -0.04 |
| Revenue effect, $\beta_R$ | 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 relies on the Delta Method. In contrast, the standard errors for the remaining structural elasticities are computed using the bootstrap method.

# D Capital Taxation in 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 B, but since the tax applies to capital, the firm minimizes the following cost function:

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

**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 pass-through 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, and perfect competition:

$$\sigma_{monop} = \frac{\epsilon^{K}_{1+\tau}(\epsilon+s_L)\epsilon + \epsilon^2 s_K + (\epsilon+1)s_L\epsilon^{L}_{1+\tau} - \epsilon^{R}_{1+\tau}(\epsilon+s_L)\epsilon}{\epsilon^2 s_K + (\epsilon+1)s_L\epsilon^{L}_{1+\tau} - \epsilon(\epsilon+s_L)} \tag{1}$$

$$\sigma_{comp} = \frac{\epsilon^{R}_{1+\tau} - \epsilon^{K}_{1+\tau} - s_K}{s_L} \tag{2}$$

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. In order to ensure full comparability, I also derive the structural parameters based on my own empirical findings from the Brazilian payroll tax cuts, using a model of perfectly competitive labor market. The results are consolidated in Table 16.

Table 16: Estimates Under Monopsony vs Perfect Competition

| | (1) | (2) |
|---|---|---|
| | **Capital Tax** (Curtis et al. 2023) | **Payroll Tax** (Lobel 2023) |
| **Panel A:** K-L Elasticity, $\sigma_{KL}$ | | |
| 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 compare 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 the 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 $\sigma_{KL}$ 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.

# E 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. With regard to the sample, 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.

## E.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 17 and 18 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 17: Within-Firm Earnings Inequality

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

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

Table 18: Firm Level Estimates

| | (1) Log(1+$\tau$) | (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 the 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 had less than 9 workers in the pre-period. Medium if they had 10-49, and large if they had more than 50 workers. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

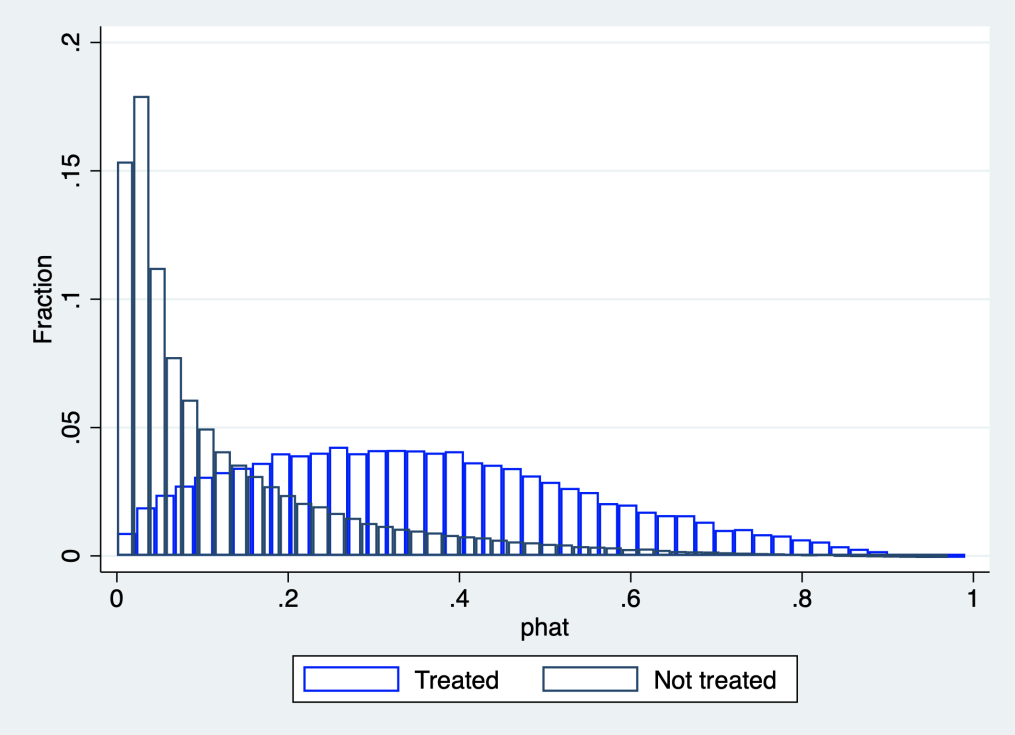
## E.2 Matched Sample

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 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 22. The noticeable overlap between groups provide evidence of support across the estimated propensity score distribution, validating the matching procedure.

