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The payroll tax exemption in Brazil: Structural and quasi-experimental perspectives*

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Contents

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policy evaluation, payroll tax exemption, quasi-experiment, structural models

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E61, J08, L52

Abstract · Resumo

This paper estimates the payroll tax exemption effects by two alternative approaches. First, a Quasi-experiment which takes into account the fact that firms under the tax regime called *Simples* already do not pay payroll taxes to define such group of firms as control. The other approach was a structural model which considers payroll tax as a source of market imperfection, whose effects can be estimated from production function coefficients. Results suggest that Quasi-Experimental methodology overestimated policy effects and the conclusion is that this policy had reduced effects.

1. Introduction

There has been an intense discussion about the best approach to evaluate public policies; some authors such as Angrist and Pischke (2010) announced the beginnings of a “Credibility Revolution in Economics”, based on Quasi-Experimental

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or Randomized Control Trials (RCTs), which are already widespread in medical sciences. Even though RCTs have their merits, not all public policies are amenable to be evaluated using such techniques. Nevo and Whinston (2010) pointed out that in Empirical Industrial Organization sometimes the focus is on "external validity", and a structural model would be a more fruitful approach.

Such discussion is especially useful in analysis of policies like the payroll tax exemption for Brazilian industries, which will be the subject of the present paper. The policy changed the payroll tax payment by a tax charged on company sales, leading to a decrease in the firms' total tax bill.

The payroll tax exemption was enacted in the beginning of 2012 by changes in labor tax law, which benefited four industries at first. From 2012 to 2014, another five changes in the labor tax laws increased the number of benefited industries to forty-five. In 2015 it became optional for all benefited industries and in 2017 the policy ended. However, payroll taxes further reduction or exemption are still discussed by policy makers, which makes it important to understand the policy effects.

This program was large enough to have important effects on the funding of Brazilian Social Security system. Some estimates from Brazilian Agency of Industrial Development (ABID)[1] put the losses at about 20 billion BRL for the two first years of the policy and at about 90 billion BRL from 2013 to 2017. These large estimated costs coupled with the absence of any prior analysis of the program effectiveness make this study especially relevant.

So far, three most relevant studies carry out a RCT analysis by using difference-in-difference methodology: Dallava (2014), Scherer (2015) and Garcia, Sachsida, and Carvalho (2018). Even though those researchers use similar methodologies, different identification strategies lead to divergent results. While Scherer (2015) estimated positive and significant policy effects, Dallava (2014) and Garcia et al. (2018) found no significant effects.

In this research, some of the RCT assumptions and results—especially those of Scherer (2015)—are reviewed in the light of a structural model based on the methodology developed by Petrin and Sivadasan (2013). The authors define the difference between the value of marginal product of a factor and its marginal cost, which they call the gap, a distortion that moves the market from competitive equilibrium. Therefore, a policy such as the payroll tax exemption can be analyzed as a decrease in the gap for the industrial labor market equilibrium, and this decrease in market imperfections allows identification of likely policy effects.

In what follows, section 2 describes the economic scenario that impelled the payroll tax exemption implementation and presents the most important policy facts. In section 3, there is a brief discussion about alternative policy evaluation

[1] *Relatório de medidas sistêmicas*, 2014, available at https://old.abdi.com.br/

methodologies and their relationship with this specific case. Section 3 also presents the RCT strategy, market imperfection estimation methodology and the dataset used. Section 4 briefly discusses the production function estimation, which is the first and central step for market imperfection estimation. Section 5 describes simulation procedures and presents the results. Section 6 concludes this research.

2. Economic scenario and policy rationale

Concerns about Brazilian industry performance were widespread among policymakers for quite a few years before 2012. Data from Applied Economic Research Institute (IPEA)[2] shows that between 2000 and 2013 the industrial sector has grown less than the rest of the economy; while the whole economy has grown 3.36% per year—and agriculture and services have grown at 3.71% and 3.42% per year respectively—, industrial sector has grown only 2.60% per year. This smaller growth rate resulted in the industrial share on economy decreasing.

Brazilian Ministry of Industry, Foreign Trade and Services (MDIC) data[3] shows that in 2014 the manufacturing sector had its worst result on balance of trade, when imports have exceeded exports in US$ 109 billion. In addition, according to the National Industries Confederation (CNI) data presented by Werneck (2012), the ratio of imports and consumption of manufactured goods in Brazilian economy increased from 11.6% to 20.7% between 2000 and 2011, in current prices, and from 17.0% to 18.5% in constant prices. According to Werneck (2012), stagnation in industrial performance was one of the most harmful problems in Brazilian economy, made worse by the macroeconomic scenario in past years. The author argues the agricultural increasing exports overvalued Brazilian currency, stimulating industrial goods imports and increasing the sector's deficits. Besides an overvalued currency, there were also microeconomic distortions on Brazilian economy, just as in other Latin American countries. These misallocations were so severe that, according to Busso, Madrigal, and Pagés (2013), their elimination would increase Latin America manufacturing productivity by a factor between 45% and 127% if eliminated. Lewis (2005) considered the huge government's participation on Brazilian economy—measured by high taxes—an important reason for this reduced productivity. Similarly, Werneck (2012) pointed out the share of taxes on Brazilian GDP increased from 24% to 36% between 1990 and 2010. For Oliveira (2011), the literature usually considers high social contributions and labor market rigidity as important sources of Brazilian industry lack of competitiveness.

The effects of industrial policies such as changes in payroll tax policies are controversial, as pointed out by the survey of Coronel, Azevedo, and Campos (2014):

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[2] Available at http://www.ipeadata.gov.br

[3] Available at http://www.mdic.gov.br/balanca/mes/2016/BCE004A.xls

part of the literature is critical, because they generate imbalances; other authors consider that such polices can reduce the damage caused by adverse economic scenarios and, therefore, the government should protect industry using instruments such as credit expansion, subsides and tax reductions. Drawing from this positive assessment of industrial policies, the payroll tax exemption was enacted as a part of the "Programa Brasil Maior" (PBM), created to stimulate Brazilian industry competitiveness.

The payroll tax exemption substitutes payroll tax for a sales tax, reducing in most cases the total tax burden. The payroll tax change had three main objectives: reduce producer costs, increase employment and increase the share of labor force formally employed. Therefore, this policy assumes high labor taxes reduce Brazilian competitiveness in international markets, therefore reducing labor demand, exports, employment and production.

Discussions on how best to increase formal employment in Brazil has been going since the mid-nineties, with the acknowledgment of its effects on Social Security funding. In 1995, labor benefits costs surpassed tax revenues, which made this problem worse according to Neri (2003). Some authors, such as Neri (2003) and Bordonaro (2003), considered a reduction in labor taxes as an way to increase the size of formal labor market, and this kind of rationale led to two constitutional amendments, one in 1998 and other in 2003. The most important policy acts related to payroll tax exemptions are presented in Table 1.

These constitutional amendments changed the article 195 of the National Constitution, which regulates financing of Social Security System. The 1998 constitutional amendment allowed different tax collections according to firms' economic activity and to employment used in production. A constitutional amendment of 2003 allowed to substitute payroll tax collection by a sales revenue tax. In 2008, a constitutional amendment project was discussed in Brazilian Congress, and lead to a series of changes in labor tax law, enacted from 2012 to 2014.

Sometimes the change had an opposite effect from what is expected, by increasing the total tax burden. When firms had a small workers by revenue rate, the benefit from labor tax reduction by shifting the tax burden from labor to sales might be too small to compensate the increasing in tax charged on sales.[4] Even though these cases were discussed in Brazilian National Congress, the policy was mandatory at first, but in 2015, the Law 13161/2015 made this tax system optional. In March of 2017, the constitutional amendment 774/2017 ended this policy to manufacturing sector, maintaining the payroll tax exemption only for transportation, civil construction and communication sectors.

