Redesigning taxes to increase economic activity? Evidence from a French Business tax reform[1]

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Abstract

Empirical evidence of input-based taxes on firms' outcomes is scarce. This paper fills this gap by exploiting a reform of the French business tax system that shifted the tax burden from both labor and capital to capital only. I apply a dynamic differences-in-differences approach leveraging cross-sectional variations in firms' exposure to the reform. Using rich administrative data, I show that the reform boosts firms' investment in capital, particularly for those initially cash-constrained. For a 1% reduction in taxes, tangible assets increase by 0.43%. This generates significant productivity gains. Wages are not affected. I estimate a negative effect on employment, which indicates slower employment growth in more affected labor-intensive firms. I can explain this effect by showing that firms replace low-skilled workers with fewer – more productive – high-skilled workers, generating lower employment growth. Another explanation is that firms might have varying tax sensitivity based on whether firms can deduct business tax payments from the corporate income tax.

JEL Classification: H22, H25, H32, H71

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1 Introduction

How should firms be taxed? This question has far-reaching consequences for key economic outcomes. In particular, the composition of the tax base – whether it targets profits, sales, labor, capital, or a combination – can influence firms' decisions and overall economic performance. Seminal theoretical models have long demonstrated that taxing input factors is distortionary (Harberger, 1962; Kotlikoff and Summers, 1987; Diamond and Mirrlees, 1971a,b). It induces firms to substitute away from the taxed input, hindering efficient production. These models are derived under strong assumptions, such as perfect competition or the perfect mobility of inputs. In practice, firms operate in complex environments with various constraints and incentives, making empirical testing of these theoretical predictions essential. This study aims to fill this gap and provide a comprehensive picture of the impact of input-based taxes on firms' outcomes, using the unique setting of business taxation in France.

Providing systematic empirical tests of these theoretical predictions has been challenging for various reasons. First, inputs are usually taxed indirectly, for example, through corporate income taxes. Property taxes are common but generally apply to buildings and land – not to production-related capital like machinery and equipment. Payroll taxes, while based on wages, are often used to fund benefit schemes and, therefore, may not be considered proper input taxes. Additionally, existing studies on these taxes tend to focus on changes in tax rates or narrow adjustments to the tax base (e.g., targeting specific groups of workers). Thus, uniform shifts in the marginal cost of labor are rarely observed. Second, obtaining a causal effect of input-based taxes requires finding an exogenous and meaningful variation that is salient to firms. In addition, targeted tax reductions may come at the cost of the sample size, which allows neither generalizability nor heterogeneity analysis, which is crucial for getting a comprehensive picture.

This paper exploits a unique French business tax reform that changed its tax base, switching from a capital-labor tax base to a tax based solely on capital. The reform was discussed in late 1998 when the debate regarding the national budget started and then implemented in 1999. At that time, the negative impact of the tax on employment and investment was already acknowledged in political circles, and this reform attempted to alleviate these concerns. The reform generated tax reductions for all firms but of different magnitudes depending on the labor

share in their pre-reform tax base: the more labor-intensive firms were, the more exposed to the reform. The tax accounted for around 30% of firms' overall tax burden and about 1.5% of firms' pre-reform turnover. The tax reduction induced by the reform was up to 20% for the most exposed.

To explore the impact of the reform on firm-level outcomes, I draw on unique administrative data that includes linked employer-employee microdata, as well as financial and tax records. By merging these data, I have information on the workforce, the capital stock, and the taxes paid for all private firms. I apply a dynamic differences-in-differences approach, taking advantage of firm-specific cost reductions proportionate to firms' labor share. I compare firms that are most exposed to the reform (those with a higher labor share) to firms that are relatively less exposed (those with a lower labor share). Since labor-intensive firms might differ significantly from capital-intensive ones, I refine the comparison by grouping firms into bins based on their labor intensity in 1998, the bins being the quintiles of the overall distribution. I estimate the effect of the reform within these bins. This allows me to have some variation in exposure while comparing firms with relatively similar production functions. The identification strategy rests on the assumption that, within the same bin, firms with different labor shares would have followed the same trajectory in the absence of the reform. I test the plausibility of the assumption using the pre-reform periods and show that before the reform, firms evolved similarly and did not anticipate the change in the tax base.