Figure 22: 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 19 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, balance holds even across dimensions that were not directly targeted. For instance, for both groups, payroll tax rates are about 34%, 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.

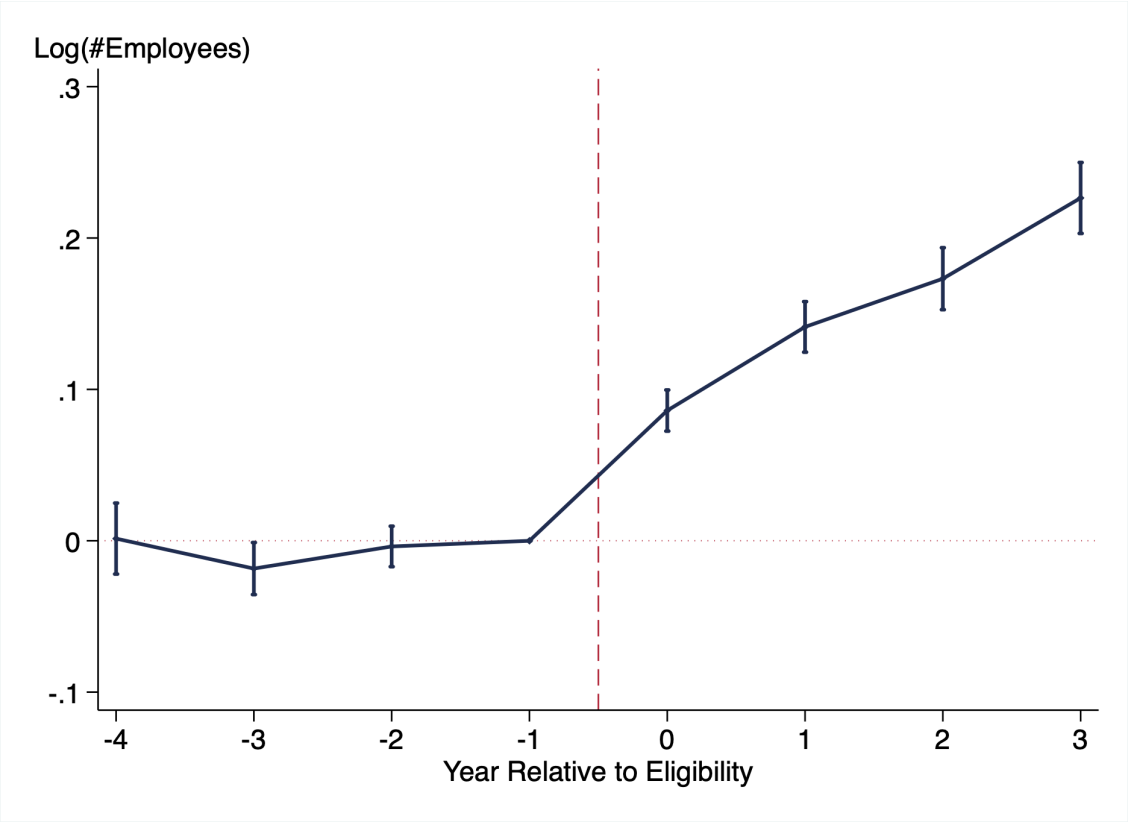
Table 19: Balance on Matched Sample

| | 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 23. 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 23: 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 conduct a placebo test, randomly assigning firms to treatment, and applying 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 21 reports. To showcase that the matching algorithm still works in the placebo sample, Table 20 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 not mistakenly generated by the matching procedure.