[4] An example of the debate about economic viability of the policy considering the firm's perspective is on the newspaper "O Estado de S.Paulo" of February 2nd of 2012 titled "Payroll tax exemption raises the total of tax paid by the firm in some sectors".

Table 1. Summary of the public acts concerning the payroll tax exemption.

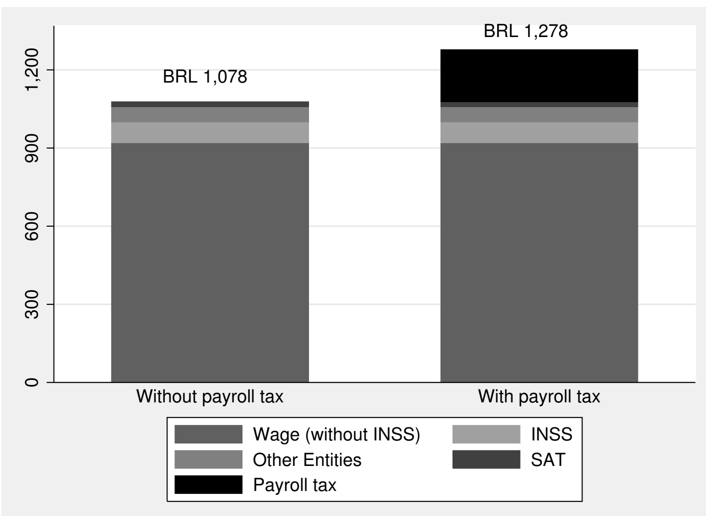
Public acts	Description
Constitutional amendment 20/1998; included paragraph 9 in article 195.	Allowed the payroll tax base to change by to economic sector and labor usage intensity.
Constitutional amendment 41/2003; included paragraphs 12 and 13 in article 195.	Allowed partial or total substitution of the payroll tax by specific taxes on revenue or earnings.
Provisory act 540/2011.	Replaced the payroll tax by a tax on sales revenue (2.5% for IT firms and 1.5 for furniture, apparel and leather articles firms).
Provisory act 563/2012	Included other industries in the payroll tax exemption policy (11 additional sectors).
Provisory act 582/2012; Provisory act 601/2012; Provisory act 612/2013; Provisory act 651/2014.	Included other industries in the payroll tax exemption policy (30 more). The industries, divided in two groups according to the tax charged on the sales revenue, are: 1%:textiles; apparel; leather and footwear; furniture; plastics; electrical equipment; auto parts; buses; shipbuilding; aircraft; mechanical capital goods; poultry; pork and derivatives; fish; breads and pastas; drugs and medicines; medical and dental equipment; bicycles; tires and tubes; pulp and paper; glasses; stoves; refrigerators and washing machines; ceramics; stones and ornamental stones; paints and varnishes; hardware; railway equipment; tools; forged steel; screws; nuts and drawn; toys; optical instruments; maintenance and repair of aircraft; air, sea and rive transports; department and magazines stores; and retail trade; 2%: call center; TI; informatics technical support; design houses; hotel; civil construction; road public transport. *
Law 13.161/2015	Made the payroll tax exemption optional for all benefited industries.
Constitutional amendment 774/2017	Ended the payroll tax exemption to manufacturing sector, maintaining it only for transportation, civil construction and communication sectors.

Note: *These sectors are not exactly the same included in the databases of PIA because of some differences in the industrial classification.

Source: Paiva and Ansiliero (2009), PBM Report and Brazilian Federal Revenue Service.

Considering only labor costs, the policy indeed reduces labor tax burden. Figure 1 shows the payroll tax exemption effect for a hypothetical monthly wage of BRL 1,000. In Brazil, a worker who receives a monthly wage of BRL 1,000 pays 8% of his or her wage to the National Social Security Institute (INSS), for a net receipt of BRL 920. The other taxes are paid by firms and are calculated as percentages of monthly wage: the payroll tax is 20% (BRL 200); "other entities" 5.8% (BRL 58); and "accidents at work insurance" (SAT) 2% more (BRL 20).[5] These costs sum 27.8% of the wage, for a gross wage bill of BRL 1.278 in this example. According to the proposed changes, the same worker would cost BRL 1.078 to the company, a reduction of 71.9% of the labor taxes, and by 15.6% of the total wage cost of a formal laborer.

Before payroll tax reduction implementation, some studies aimed to discuss its likely effectiveness. Luchiezi (2011) concluded economic growth, interest rate and credit stimulate formal hiring more than tax reductions. The author also considers this policy as a way to increase companies' profits without much change in employment level. Paiva and Ansiliero (2009) also criticize this policy, considering it distorts the Social Security System.



Source: Labor legislation and authors' calculations.

Figure 1. Total Labor Costs associated with a net wage of 1000 BRL.

[5]Some specificities can change the total amount of taxes paid by the firm. The tax for accident at work insurance (SAT), for instance, can vary from 1% to 3% according to the hazard characteristics of the specific work that the employee is hired for, and according to the companies' investments on training and on security. There are also some additions in the base wage paid by the firm according to the dangerousness related to the specific work. However, the example illustrates most of the cases for industrial companies considered in this research.

On the other hand, Ulyssea and Reis (2006) pointed out some evidences that labor costs affect formal labor hiring, and therefore reductions in labor taxes boost employment. Bordonaro (2003) analyzed data from nine Latin American countries, from 1980 to 2000, trying to identify formal employment causes, and found that reductions in labor costs improve formal hiring, even though economic growth has a greater impact. Fernandes, Gremaud, and Narita (2004) developed a general equilibrium model considering various changes in taxation to calculate employment and production effects. Their estimations showed that substituting labor taxes to consumption taxes potentially increases production by 6.5% and employment by 2.0%.

Looking at the relationship between labor demand and labor regulations, Barros and Corseuil (2004) tried to identify the effects of changes in labor legislation. Even though firing costs increased almost four times during the period analyzed by the authors, no significant change on estimated labor demand curves where observed.

More recently, three studies aimed to evaluate specifically the payroll tax exemption by using RCTs approach after its implementation: Dallava (2014), Scherer (2015) and Garcia et al. (2018). The first two studies evaluated the policy effects for textiles, garment and leather and shoes industries, which were the three manufacturing industries benefited by the policy before its expansions in 2012 and 2014. Garcia et al. (2018) estimated the effects for all industries affected by the policy from 2012 to 2017. Meanwhile Dallava (2014) and Garcia et al. (2018) did not find any significant effect, Scherer (2015) estimated an increasing of 14.4% for fixed effects and 13.8% for difference-in-difference regressions.

Such a large disparity draws attention to its study identification strategy, which will be analyzed closely on this research. These estimates around 14% calculated by Scherer (2015) were estimated considering only companies with less than 50 employees, which is more suitable for this study identification strategy according to the author. However, considering firms of all sizes, the estimated effect reduces to 4.6% for fixed effects regression. In this regard, both Dallava (2014) and Garcia et al. (2018) did not consider only small firms on their sample used to estimate policy effects.