My results are as follows: First, the reform significantly reduces the taxes paid by the most exposed firms. For an additional one percentage point in labor share, the tax paid is reduced by 0.71%. This first exercise can be seen as the "first stage" of estimation as it provides evidence that the labor share is a good proxy for the exposure to the reform. It also shows that the reform generates a significant tax windfall.

I investigate how firms use this substantial tax windfall and find a strong and positive effect on capital. For a 1% reduction in taxes, the stock of tangible assets increases by 0.43%. I also estimate that, overall, these adjustments increase productivity. I identify that firms that were cash-constrained before the reform drive the effect on investment. This suggests a liquidity

effect, where the tax reduction alleviates credit constraints and enables these firms to expand by providing additional cash flow.

Regarding labor, canonical tax incidence models suggest that tax changes might affect wages. Investigating the effect of tax reductions on the median gross hourly wage, I show that the reform did not prompt firms to increase wages. The absence of wage adjustment is at odds with the theory of tax incidence but in line with recent evidence from the payroll tax literature (see Benzarti, 2024, for a review). In addition, my estimates allow me to rule out any economically meaningful responses regarding the hours worked per worker. Therefore, the fact that labor became cheaper did not induce employees to work more hours.

Given that I observe no changes in wages or the number of hours worked and that one of the goals of the reform is to increase employment, I then turn to the employment responses of firms. I estimate a negative effect on employment, with an elasticity of 0.11. While this might seem surprising initially, given the existing literature, this negative effect indicates slower employment growth in – more exposed – labor-intensive firms. I identify two potential mechanisms explaining these differences in employment growth. The first explanation is that relatively more labor-intensive firms replace low-skilled workers with fewer – more productive – high-skilled workers, generating lower employment growth. The estimates suggest that firms roughly replace three low-skilled workers with one high-skilled worker, leading to a net reduction in total employment. The second explanation is that firms might have different abilities to deduct business tax payments from the corporate tax base. Before the reform, non-profitable firms, which could not offset the tax due to their lack of corporate tax liability, bore the full cost of the business tax on labor, constraining their labor demand. After the reform, these firms benefit relatively more from the tax reduction, leading to stronger employment growth. Supporting this mechanism, I find that (i) capital-intensive firms, which increase employment the most, tend to be less profitable and therefore less likely to pay corporate taxes, and (ii) firms that did not pay corporate tax before the reform and thus could not deduct the tax on labor, exhibit the largest employment growth after the reform.

I test the robustness of my results and find similar point estimates after including a comprehensive set of flexible non-parametric local controls at different levels of aggregation. This

suggests that potentially relevant omitted variables, such as local or industry shocks, are not driving the results. I show that the point estimates are also robust to different levels of clustering, implying that standard errors are not artificially low. The results do not depend on the window of the event study. More importantly, I show that my results are robust to a change in the bins definition and the proxy of exposure to the reform. I also conduct additional checks and show that my estimates are unlikely to be driven by the reduction in working time, a potential confounding reform that started in 2000.

The contributions of this paper are threefold. First, studying the effects of this specific business tax (directly based on factors of production) allows me to test the theoretical conclusions of the taxation models empirically. The theoretical literature has long highlighted the distortions induced by taxing input factors (Harberger, 1962; Kotlikoff and Summers, 1987). In particular, the production efficiency results of Diamond and Mirrlees (1971a,b) provide a strong case against using taxes on production inputs as part of optimal policies. This result is, however, derived under strong assumptions such as an economy with no profit – or significantly taxed. In this paper, I turn the question around and ask whether removing taxes on input factors – here labor, improves firms' productivity. I provide empirical insights suggesting that taxing labor inputs directly is not desirable even in a non-perfectly competitive economy with profit.

Second, I contribute to the corporate tax literature. I provide new insight into firms' responses to taxation, opening the black box of firms to evaluate a different type of taxation. In addition, I demonstrate the importance of considering the overall tax liabilities and existing interaction between the different tax bases. Several papers underline the harmful effects of corporate taxation. Most studies highlight that a non-negligible share of the burden is borne by workers when the tax increases and wages fall (Suárez Serrato and Zidar, 2016; Fuest et al., 2018). Recent evidence suggests the effect is symmetric, following a tax cut wages increase, at least for manufacturers in Québec (Duan and Moon, 2023). We also know that corporate tax changes affect employment (Giroud and Rauh, 2019; Kennedy et al., 2022), but also investment (Mukherjee et al., 2017; Lichter et al., 2024; Link et al., 2024). Most evidence rests, however, on the increase of taxes based on profits. Closer to my setting, are the targeted tax incentives