Table 20: Balance on Placebo Matched Sample

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

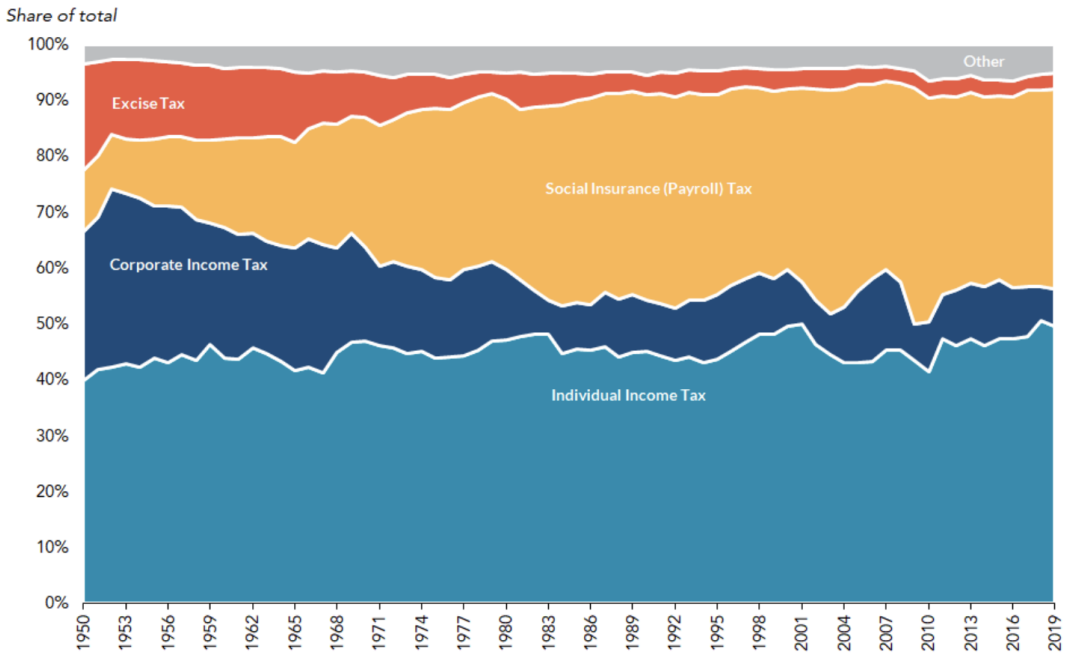
Table 21: Reduced Form on Placebo Matched Sample

| | (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 parenthesis.