3. Methodology

Regarding the credibility of public policy evaluation, the Journal of Economic Perspectives published in 2010 three articles whose main objective was to discuss the tradeoffs between RTCs methodologies and structural models. The discussion was based on the classic work of Leamer (1983), who pointed out the lack of credibility in empirical analyses. Angrist and Pischke (2010) classified the changes in this research field since then as a "credibility revolution". Nevo and Whinston (2010) summarized the discussion considering much of this "credibility revolution" was related to improvements in quality and in availability of databases. The development of several new methodological frameworks, more complex and able to deal with a large number of cases, is also pointed out and the better understanding of the relationship between economic theory and applied research also played an important role. They concluded that considering the wide range of alternative methodologies, the choice among them relies mainly on specifics and, therefore, there is no reason to rely more on experimental analysis or on structural analysis. However, the authors also pointed out to the importance of external validity, that means, whether the results are valid for different contexts or even different samples than the ones considered on RCTs models, which may be the reason for the divergence among the studies which implemented such framework to evaluate the payroll tax exemption.

There are many factors to control in empirical analysis which affect industry performance and make difficult policy effects identification. Macroeconomic fluctuations, changes in international markets, or changes in input markets can affect industrial performance and lead to biases in estimated policy effects. Therefore, it is necessary to evaluate the effects of the policy on a group of firms by comparing its effects with what would have happened to this same group of firms if they were not benefited by the policy. Since it is impossible to observe it, the researcher must create a group of firms very similar to those which were benefited by the policy, but ended up not being affected by it.

In this section, our empirical strategy is based on Scherer (2015) in order to find out the relationship between the assumptions made by the author and his policy estimated effects. The author noticed that firms under the tax regime called *Simples* already did not pay the payroll tax before the policy. Therefore, firms from the same sector which were under *Simples* regime and had similar characteristics to firms benefited by the payroll tax exemption were considered as proxies to the behavior of the firms that were affected by the payroll tax exemption, i.e. firms under *Simples* are the control group and firms not under such tax regime and affected by the policy are the treatment group.

Accordingly, the regression used to estimate the policy effect is as follows:

$$Y_{it} = \beta_0 + \delta_0 T_t + \beta_1 treated_{it} + \beta_2 taxchg_{it} + \beta_3 ed_{it} + a_i + u_{it}\,, \tag{1}$$

where Y_{it} is the number of employees; T_{it} is a dummy for time (being one for 2012); $treated_{it}$ is the treatment (being one for firms not under *Simples* in 2012); $taxchg_{it}$ is a dummy for firms which changed its tax regime from *Simples* to *non-Simples* or vice versa; ed_{it} is the share of workforce with education level equal or above the intermediary level. This equation is estimated by Fixed Effects and by difference-in-difference. A non-parametric estimation of difference-in-difference indicates the changes in averages for outcome variables across groups and time.

Since *Simples* is a tax regime applied for smaller firms, Scherer (2015) considers that the suitable control group is composed only by firms with less than 50 employees, which will also be the case in this study for the baseline estimations. In order to investigate different firm sizes influence on policy effects estimations, regressions will also be implemented considering only firms with 30 to 50 employees and the whole sample.

Regarding the structural model approach, according to Petrin and Sivadasan (2013), the difference between the value of marginal product of a factor and its marginal cost, called gap, is a result of anything that moves the economy away from neoclassical equilibrium in which they are equal, such as mark ups, firing and hiring costs, capital adjustment costs, taxes and subsides. Therefore, the payroll tax exemption is a potential decrease in the gap on industrial labor market.

The gap is defined by Petrin and Sivadasan (2013) as follows:

$$G_{it}^{l} = |VMP_{it}^{l} - w_{it}|, \tag{2}$$

where G_{it}^{l} is the gap on labor market; VMP_{it}^{l} is the value marginal product of labor; and w_{it} is wage. The gaps are calculated in Petrin and Sivadasan (2013) for two types of work: blue collar (unskilled employees) and white collar (skilled employees).

Applying equation (2) to Chilean manufactory sector between 1980 and 1990, Petrin and Sivadasan (2013) identified an increase in the labor market gaps. The authors considered that this increase was associated with changes in Chilean labor market regulation, which increased hiring and firing costs, as well as bureaucratic costs.

Taking this approach to data, the first step to estimate the gap is to calculate the value of marginal product of labor (VMP_{it}^{l}). Considering a Cobb–Douglas production function, the VMP_{it}^{l} is estimated as follows:

$$VMP_{it}^{l} = \beta_l \frac{Y_{it}}{L_{it}}, \tag{3}$$

where Y_{it} is output; L_{it} is labor; and β_l is its marginal effect on output, calculated by the estimated production function.

To simulate the policy effects on employment, Y_{it} was replaced in expression (3) by the Cobb–Douglas production function in order to have the value of marginal product of labor (VMP_{it}^{l}) in terms of production function inputs. By substituting this relationship into gap definition in expression (2) and rearranging terms, the quantity of labor used in production can be expressed in terms of other inputs, their coefficients, wage and gap as follows:

$$LB_{it} = \mathbb{1}_{\{0<W_{it}^{lb} \le VMP_{it}^{lb}\}} \left(\frac{\beta_{lb} Y_{it}^{*}}{W_{it}^{lb} + G_{it}^{lb}} \right)^{\frac{1}{1-\beta_{lb}}} + \mathbb{1}_{\{0<VMP_{it}^{lb}<W_{it}^{lb}\}} \left(\frac{\beta_{lb} Y_{it}^{*}}{W_{it}^{lb} - G_{it}^{lb}} \right)^{\frac{1}{1-\beta_{lb}}}, \tag{4}$$

where $\mathbb{1}$ is the indicator function; LB is blue collar employees; and

$$Y^* = LW^{\beta_{lw}} K^{\beta_k} M^{\beta_m} E^{\beta_e} S^{\beta_S} e^{\omega_{it}+\epsilon_{it}},$$

where LW is white collar employees, K is capital, E is electricity, S is hired services; β are factor production function estimated coefficients. Finally, ω_{it} and ϵ_{it} represent productivity shocks from estimated productions function.

Before proceeding to data analysis it is important to state this approach main limitation. The present extension of Petrin and Sivadasan (2013) methodology applied in this paper does not take into account dynamic factors usually considered on labor market models, such as intertemporal considerations on hiring and firing workers or search frictions. Araujo (2015) is a good example of such approach applied to analyze wage inequality and job stability considering dynamic impacts of firing costs. Therefore, there may be dynamic effects of changes on gaps which are not accounted by the present methodology. A dynamic version of this model and its application to payroll tax evaluation is a natural extension of this research. [6]

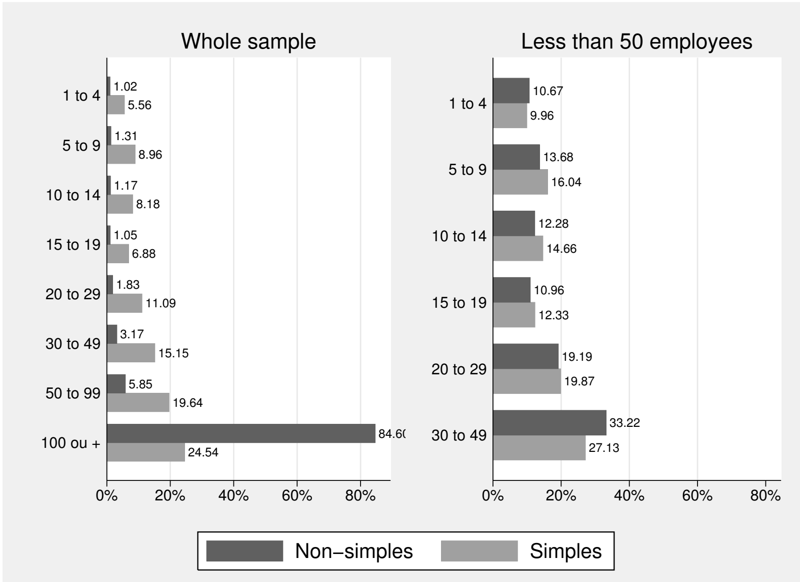
3.1 Database

In order to evaluate the payroll tax exemption effects by using the RCT methodology, the variables used are from the Annual Social Information Report (RAIS). The data are provided annually and contain information about the firm size, location, firms sector, tax regime and employees characteristics.