aiming at reducing the cost of a particular input. This strand of the literature has been growing over the last few years. However, there is still limited evidence examining the effect of corporate tax incentives on labor and capital outcomes within the same firms and across various sectors. Existing papers connecting these two outcomes within the same firms have been limited to manufactures (Lerche, 2022; Duan and Moon, 2024). The only exception is probably Harju et al. (2022), but it is limited to small firms. Other studies focusing on one outcome at a time have underlined that investment is highly sensitive to taxes (Zwick and Mahon, 2017; Ohrn, 2018; Maffini et al., 2019; Chen et al., 2023). Despite targeting specific capital and industries, these tax incentives can have spillover effects on the local labor market (Curtis et al., 2022; Lerche, 2022). Studies evaluating the effect of tax incentives targeting labor are even scarcer; to my knowledge, only Carbonnier et al. (2022) do so. In this paper, I use a substantial tax reduction based on labor share and provide evidence of its effect on labor and capital outcomes of firms across all industries.

Third, by removing the wage bill from the tax base, this paper contributes to the payroll tax literature, as the reform I study can be seen as an employer payroll tax cut without benefit linkage. Existing studies on payroll taxes typically focus on rate changes or narrow adjustments to the tax base (e.g., targeting specific worker groups), which can distort the labor market and, therefore, provide limited insights into the effects of broader tax base changes (e.g., Saez et al., 2012). In contrast, I offer new evidence that applies universally, avoiding wage distortion and reducing the marginal cost of all workers. My results support recent studies showing that not only employment but also investment can be affected by a reduction in the taxation of labor and that effects are particularly large for firms initially cash-constrained (Saez et al., 2019; Benzarti and Harju, 2021a,b). I also outline new mechanisms likely to be at work for the payroll taxes: the interaction of the different tax bases is likely to shape firms' responses.

The remaining of this paper is organized as follows: Section 2 provides information about the institutional background and describes the data sources; Section 3 describes the identification strategy; Section 4 presents the results; Section 5 discusses the potential mechanisms at work; Section 6 concludes.

2 Institutional Setting and Data

Section 2.1 first describes the institutional context and Section 2.2, the reform of interest. Section 2.3 details the data sources and the construction of the panel of estimation.

2.1 The French business tax from 1975 to 2010

Since 1975, a local business tax has been levied on establishments having a for-profit activity in France.[3] Three sub-national administrative divisions – region, county, and municipality – determine the final rate faced by plants.[4] Tax rates are voted on every year. As a result, the final tax rate faced by economic agents varies over time and across municipalities. Details on the definition of the local tax rates can be found in Appendix C. In my identification strategy, I do not leverage this source of variations (see Section 3 for more details).

Before 1999, the tax was based on fixed tangible assets, properties, and 18 % of the wage bill. The rental value of fixed tangible assets is defined as 16% of the cost price[5] or is equal to the rent paid by plants. Fixed tangible assets are usually machinery and equipment used in the production process. For properties, the rental value equals 9% of the value stated in the balance sheet. Even though this base has evolved, it has always been defined at the national level. The tax base of each jurisdiction depends solely on the inputs located within its boundaries; there are no apportionment rules.

Figure 1 displays the share and values of each item of the tax base across industries in 1999 (without accounting for the reform yet). It is worth noting that the wage bill accounts for about 40%, a non-negligible share. Second, Figure 1 suggests that the exposure to the reform is not the same across industries. For example, the construction sector is likely to be more affected than other activities, given the difference in the shares of the wage bill. However, as we will see in the next section, the 1-digit industry classification hides significant heterogeneity across firms.

[3]Independent white-collar professionals, such as physicians or lawyers, with less than five employees, are taxed on their income only. They are, therefore, not affected by the reform and are excluded from the sample.

[4]These jurisdictions divide the national territory into different tiers of local government. Each division has a fiscal power, setting the tax rates levied in their jurisdiction.

[5]The cost price is generally equal to the depreciation base of assets.