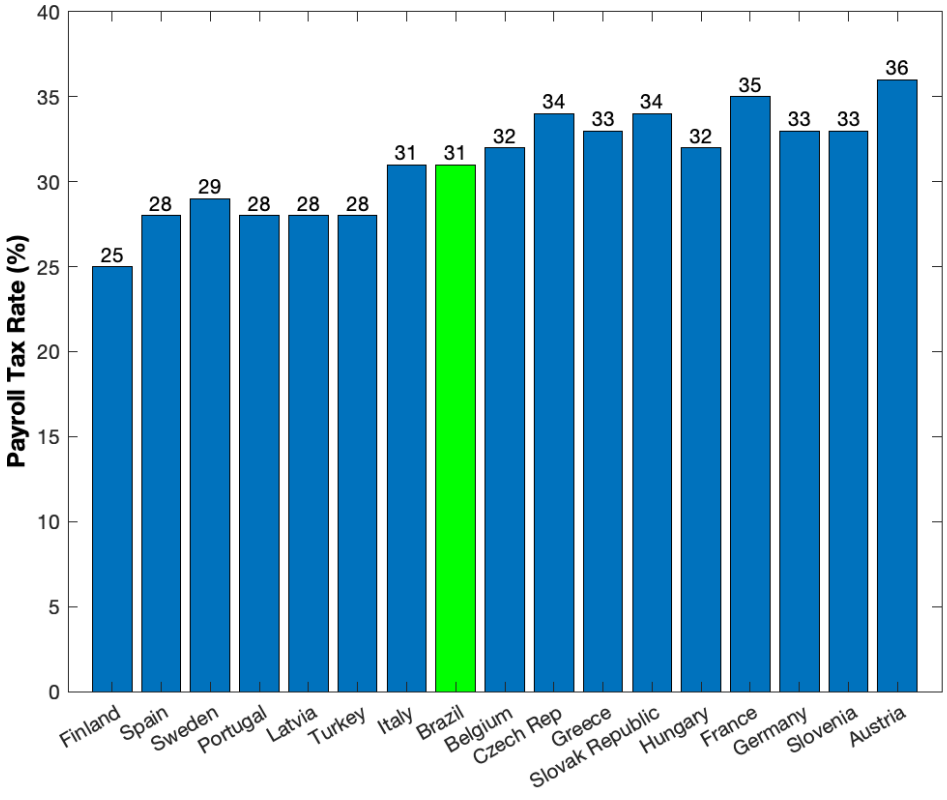
# F Additional Figures and Tables

Figure 24: Sources of Government Revenue



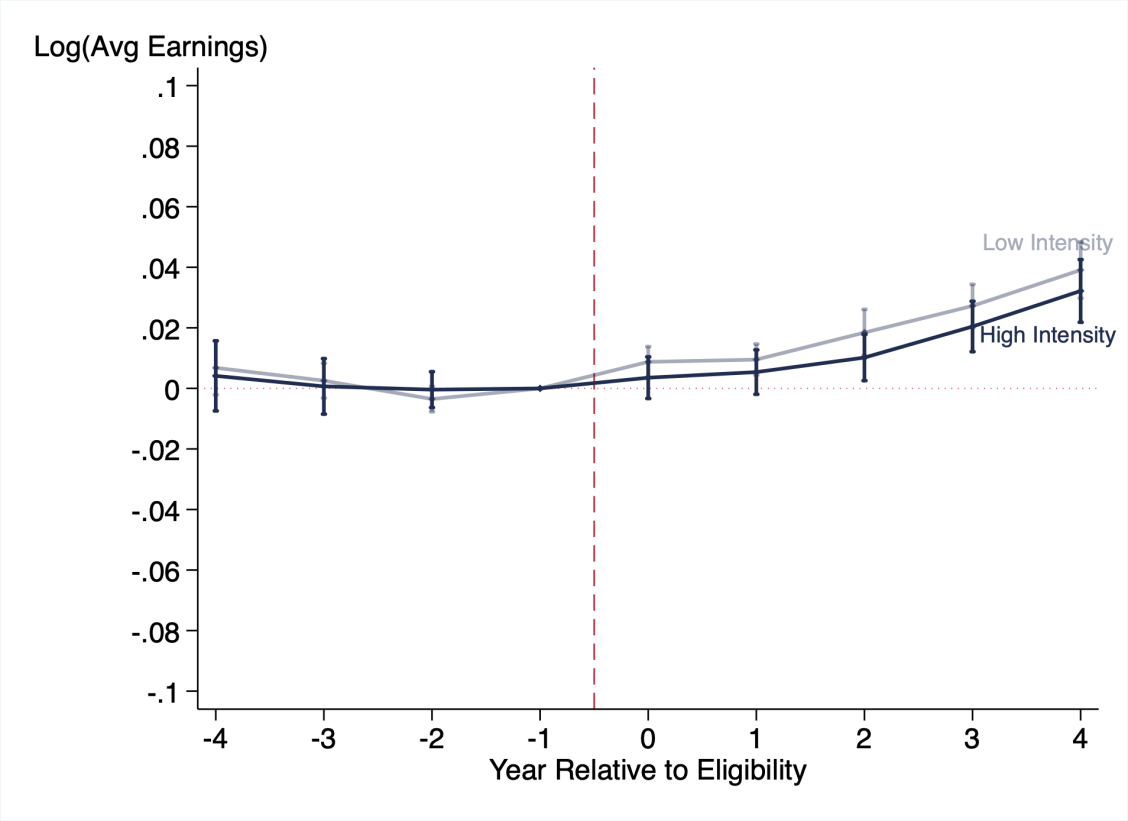

*Note:* This figure shows growing importance of payroll taxes compared to other souces of revenue for the US Federal Government. Currently, payroll taxes are the second most important source, accounting for more than 30% of total revenue.
*Source:* Office of Management and Budget. Historical Tables.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

Figure 25: Payroll Tax Rates Around the World



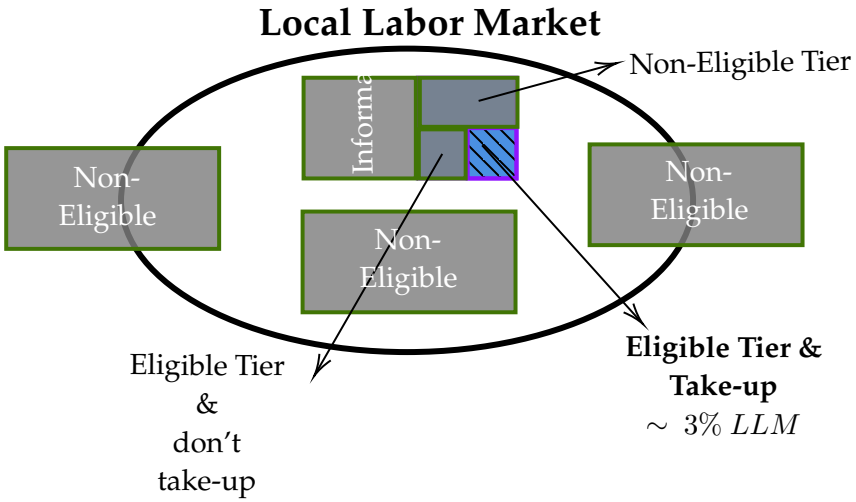

*Note:* This figure reports payroll tax rates around the world. The payroll tax rate is composed by the sum of employer and employee's contributions.
*Source:* Elaborated by author, based on information from OECD 2019.