A panel data set of existing firms in 2010, 2011 and 2012 was used. The variables used in this research were the identification of the firms, firms economic sector according to the National Economic Activities Classification (CNAE), tax regime and proportion of employees with schooling level equal or above intermediary level.

Figure 2 shows the frequency of employees which work for companies grouped by the number of hired workers separated between firms under *Simples* regime and not under *Simples* regime. For the whole sample, for instance, 84.60% of employees hired by firms not under *Simples* work for companies with one hundred or more employees, meanwhile for companies under *Simples* only 24.54% of employees work in companies of such group. On the other hand, 5.56% of employees hired by firms under *Simples* work for companies with one to four employees, while only 1.02% of employees hired by firms not under *Simples* work for such group of firms. This means that *Simples* is a tax regime which is adopted mainly by smaller companies. Since the identification strategy separates firms under *Simples* as control group and firms not under *Simples* as treatment groups, the observed discrepancy between the

[6] Araujo (2015) applies a dynamic model with severance payment and firing costs and finds out that raising firing cost may have diverse impacts depending on worker wage level: the lower the wage, the higher is the impact of increasing firing costs on salary, which implies that increasing firing costs contributes to worsen wage inequality.



Source: RAIS.

Figure 2. Share of employment hired by firms under *Simples* and non-*Simples* grouped by number of employees per firm – 2011.

groups for the whole sample suggests that such strategy may not be suitable for the whole sample.

Figure 2 also shows that the distribution between groups of firms is much more similar between *Simples* and non-*Simples* firms considering only firms with less than 50 workers. Non-*Simples* group still have more employees on larger companies, but the discrepancy between groups is much lower in this sample. It means that to consider firms with less than 50 employees makes treatment and control groups much more similar, which endorses such identification strategy. However, one can question whether the estimated effects calculated by this methodology is valid for firms of all sizes. That is, by selecting only part of the sample, the estimated effect may be specific for a particular kind of firms and not have external validity.

Taking into account only firms with less than fifty employees in order to classify firms on treatment and control groups, Table 2 points out some similarity between the groups by showing their descriptive statistics.

The average number of employees per firm—which is the variable used to estimate the policy effect—is very close between treatment and control groups. This similarity is expected since treatment and control groups consider both only firms with less than 50 employees, which makes the employment distribution among firms similar between the groups as Figure 2 showed. The average hourly wage, however, is higher for treatment group, which may be related to the higher share of employees

Table 2. Descriptive Statistics – treatment and control groups in 2011.

	Treatment		Control	
	Mean	Std. Dev.	Mean	Std. Dev.
Average number of employees	10.54	11.55	10.29	10.49
Average hourly wage (BRL)	7.03	8.20	4.25	2.01
Sex (male = 1)	0.54	0.36	0.35	0.34
Average age (years)	34.40	8.30	33.55	7.72
Share (schooling)	0.62	0.39	0.55	0.37
Share of young employees	0.22	0.26	0.27	0.27
Share of adult male employees	0.41	0.34	0.23	0.28

Source: RAIS.

with education level equal or above intermediary on treatment group. The share of male workers is also higher in treatment group.

Regarding the structural model, all variables are from the Annual Industrial Research – Enterprise (PIA–Empresa), which is developed and published by the Brazilian Institute of Geography and Statistics (IBGE). Table 3 contains variable definitions.

All variables from "PIA–Empresa" were recorded at firm level. IBGE makes public only industry level data, but data at firm level can be used after IBGE's authorization and under IBGE's supervision due to data security reasons.

The 2011 firms' assets were used as a proxy for capital stock. The 2010 capital values were calculated subtracting the investments and adding back the depreciation from 2011 values, following the perpetual capital stock method; this procedure was applied to construct the capital stock series for all years. This procedure was used because capital stock values for the years before 2000 were not collected. The number of employees in December 31st was used as labor quantity and their total compensation was used as wage measure. Raw material purchases were used as intermediate consumption. Electric energy and fuel purchases were used as purchased electricity and outsourcing expenditures as hired services. The revenues from sales of industrial products were used as output measure.

All values were adjusted for inflation when suitable. The price index used to deflate the capital stock series is the "implicit deflator of capital formation", published by IBGE through the Nacional System Accounting. For other series, the index was the "wholesale price index", published by Getulio Vargas Foundation (FGV). All monetary variables were expressed in 2011 BRL.

The period analyzed is from 1996 to 2011. The firms were grouped in industries according to the National Economic Activities Classification (CNAE). Industries with less than 1,000 observations during these 16 years were not considered. This

Table 3. Variables used.

Constructed variables	Available series in PIA
Sales Revenue	Revenue from industrial products sales.
Investment	Acquisition and improvements on: assets, land, buildings, machinery and equipment, transport and others.
Depreciation	Write off of land and buildings, machinery and equipment, transport and other means.
Capital	Total assets.
Intermediate materials	Purchases of raw and auxiliary materials.
Purchased energy	Electric energy and fuel purchase.
Hired services	Outsourcing.
Labor – blue collar	Number of employees active on December 31rd involved with industrial activities.
Wages – blue collar	Salaries, withdrawals and other rewards of blue collar employees.
Labor – white collar	Number of employees active on December 31rd and not involved with industrial activities.
Wages – white collar	Salaries, withdrawals and other rewards of blue collar employees.

Source: Annual Industrial Research – Enterprise (IBGE).

procedure resulted in 183,940 observations, grouped in 52 industries. Among these 52 industries, 4 presented problems on capital series construction. Therefore, the estimation sample included 48 industries. From this set of industries, 9 presented econometric problems on production function estimation, such as negative coefficients, identification problems or serial correlation. Therefore, the gap was estimated for 39 industries. From these 39 industries, 31 were benefited by the payroll tax exemption and 8 were not. In order to analyze the policy and to compare its results with those of the RCT approach, the estimation of the policy results at first were considered only for six industries, which are classified in three groups: Textile, Garment, and Leather and shoes.[7]

[7] Scherer (2015) estimate the effects of the policy also for services, which were also benefited by the policy in 2012. However, the production function estimation is not suitable for these industries.

4. Production Function Estimation

The functional form considered was the Cobb–Douglas production function with production factors as such:

$$y_{it} = \beta_k k_{it} + \beta_{lb} lb_{it} + \beta_{lw} lw_{it} + \beta_m m_{it} + \beta_e e_{it} + \beta_s s_{it} + \omega_{it} + \epsilon_{it}\,, \tag{5}$$

where lower case indicates the natural logarithms, i represents firms, and t years; y_{it} is sales revenue; k_{it} is capital; lb_{it} and lw_{it} are number of unskilled employees (blue collar) and skilled employees (white collar) respectively; m_{it} is value of intermediate materials; e_{it} is value of purchased electricity; and s_{it} is value of hired services. The terms ω_{it} and ϵ_{it} represent productivity shocks, where the first is a transmitted component, and the second is an iid (unexpected) productivity shock.

There are several potential biases in Ordinary Least Squares (OLS) estimation of such a production function. The econometric literature has focused mainly on simultaneity bias, which led to several alternative methodologies. Table 4 contains a summary of alternative estimators that aim to correct the simultaneity bias, as well as a brief description of the assumptions made and the most important references for each estimator.