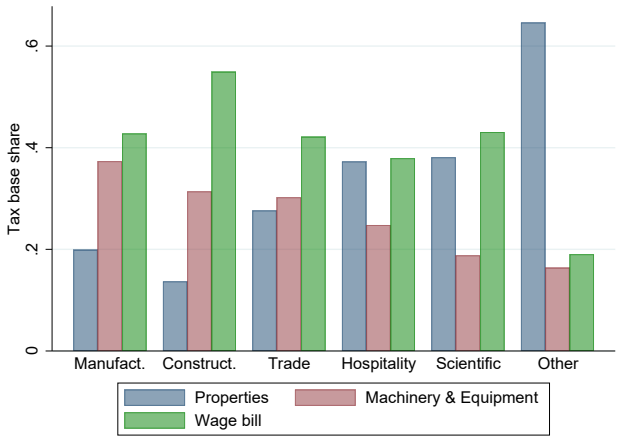


Figure 1: Average shares of the tax base components across industries

Note: This figure plots report the average share of the main items of the business tax base across the main industries of the sample.

The business tax is due whether firms are profitable or not. When firms are profitable, they must also pay the corporate income tax. In this case, the business tax payment can be deducted from the corporate tax base. As a result, the top-up induced by the business tax is the same across firms, but the net cost of inputs will be larger for non-profitable firms, where they cannot deduct the business tax from the corporate tax and, therefore, not offset its cost.

2.2 The 1999 reform

As the local business tax was denounced as constraining economic activity, especially employment (Bayard and Balligand, 2019), the national government decided to remove the wage bill from the business tax base gradually. The reform was officially announced in the fall of 1998, when the budget for the upcoming year was voted on.[6] The reform took place over four years. Each year, a fixed amount is deducted from the taxable value of the wage bill. The tax allowance cannot exceed the initial taxable value. The yearly fixed allowance is granted at the municipal-by-firm level.[7] The yearly tax allowance spanning 1999 to 2002 is summarized in Table 1. The intention behind this phased allowance was to alleviate the burden on smaller

[6] A quick research in the archive on the biggest French newspaper *Le Monde* suggests that the discussion about the reform started to be covered in September, shortly before the official announcement.

[7] This implies that multi-plant firms can benefit multiple times from the yearly allowance only if they have establishments across different municipalities. As a result, multi-plant firms have incentives to strategically reallocate their workforce to maximize the benefits of the reform.

businesses swiftly. For example, a firm with a taxable value of 50,000 € before the reform would end up with a taxable value of 31,477 € in 1999 and 0 € from 2000. A firm with a taxable value of 500,000 € before the reform would stop paying taxes on labor from 2002.

Table 1: Yearly tax allowance related to the 1999 reform

Year	1999	2000	2001	2002
Allowance				
in Francs	100,000	300,000	1,000,000	6,000,000
in Euros	18,523	55,235	180,413	1,062,362

Note: The values are expressed in 2010 euros. The table reads: in 1999, all firms received a tax allowance of 18,523 euros. This means they subtract this amount from the 18% of the gross wage bill initially taxed. In the year 2000, 55,235 is subtracted from the taxable value of the wage bill, and so on. Source: PROJET DE LOI DE FINANCES (1999)

From 2003 onward, the wage bill was no longer part of the tax base. Although the phasing out process occurred gradually, all firms experienced its effects from 1999 onwards due to uniform yearly allowances. However, the impact varied in intensity across different firms. Smaller firms with lower total wage bills benefited from the reform sooner. In earlier years, the tax allowance was more likely to fully cover the 18% of the wage bill for these firms. Meanwhile, larger firms had to wait until 2003 before they were relieved of business taxes on the wage bill.

The exposure to the policy also depends on firms' production functions. The more labor-intensive the firm, the larger the tax reduction induced by the reform. Indeed, if the wage bill accounts for a larger share than the capital (machinery and buildings) in the tax base, the tax liabilities are substantially reduced thanks to the reform. The last element affecting the exposure to the reform is the firms' location. As underlined in the previous section, the business tax rates are locally defined. So, depending on the firms' location, they face a more or less higher tax rate, which mechanically affects the size of the tax cut induced by the reform. I will, however, show that this is not the key determinant of exposure to the reform.

Importantly, the national government compensated municipalities for the change in tax collected due to the reform. More precisely, the difference between the amount of tax collected in a given year and what should have been collected had the reform not happened. As a result, the 1999 reform removing the wage bill from the tax base did not affect the resources of municipalities (PROJET DE LOI DE FINANCES, 1999; Bayard and Balligand, 2019). The tax

rates were not affected directly by the reform; they are defined independently by municipalities. However, municipalities could adjust their rate during this period if they wanted to.