Figure 26: Firm vs Market Level Shock



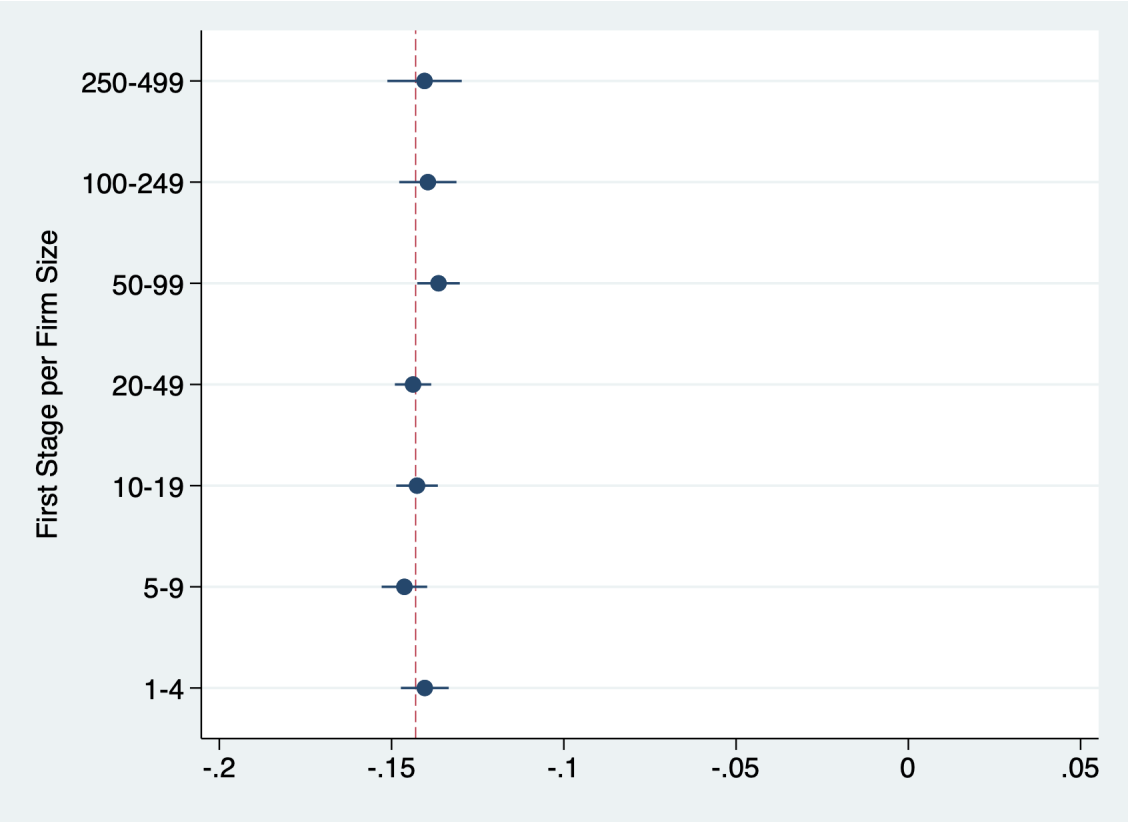

*Note:* This figure provides additional test on the spillover effect. It compares the worker's earnings effect for high and low intensitively treated markets. To measure market treatment intensity I compute the share of treated workers in each labor market, which are defined by the occupation x region cells. Then it separately estimates equations 7 and 8, 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 option 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 27: Illustration of Treatment Coverage in LLM