The main source of simultaneity bias in production function estimation is the relationship among firms' input choices and productivity shocks observed by firms, but not by the econometrician. In empirical applications, this problem usually leads to an upward bias on labor coefficient and to a downward bias on capital coefficient (Van Beveren, 2012, p.5).

Assuming each firm relates productivity expectations and input usage in the same way over the years, this could be solved using the fixed effects estimator (FE). In fact, Van Beveren (2012) pointed out that the FE estimator was introduced in Economics by Mundlak (1961) in order to correct the simultaneity bias of production functions. However, empirical practice shows this assumption is not valid, and the FE estimation procedures has shown little advantage over the OLS to correct the simultaneity bias. Besides, for the series which does not show much variability across time and have measurement error problems, fixed effects estimates are plagued by attenuation bias.

An alternative is to consider some persistence in productivity shocks (usually an AR(1) process), which results in a dynamic specification for the production function. However, as pointed out by Cameron and Trivedi (2005), the dynamic is not free of simultaneity bias, requiring instrumental variables. Arellano and Bond (1991) developed an estimator that uses lagged values of variables as instruments in a Generalized Method of Moments (GMM) estimator. However, Griliches and Mairesse (1995) pointed out that Arellano and Bond (1991) instruments are valid but weak in this case, not helping with the downward bias on capital coefficient estimates, even though the labor parameters problem is solved. Besides, another

Table 4. Summary of assumptions and selected references for produnction function estimation approaches.

Estimator	Assumptions	References
Fixed Efects	Productivity is plant-specific, but time-invariant.	Mundlak (1961); Van Beveren (2012).
GMM	Productivity has a persistent component that leads to a dynamic specification; the variables past values are not related to the contemporary productivity but are related with the contemporary variables values.	Anderson and Hsiao (1981); Arellano and Bond (1991) ; Bond and Blundell (2000).
System GMM	Besides GMM assumptions, it assumes that the variables' lagged difference are not related with the productivity level.	Bond and Blundell (2000).
Olley and Pakes (OP)	Invertibility condition: investment should be strictly increasing in productivity.	Olley and Pakes (1996); Ackerberg et al. (2006).
Levinsohn and Petrin (LP)	Invertibility condition: material consumption should be strictly increasing in productivity.	Levinsohn and Petrin (2003).
Wooldridge	Besides the invertibility condition of the material consumption, it is considered the labor use is possibly affected by the productivity shocks.	Ackerberg et al. (2006); Wooldridge (2009).

Source: Elaborated by the authors based on Van Beveren (2012).

issue commonly observed in GMM production function estimators is a too low returns to scale estimate.

According to Bond and Blundell (2000), the weak instruments problem can be solved by increasing the information set used in estimation, which can be done by the use of suitably lagged first differences of the variables as instruments for the equations in levels. Both sets of moment conditions can be exploited as a linear GMM estimator in a system containing both first-differenced and levels equation. Combining both sets of moment conditions provides the System GMM (SGMM) estimator. The authors show that the SGMM corrects the capital downward bias and lead to a more reliable returns to scale estimates (Bond & Blundell, 2000, p.7 and 13).

The alternative way to treat simultaneity bias without considering a dynamic specification is by considering the firms' decisions about some other variable as a function of productivity shocks and try to use this variable as a proxy for the unobserved productivity. Olley and Pakes (1996) originally developed this methodology relating productivity and investment decisions, assuming a monotonically relationship between them. With these assumptions in hand, it is possible to specify the productivity in terms of investment and capital by a polynomial approximation. However, investments data might not be available in a way to help estimation. Considering this problem, Levinsohn and Petrin (2003) developed a very similar methodology using intermediate materials instead of investments as instrumental variables.

In both cases Olley and Pakes (1996) and Levinsohn and Petrin (2003), the estimation has two steps: in the first step the labor coefficients are estimated; in the second step the other coefficients are estimated non-parametrically. However, Ackerberg et al. (2006) pointed out the risk of collinearity in this procedure and, in order to avoid econometric problems, suggested estimation of labor coefficients in the second step also. Wooldridge (2009) developed a GMM estimator that takes into account the considerations of Ackerberg et al. (2006).

Since the literature does not have reached any consensus on which estimator is better in any econometric sense, we compared the results of five different estimators: Fixed Effects, GMM, System GMM (SGMM), Levinsohn and Petrin (LP) and Wooldridge. Figure 3 presents a kernel density histogram for the estimated parameters of the six inputs in equation (5).[8]

There is no expected bias on the coefficients for intermediate materials, purchased energy and hired services because most of the econometric considerations about simultaneity bias on production function estimation consider only labor and capital as inputs. Nonetheless, Petrin and Sivadasan (2013) used the same specification and use same estimators as here; therefore, their results can guide our analysis.

The average estimated coefficients for these three inputs obtained by Wooldridge's methodology are very close to the ones presented by Petrin and Sivadasan (2013), albeit using a different dataset. The intermediate materials coefficients estimated by Wooldridge's methodology are more widespread than the ones estimated by Petrin and Sivadasan (2013), which are more similar to the LP estimates.

Considering all methods, there is no big difference in estimates using different estimators. For hired services, the FE, GMM and SGMM coefficients are lower in average and have lower dispersion. The distribution of capital coefficient estimates is in accordance with the literature. Both FE and GMM coefficients are close to zero in most industries, suggesting the attenuation bias playing a role here. Besides, as pointed out by Bond and Blundell (2000), the GMM estimator cannot eliminate the simultaneity bias. By increasing the instrument set, the SGMM was supposed to deal with this problem, and these results point in this way also. The LP and Wooldridge estimators were able to correct the effects of simultaneity bias on capital as well.

[8] The coefficients and their identification and serial correlation tests are presented on Table A-3 in Appendix.

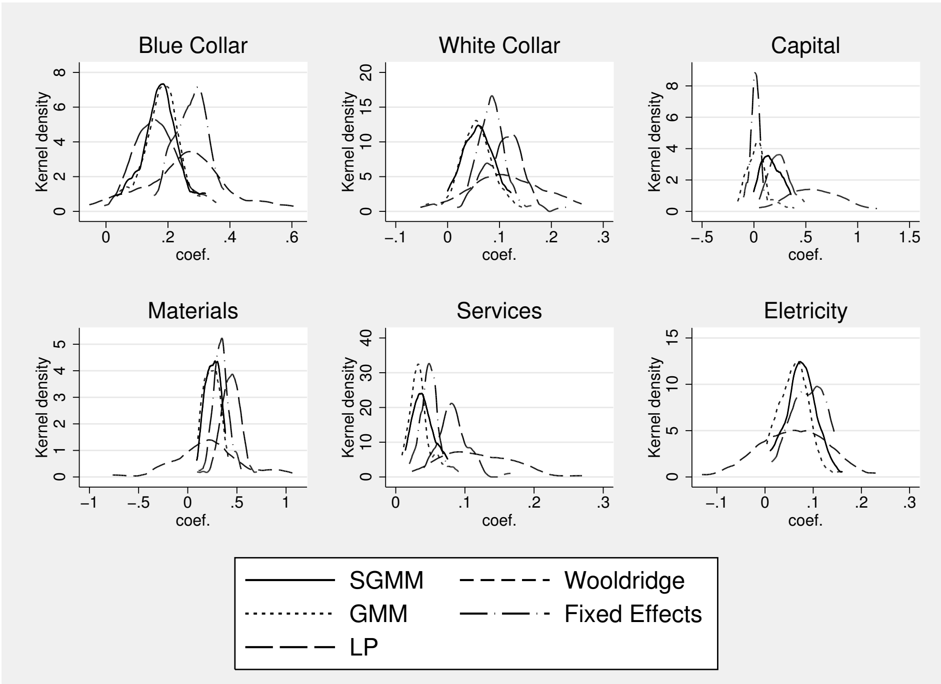


Figure 3. Kernel distribution of the coefficient estimates by alternative estimators.