2.3 Data

Linked employer-employee data. I obtain information on wages and employment from the French annual declaration of social data at the job position level (*Déclaration Annuelle de Données Sociales* au niveau poste - DADS Postes). It is a mandatory procedure that any employer has to fulfill. This dataset is an exhaustive panel from the 90s onwards. I observe gross and net wages, hours worked, occupation, and working time.[8] Thanks to a unique plant identifier (*SIRET*), I can aggregate the panel at the plant-year level. I define the main workforce as the number of workers present in the firm at the end of the year.[9] The corresponding gross wage bill is computed by summing the annual gross wage of these individuals in the main labor force. This is the wage bill used to construct the proxy of exposure to the policy. I then derive the median hourly gross wage of employees. To investigate the mechanisms, I identify minimum wage workers and sort the workforce according to their skill level. As occupational codes are not well-reported before 2002, I approximate skill levels using the wage distribution relative to the minimum hourly gross wage. High-skilled workers are defined as those earning an hourly gross wage greater than twice the minimum wage, while low-skilled workers are those earning below this threshold. Appendix Figure B.1 plots the distribution of hourly wages relative to the minimum wage by occupations in 2002. Nearly all clerks and blue-collar workers fall below the two-times-minimum-wage threshold, whereas most high-skilled workers exceed it.

Balance sheet data. I use the FICUS source containing balance sheet and income statement data. This source is based on tax and social contribution statements. This dataset covers the universe of firms, except those in the financial and agricultural sectors. It provides detailed

[8]However, occupational codes were not well reported before 2002, as firms with less than 20 employees did not have to report them.

[9]I focus on workers with some attachment to the labor market ("postes non-annexes"), which are defined as contracts involving either more than 120 hours of work or more than 30 days of work, with more than 1.5 hours of work per day, or contracts that paid over three times the monthly minimum wage over the year. This definition comes from the French National Statistics Office (*Institut National de la Statistique et des Études Économiques* - INSEE).

information on firms' revenues and expenses. Key variables are found in this source, such as the total taxes paid and the stock of tangible assets. The total taxes paid variable encompasses the payment related to the local business tax, the property tax, and other smaller taxes (such as the training tax). I winsorize those variables at the top 1%.[10] While the labor force information is reported at the plant level, the financial data are only reported at the firm level. As a result, I will focus on single-plant firms.

Tax statement and local tax rates. I have access to the annual business tax statement for all establishments from 1999-2010. This database details the amount declared for each item of the tax base. However, as the business tax statement is available only starting from 1999 – the year of the reform, I do not base the measure of exposure to the reform on these data. I, however, use them to provide descriptive statistics and supporting evidence that my measure of exposure is a good proxy, see Section 3.

I retrieve fiscal information and especially the local business tax rates in the exhaustive administrative panel of French municipalities (*Recensement des Eléments d'Imposition* – REI). The observations have been reported at the municipal level every year since the 90s. Each municipality is identified thanks to a unique identifier. This panel contains tax rates of all administrative divisions, the number of tax returns, the tax base, and the amount of collected taxes.

Baseline sample. I select a ten-year panel of establishments from the linked employer-employee data covering 1995-2006. This period restriction enables me to study the effect of the tax base reform in a window of four years before and six years after the reform. Limiting the sample to this period enables me to observe the reform's effect after the complete phase-out of the wage bill from the tax base. I restrict the sample to private and single-plant firms and exclude financial and agricultural sectors. These restrictions stem from the coverage and aggregation level of the financial statements. I verify that plants report positive values of tangible assets and at least three employees every period.[11] This has the advantage that I estimate

[10]The data have been imported and cleaned similarly as Burstein et al. (2020); De Ridder et al. (2024).

[11]A large number of firms are tiny (0, 1, or 2 employees). For those firms, discrete adjustment of workers would generate extreme employment growth values (e.g., 100 percent for firms growing from 1 to 2). Hence, I drop these tiny firms.

meaningful variations and ensure the sample is balanced, meaning that firms constantly exist throughout the period of study. As a result, point estimates are not driven by a change in sample composition over time. Details concerning the construction of the panel of firms can be found in Appendix D.

3 Empirical Strategy

This section presents the empirical strategy of the paper. Section 3.1 defines the proxy for the exposure to the reform and shows that this is a good one. Then, Section 3.2 specifies the estimation model.