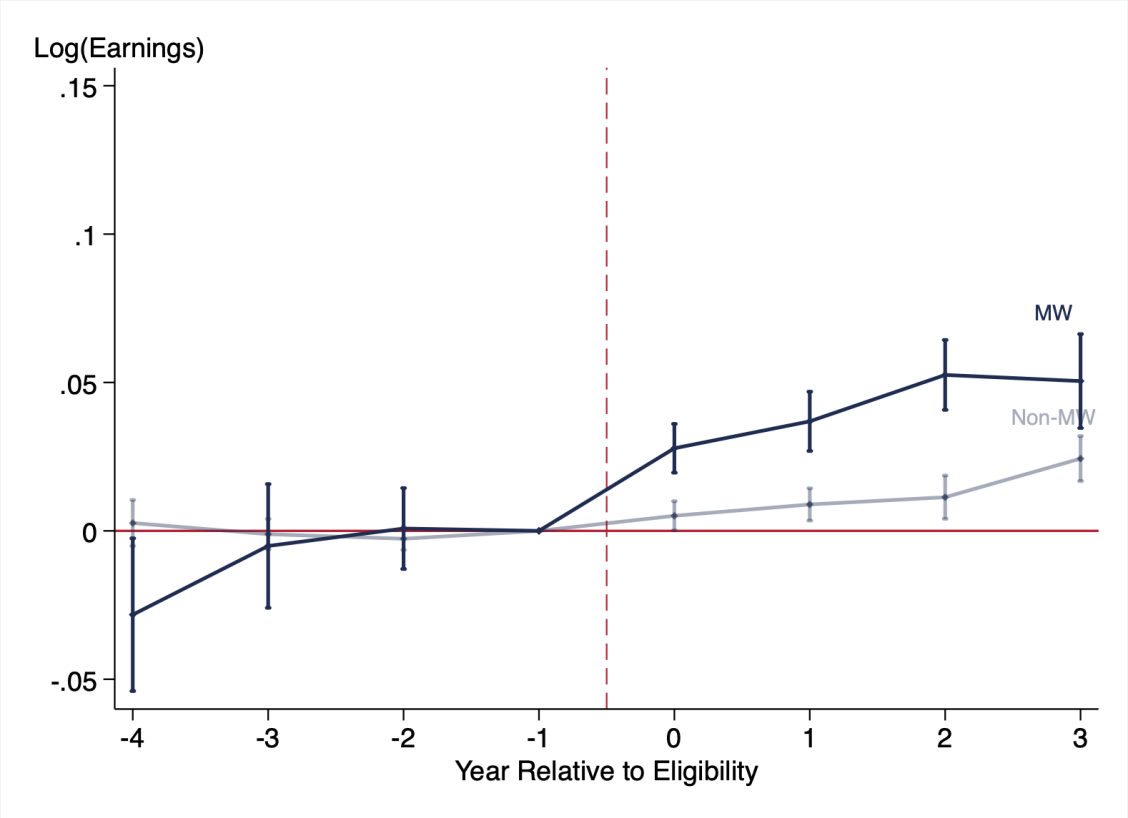

*Note:* This figure provides illustration of the policy coverage within local labor markets (LLM), which are defined as occupation x region cells according to job switching patterns. The figure shows that within LLM there are eligible and non-eligible sectors. In eligible sectors, there is ineligibility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