Finally, literature points to an upward bias on labor coefficient estimates; however, the LP estimated parameters for the white collar labor are higher than the others. This result may be related to the problem in Levinsohn and Petrin (2003) method pointed out by Ackerberg et al. (2006). For blue collar labor estimates, the Wooldridge's estimator leaded to lower coefficients than the others estimators.

Therefore, the labor's coefficients estimated by productivity proxies—i.e. LP and Wooldridge—were apart from each other and were not in accordance with prior literature. Besides that, the econometric tests revealed a smaller number of industries with identification problems for SGMM estimates than the others.

5. Payroll tax exemption effects

The RCT approach applied in this research was based on Scherer (2015). The author's results are very different from other works which applied similar methodology, i.e. Dallava (2014) and Garcia et al. (2018). Scherer (2015) found out divergent results when considering only firms with less than 50 employees and the whole sample. Considering the whole sample, the payroll tax exemption estimated effect was 4.6%, while the effect for the reduced sample was 14.4%. Such difference makes it important to understand the impact of the sample considered to identify the policy effect. For that reason, in this research the policy impacts were estimated considering three different samples: (i) firms with less than 50 employees, which is in accordance with the identification strategy; (ii) the whole sample, which harms the identification strategy; and (iii) firms with 30 to 50 employees, which is also in accordance with the identification strategy, but eliminates the smallest firms.

In order to check the common trend between treatment and control groups, Figure A-1—presented in Appendix—shows the average number of employees per firm evolution for the three samples and for treatment and control groups in the period before the policy, i.e. 2010 and 2011, and right after the policy implementation in 2012. For firms with less than 50 employees, there is a common trend for average employees per firm between 2010 and 2011, which endorses the identification strategy. Also, both groups have close average employees per firm—from 9.5 to 11. In 2012 this values grows markedly more for the treatment group, which may be related to he payroll tax exemption, even though it is necessary to control other variables effects. The common trend is not so accentuated considering firms with 30 to 50 employees. However, both groups have very close average employees per firm, and the divergence between the values is reduced. Finally, for the whole sample there is a remarkable divergence between the groups both for the variable trend before the policy and for the average values. Therefore, the identification strategy is justified and seems to be consistent. However, it throws away as important part of the sample and may lead to a result which does not have external validity, i.e., the estimated policy effect only for small companies.

The payroll tax exemption effects is estimated by equation (1) for the three samples and for Fixed Effects and Difference-in-Difference procedures. Estimates are presented in tables A-1 and A-2 on Appendix. For firms with less than 50 workers, estimates are very close do the results found by Scherer (2015). With these estimates and considering the average number of employees, the percetual impact on employment for each samples, which is showed in Table 5.

Accordingly to Scherer (2015) results, the estimated effects on employment are considerably higher for the sample with less than 50 workers than for the whole sample both for fixed effects and difference-in-difference, even though the disparity

Table 5. Percentual change on employment by firms sizes – FE and diff-in-diff.

	Δ%	
	FE	Diff-in-Diff
Less than 50 employees	14.40	15.21
Whole sample	9.99	10.29
30 to 50 employees	8.47	8.83

is lower for our estimates. However, to consider the whole sample increases disparity between control and treatment groups. In order to investigate if the policy impact depends on firms sizes, policy effects were estimated for firms with 30 to 50 workers, which does not harm the identification strategy and allows to identify the effect for larger firms. The results presented indicated that policy indeed have more effect on smaller firms, since the effects reduces from at around 14% to around 8% for larger firms both for fixed effects and difference-in-difference. Therefore, these estimates suggests that Scherer (2015) results may overestimate payroll tax exemption total effects and that the policy affects smaller firms more.

Such conclusion may be interpreted in line with what Nevo and Whinston (2010) called the "external validation" problem. The identification strategy used in this quasi-experiment correctly considers only firms with less than 50 employees since it makes treatment and control groups more alike. The groups' common trend confirms that this identification strategy succeeds in creating similar and comparable groups, since they share the same trend for dependent variables before the policy and the main difference between the groups is that one is affected by the policy while the other is not. However, the parallel trend disappears when the whole sample is considered, which makes the estimated effects for this case much less credible. Then, whether results for the smaller firms may or may not be projected for the whole sample is a question that remains open and this approach is not capable of shedding much light on it.

Regarding the payroll tax effects estimated by the structural model, the gaps used on the simulation were calculated according to expression (2) with production function coefficients estimated according to expression (4). These coefficients where used to estimate the value of marginal product of blue and white collar labors according to expression (3). The estimated gaps are in line with the results presented by Petrin and Sivadasan (2013) for Chilean industry, both in terms of the relationship between gaps and average wage and for the gap evolution across time.

To simulate the payroll tax exemption effects it was supposed 5%, 10%, 15% and 20% reductions on gaps. The gaps used for simulation were gaps of 2011, the year before the payroll tax enforcement. The simulation was calculated for all industries separately and for gaps calculated based on all production function estimators, both for blue collar jobs, for white collar jobs, and both the general effect (i.e. blue collar jobs plus white collar jobs). Gaps were estimated using the production function coefficients estimated by the five alternative estimator presented on section 3, i.e., System GMM (SGMM), GMM, Wooldridge, Levinsohn and Petrin (LP) and Fixed Effects.

First, in order to check the relationship between the policy estimated effects by RCT approach and by the structural model, the gap simulations first presented are those for textile, garment and leather and shoes industries, which were the industries also considered for the RCT estimates. Figure 4 shows the total effect for the three

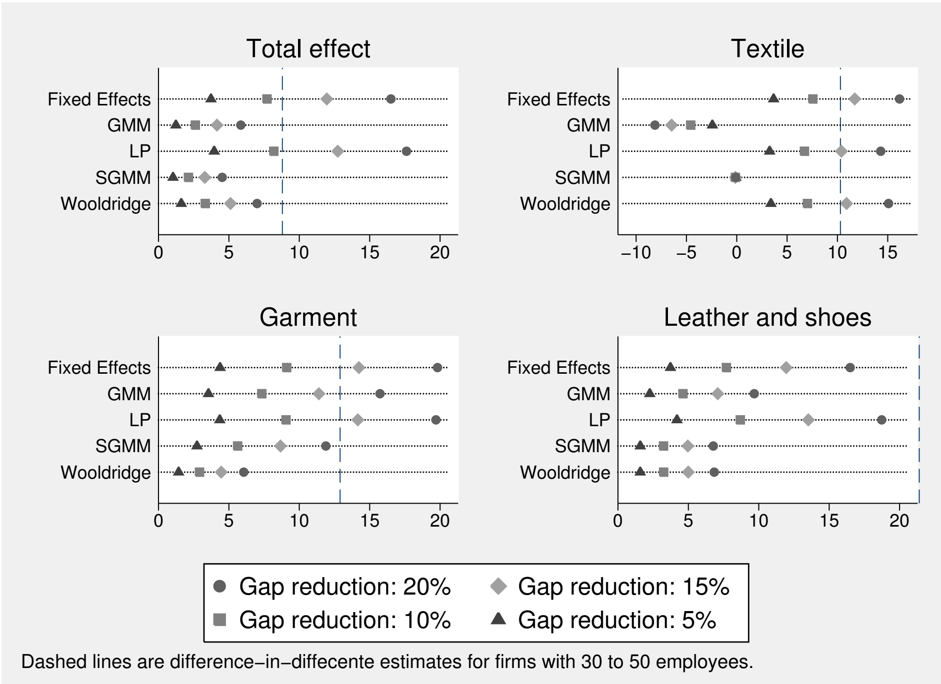


Figure 4. Payroll tax exemption effects for textile, garment and leather and shoes industries.

sectors together, as well as estimated effects for each one. RCT estimative for firms with less than 50 employees estimated by difference-in-difference are indicated by dashed lines.