3.1 Proxying exposure to the reform

As emphasized in Section 2.2, the exposure of firms to the reform depends on three elements: (1) the location of firms determines the business tax rate they face; (2) the production function of firms, i.e., labor-intensive firms are more exposed; (3) the size of firms, i.e., the smaller the firms, the quicker they stop paying taxes on labor. The ideal proxy of exposure to the reform should accurately predict the tax reduction, provide sufficient variation, and, importantly, be persistent over time.

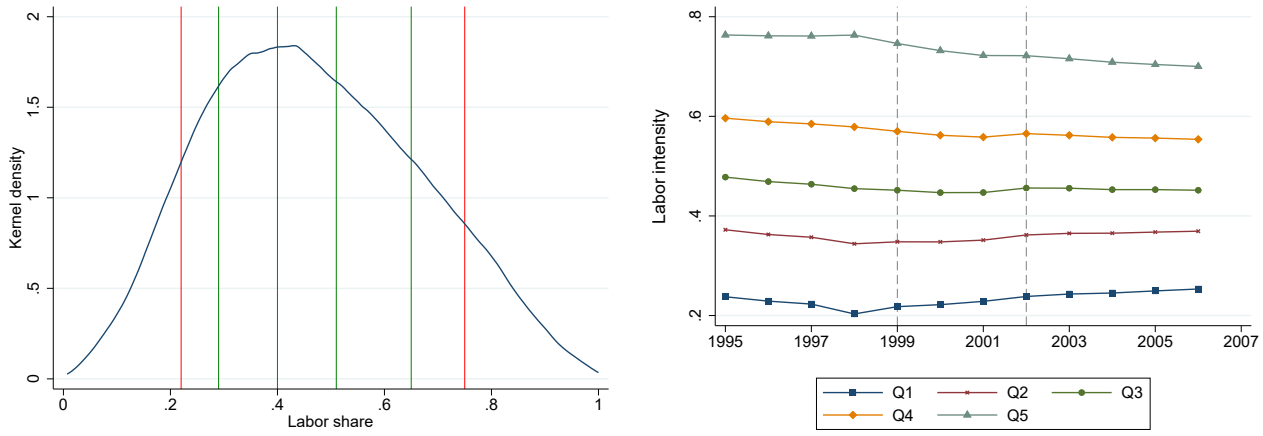
Therefore, I proxy the exposure to the reform by the labor intensity in 1998.[12] More precisely, I define labor intensity as the share of labor in the tax base in 1998:

$$LaborShare_i = \frac{W_{i,1998}}{W_{i,1998} + K_{i,1998}} \tag{1}$$

$W_{i,1998}$, $K_{i,1998}$ are the wage bill and the stock of tangible assets of firm i in 1998. This is a simplified proxy of exposure to the reform. Indeed, the wage bill and the stock of tangible assets of firm i in 1998 are the real values, not the taxable ones. However, this proxy provides sufficient variation (see Figure 2a), is persistent over time (see Figure 2b), and importantly predicts well the reduction in taxes induced by the reform (see Figure 3). I will refer to the

[12]There are different ways to compute the pre-reform labor intensity – over multiple periods or a different period, I carry robustness checks along those lines and find similar effects. I also conduct robustness checks with respect to changes in the tax rate and the firms' size.

ratio defined in equation (1) as the labor share or labor intensity. Firms with a low labor share will sometimes be referred to as capital-intensive.