Figure 28: First Stage per Firm Size



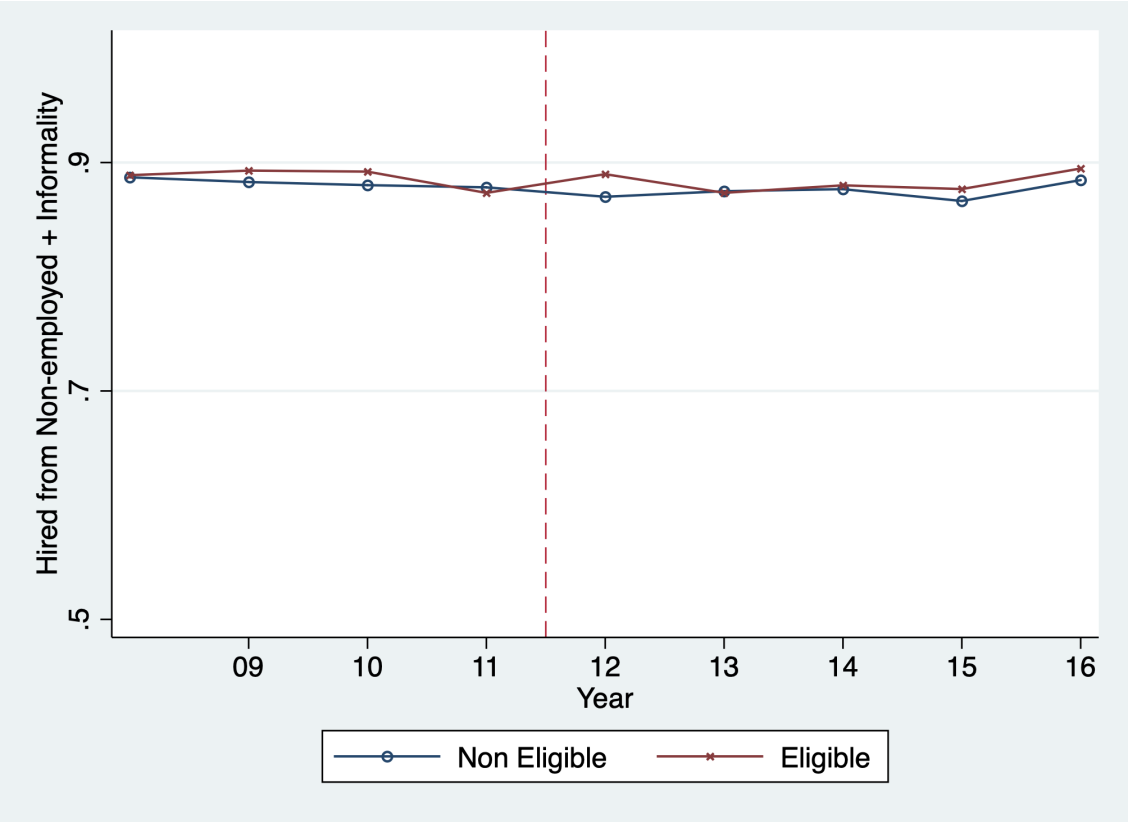

*Note:* This figure presents the difference-in-differences IV estimate for the effect of the tax treatment on the log of labor cost. Firms are categorized on size bins based on pre-reform levels. It shows that the effect of the reform on labor cost is similar for all size bins. Standard errors are conservatively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