Policy estimated effects are directly related to gaps sizes, which in its turn is also directly related to production function coefficients. As mentioned on section 4, Fixed Effects and LP resulted in overestimated labor coefficients, which are related to the higher policy effects estimated based on these coefficients. Considering total effects, textile and garment, these estimators lead to results close to those estimated by RCT procedure, which is another evidence that its identification strategy lead to overestimated effects. For leather and shoes, these estimators lead to simulated changes on employment closer to difference-in-difference, but even these results were significant inferior to those calculated by RCT approach. This also means that estimates presents by Scherer (2015) for the effects on this sector, which was 35.4% increasing in employment, is overestimated.

Taking into account SGMM coefficients, which were the coefficients with best econometric tests performance, structural model simulated policy effects are lower than those estimated by the RCT approach, even considering 20% of gap reduction for all sectors. Considering aggregate effects on these three industries, the results which considered only 5% of gap reduction are close to the estimates presented by

Dallava (2014) and Garcia et al. (2018), which did not find any significant policy effect. For textile, structural model simulated effects were also close to zero.

Since the payroll tax effects estimated by the gap methodology are based on production function coefficients, and not on observed statistics, it is possible to carry out counterfactual analysis in order to estimate the policy likely effects on industries not affected by the policy. Therefore, the simulation was calculated for all industries of the *Simples*, i.e. for industries benefited and not-benefited by payroll tax exemption. Since the non-benefited industries were not affected by the policy, these results are projections of likely effects of the policy, while benefited industries estimates intend to estimate the changes that actually happens as consequence of the policy. This is one advantage of structural approach over RCT models, which cannot estimate likely effects on non-benefited sectors.

Considering only industries benefited by the police between 2012 and 2017, results show that payroll tax exemption has an impact of 4.13% on employment for gap reductions of 5% on each sector. It is important to notice this is not an estimative of the total effect of policy, since the group of industries considered here does not include some sectors that were benefited by it, as the Information Technology, for instance. This estimate is higher than those of Garcia et al. (2018), which estimated no significant policy effect for all industries which were benefited by the policy considering the whole time that it was in force.

Considering the same gap reduction of 5%, simulated policy effect for industries which were benefited in 2012 was 2.3%. Since the policy effect for all industries was 4.13%, the policy expansion on further years increased the policy impacts by including sectors which were more sensitive to the policy. For all industries, the simulated employment expansion caused by the same gap reduction is 3.71%. This means further expansions on the group of benefited industries would have positive but marginally decreasing effects.

6. Conclusion

A wide range of methodological frameworks and available data allows researchers to choose among many alternative procedures to evaluate public policies. This research tried to study a case—payroll tax exemption for Brazilian firms—in order to understand the relationship and complementarities between Quasi-experiment—or RCT—and Structural Model.

Based on literature, the RCT approach considered at first only firms with less than 50 employees in order to increase similarities between control and treatment groups. Even though such procedure leads to similar trends between control and treatment and to significant estimated coefficients, regression with whole sample and only firms with 30 to 50 workers suggest that these identification strategy lacks external validity, i.e. results are valid only for small firms. Results from structural model also indicates that this RCT procedure overestimates policy effects, since its results were lower than RCT's even considering gap reductions of 20%. Regarding the structural model estimates, results which considers gap reductions of 5% are more close to the literature results which show that the policy had reduced effects, boosting employment by 2.3%, much less than RCT's estimated effects. This approach also allows estimates of likely effects on other industries than only those benefited by the policy. These estimates show that payroll tax expansions during 2012 and 2014 included industries which react more to payroll tax exemption than the industries benefited by the policy at first, but further expansions would improve employment by a lower rate. As conclusion, even though this research found positive effects for payroll tax expansion, huge tax collection reductions for small impact on employment casts doubt up on this policy cost-benefit.

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Appendix.

Table A-1. Fixed effects estimates by industries for different samples.

	(1)	(2)	(3)	(4)	(5)	(6)
time	0.372***	−0.258	−1.115***	0.363*	0.108*	0.533**
	(7.28)	(−0.76)	(−3.68)	(2.57)	(2.05)	(3.06)
treat	1.551***	3.706***	3.236***	0.977***	1.253***	2.585***
	(15.14)	(5.69)	(5.84)	(3.64)	(9.67)	(8.42)
$e[cnpjraiz, t]$	−1.376***	−3.565*	−10.67***	−0.608	−0.516*	−5.631***
	(−6.07)	(−2.31)	(−5.28)	(−1.05)	(−2.22)	(−8.13)
Constant	−0.378***	−0.378	0.0809	−0.312***	−0.158***	−0.676***
	(−12.07)	(−1.84)	(0.45)	(−3.69)	(−4.65)	(−6.66)
Observations	157,166	175,232	12,980	16,465	85,022	24,589

Notes: (1) Three industries, firms with less than 50 employees; (2) Three industries, firms whole sample; (3) Three industries, firms 30 to 50 employees; (4) Textile, firms 30 to 50 employees; (5) Garment, firms 30 to 50 employees; (6) Leather and shoes, firms 30 to 50 employees.
t statistics in parentheses.
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Table A-2. Difference-in-difference estimates by industries for different samples.

	(1)	(2)	(3)	(4)	(5)	(6)
treattime	0.363***	−0.258***	−1.118***	0.328***	0.0969**	0.566***
	(9.48)	(−4.39)	(−4.36)	(3.66)	(3.16)	(4.54)
treatind	−0.862***	−1.964***	−1.757***	−0.647**	−0.783***	−1.254***
	(−11.67)	(−3.53)	(−4.94)	(−3.26)	(−5.71)	(−6.36)
Diff-in-diff	1.638***	3.817***	3.375***	1.138**	1.412***	2.538***
	(11.55)	(3.53)	(4.97)	(3.21)	(5.43)	(6.59)
$e[cnpjraiz, t]$	−1.366***	−3.543***	−10.67**	−0.623	−0.520	−5.626***
	(−4.38)	(−5.06)	(−3.26)	(−0.75)	(−1.69)	(−4.46)
Constant	−0.177***	0.129***	0.560***	−0.145***	−0.0438**	−0.291***
	(−9.08)	(4.16)	(4.19)	(−3.32)	(−2.79)	(−4.49)
Observations	157,166	175,232	12,980	16,465	85,022	24,589

Notes: (1) Three industries, firms with less than 50 employees; (2) Three industries, firms whole sample; (3) Three industries, firms 30 to 50 employees; (4) Textile, firms 30 to 50 employees; (5) Garment, firms 30 to 50 employees; (6) Leather and shoes, firms 30 to 50 employees.
t statistics in parentheses.
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Table A-3. Coeficients and tests by industry–System GMM and Wooldridge.