Figure 29: Earnings Effect for MW Workers



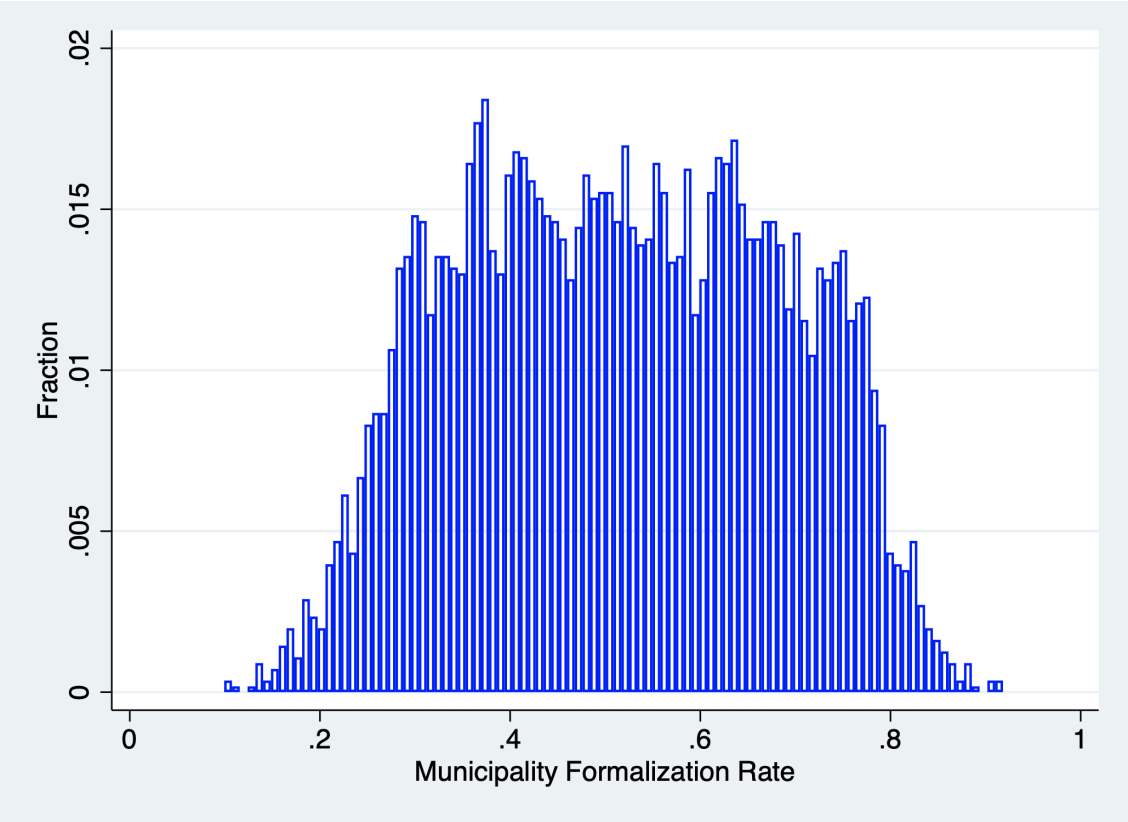

*Note:* This figure presents the event study estimates for workers below and above the minimum wage constraint in the pre-period. The blue plot reports the pass-trough for workers that were constrained by the minimum wage in the pre-reform years. The plots show the average earnings effect for workers that were employed for at least three years in the pre-reform period. I normalize the results with respect to one year prior to the treatment event. The analysis spans four years prior to entering the payroll tax cut program and three years after. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Figure 30: New Hires Origin by Eligibility Status



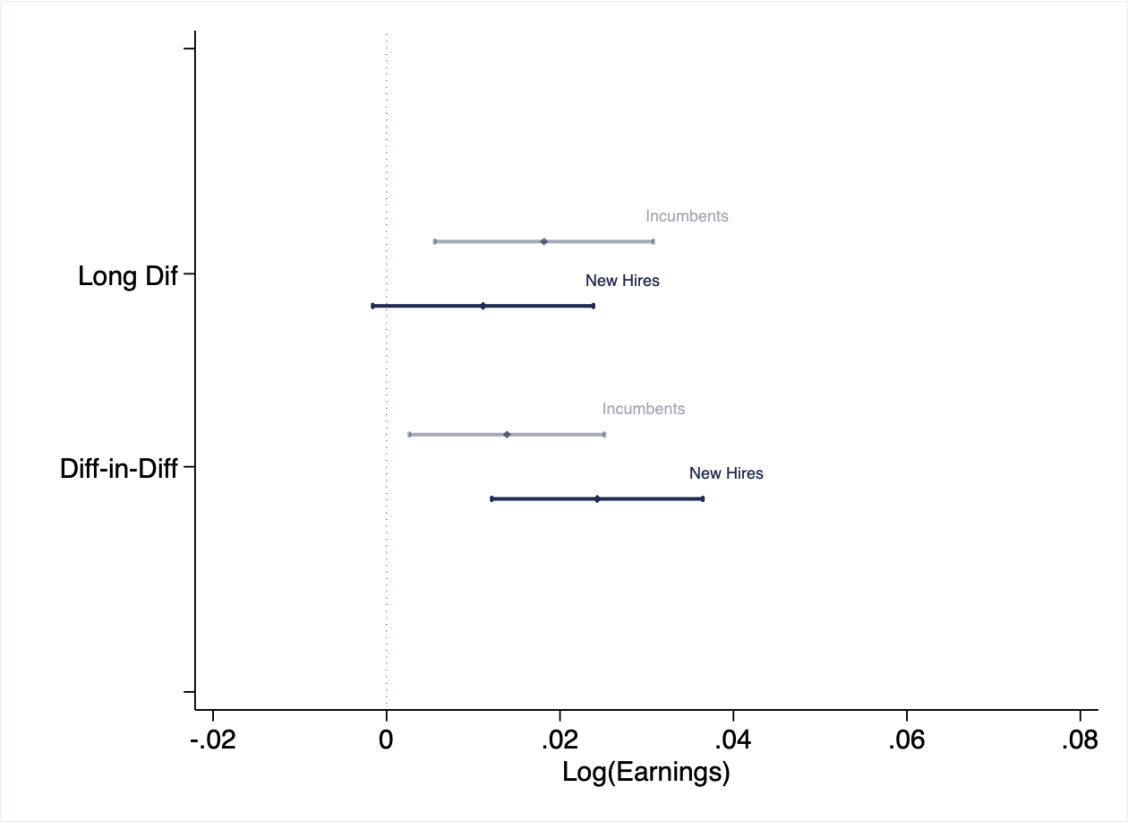

*Note:* This figure plots the share of new hires coming from non-employment or informality. A new hire is classified as previously informal or non-employed if she was not holding a formal job in the three months prior to being hired. Eligibility is defined based on the sector of employment.

Figure 31: 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 32: 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 incumbent's response. Standard errors are conservatively clustered at the 5-digit industry-by-state level.

Table 22: 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 existing at least one eligible sector on the given LLM. Row (2) reports worker's average formality rate in Brazil; row (3) reports the share of firms in the eligile 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 23: 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 24: 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.