	System GMM								Wooldridge						
cnae	β_e	$\beta_c i$	β_s	β_k	β_{lb}	β_{lw}	SG	$AR2$	β_e	β_{ci}	β_s	β_k	β_{lb}	β_{lw}	SG
151	0.08	0.36	0.02	0.18	0.18	0.02	1.00	0.60	0.24	0.15	0.08	0.43	0.19	0.07	0.94
152	0.08	0.27	0.03	0.22	0.10	0.07	1.00	0.09	0.01	0.44	0.05	0.13	0.29	0.17	0.23
158	0.09	0.33	0.03	0.18	0.25	0.06	1.00	0.06	0.03	(0.01)	0.17	0.85	0.34	0.17	0.15
173	0.16	0.22	0.04	0.23	0.13	0.02	1.00	0.52	0.14	0.25	0.02	0.50	0.26	0.09	0.02
176	0.13	0.12	0.04	0.13	0.16	0.09	1.00	0.32	0.09	(0.36)	0.14	0.77	0.33	0.22	0.00
177	0.12	0.22	0.02	0.12	0.06	0.02	1.00	0.46	0.00	(0.14)	0.05	0.69	0.43	0.15	0.14
181	0.05	0.16	0.04	0.28	0.15	0.03	1.00	0.79	0.09	(0.05)	0.09	0.72	0.29	0.05	0.01
191	0.08	0.25	0.03	0.30	0.18	0.08	1.00	0.15	0.11	0.26	0.08	0.81	0.26	0.21	0.20
193	0.10	0.18	0.04	0.22	0.17	0.05	0.76	0.66	0.07	(0.17)	0.11	0.28	0.50	0.08	0.03
201	0.11	0.20	0.07	0.22	0.25	0.04	1.00	0.29	0.13	0.11	0.10	0.39	0.35	0.05	0.29
202	0.08	0.33	0.06	0.12	0.19	0.09	1.00	0.18	0.07	0.87	0.07	0.47	0.06	0.15	0.09
212	0.07	0.17	0.06	0.06	0.19	0.05	1.00	0.05	(0.02)	0.16	0.16	0.35	0.28	0.12	0.23
213	0.05	0.27	0.04	(0.00)	0.16	0.05	1.00	0.26	0.02	0.74	0.16	0.53	0.07	0.03	0.22
214	0.08	0.24	0.01	0.18	0.10	0.08	1.00	0.65	0.07	(0.76)	0.11	0.45	0.45	0.08	0.00
241	0.05	0.32	0.03	0.27	0.10	0.04	1.00	0.52	0.05	0.20	0.20	0.71	0.08	0.07	0.24
242	0.02	0.20	0.03	0.33	0.17	0.08	1.00	0.67	0.01	0.43	0.09	0.45	0.17	0.07	0.34
251	0.11	0.13	0.04	0.17	0.24	0.04	1.00	0.40	(0.05)	0.28	0.17	0.73	0.27	0.11	0.08
252	0.08	0.30	0.04	0.13	0.22	0.05	0.63	0.42	0.04	0.25	0.19	0.49	0.30	0.12	0.00
263	0.08	0.30	0.04	0.09	0.20	0.07	1.00	0.35	0.14	(0.19)	0.10	0.37	0.32	0.11	0.75
269	0.11	0.17	0.04	0.21	0.20	0.05	1.00	0.39	0.09	0.34	0.16	0.76	0.29	0.09	0.36
274	0.10	0.33	0.05	0.09	0.16	0.08	1.00	0.11	0.03	0.06	0.19	0.92	0.34	0.02	0.09
275	0.09	0.26	0.04	0.07	0.32	0.02	1.00	0.08	0.09	0.53	0.13	0.46	0.29	0.01	0.72
283	0.07	0.20	0.04	0.11	0.23	0.08	1.00	0.79	0.14	0.17	0.17	0.05	0.31	0.10	0.01
284	0.08	0.27	0.05	0.07	0.16	0.06	0.98	0.23	0.15	(0.19)	0.14	0.71	0.62	0.23	0.31
289	0.12	0.29	0.02	0.18	0.13	0.07	0.93	0.46	0.12	0.16	0.18	0.61	0.16	0.13	0.00
291	0.04	0.35	0.04	0.28	0.17	0.09	0.99	0.05	(0.05)	0.17	0.10	0.79	0.28	0.18	0.11
292	0.02	0.32	0.05	0.07	0.18	0.10	1.00	0.77	0.03	0.40	0.09	0.26	0.12	0.12	0.00
293	0.02	0.28	0.05	0.03	0.32	0.06	1.00	0.03	0.01	0.51	0.07	0.06	0.33	0.12	0.08
294	0.07	0.36	0.07	0.06	0.22	0.07	1.00	0.62	0.13	0.06	0.09	0.54	0.24	0.15	0.38
295	0.09	0.33	0.05	0.33	0.22	0.05	1.00	0.36	0.04	(0.26)	0.18	0.84	0.45	0.19	0.49
296	0.07	0.37	0.07	0.01	0.16	0.07	1.00	0.51	0.07	0.18	0.15	0.43	0.25	0.14	0.00
299	0.05	0.22	0.04	0.36	0.30	0.11	0.98	0.59	(0.03)	0.06	0.27	0.78	0.19	0.01	0.33
311	0.06	0.17	0.03	0.15	0.22	0.04	1.00	0.90	0.04	0.39	0.12	0.89	0.10	0.06	0.08
312	0.07	0.28	0.06	0.01	0.16	0.12	1.00	0.35	0.07	0.16	0.06	0.53	0.24	0.23	0.98
313	0.11	0.38	0.03	0.12	0.06	0.05	1.00	0.09	0.12	0.11	0.08	0.35	0.20	0.21	0.22
331	0.01	0.21	0.03	0.13	0.21	0.04	1.00	0.06	(0.13)	0.34	0.12	0.42	0.25	0.18	0.15
344	0.10	0.29	0.08	0.13	0.16	0.08	0.03	0.94	0.10	0.22	0.05	0.61	0.23	0.11	0.04

Notes: p-values for Sargan (SG) and second-order autocorrelation tests.

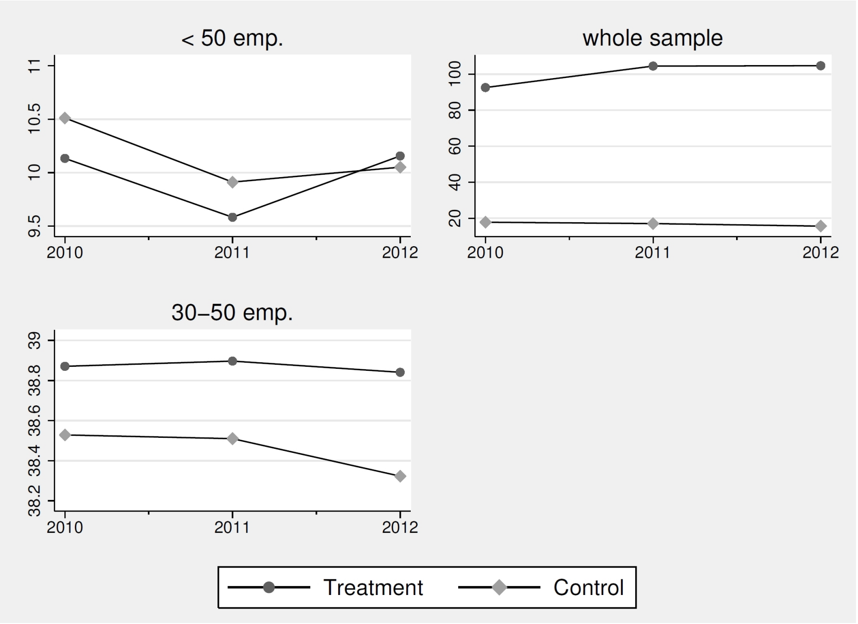

Figure A-1. Average employment in treatment and control groups—firms with less than 50 employees, the whole sample and firms with 30 to 50 employees.