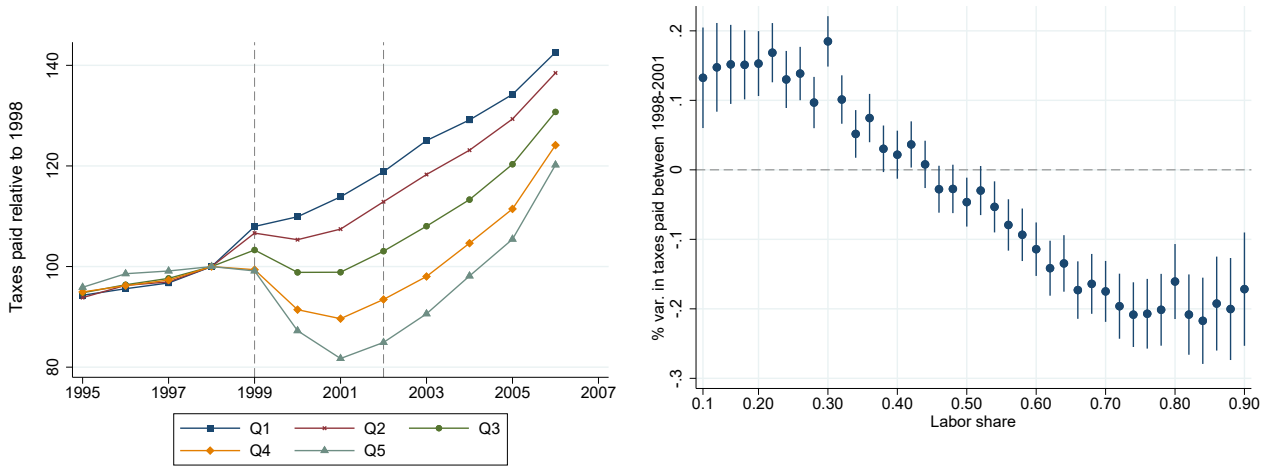
(a) Distribution of labor share in 1998 **(b) Evolution of labor share by quintiles**

Figure 2: Evolution and Distribution of labor shares

Note: Panel (a) shows the labor intensity distribution in 1998. The green vertical lines mark the quintiles of the distribution while the red lines mark the 10th and 90th percentile of the distribution, considered as outliers. Panel (b) plots the average labor share over time for each quintile of the labor intensity distribution in 1998.

In Figure 3a, I plot the average taxes paid over time by quintile, expecting that the more labor-intensive firms, the larger their tax reduction. While the taxes paid before the reform evolve similarly across quintiles, in 1999 – at the time of the reform – the taxes paid by firms decrease proportionally to their labor intensity. In the longer run, however, we observe that the taxes paid increase. This can be explained, as we will see in Section 4.1, by the fact that capital remained tax and firms invest significantly following the reform. As this variable contains other local tax payments – such as the property taxes, I show in Appendix Figure B.5 that this pattern is indeed driven by a reduction in business tax payments using the business tax statements starting in 1999. In Figure 3b, I provide additional evidence that this negative correlation holds within quintiles. In Appendix Figure B.4, I show that this negative correlation only exists in the post-reform period. In Section 4, I show that these correlations can be causally interpreted and are not driven by confounding factors.

I further describe the sample of estimation in Appendix Table A.1 and Table A.3 across the five quintiles of the labor share distribution 1998. We can see that most labor-intensive firms (in Q5) tend to have more employees than the other firms; they have fewer tangible assets and pay relatively less taxes. Firms in the lower quintile of the distributions pay relatively



(a) Evolution of taxes paid by quintiles

(b) Percentage change in taxes paid between 1999 and 2001 by labor share

Figure 3: Relations between labor share and taxes paid

Note: Panel (a) plots the average taxes paid over time for each quintile of the labor intensity distribution. I discretize the treatment intensity into five groups using quintiles of the distribution of labor intensity in 1998. I normalize the annual values, using 1998 as a reference (base 1998=100). Panel (b) shows the average percentage change in taxes paid between 1999 and 2001 and the related 95% confidence interval across the labor intensity distribution, divided into two-percentage-point bins.

lower wages. One might worry that this is driving their labor share down and that they are, therefore, defined as capital-intensive. However, the average amount of capital for one worker is substantially larger for firms in Q1 than in Q5 (about 63 against 11 thousand euros). In addition, it is worth noting that the tax rate in 1998 does not vary much across quintiles nor the share of industries (except for the construction sector, see in Appendix Table A.3). These suggest that estimations are unlikely to be driven by differences in tax rates or a particular industry. I carry out additional robustness checks along these lines in Section 4.3. It also is worth noting that within the quintiles, there is still sufficient variation in exposure to the policy.

Alternative proxies of exposure. There are different potential proxies: the tax rate alone, the labor share in the tax base (as defined in equation (1)), and the interaction of the former and latter. As highlighted earlier, the ideal proxy should accurately predict the tax reduction, provide sufficient variation, and, importantly, be persistent over time. From these three proxies, only the labor share in the tax base fulfills the three requirements. The business tax rate alone and the interaction of the tax rate and the labor share have limited variation, with standard deviations of 0.063 and 0.057, respectively. In contrast, the labor share has a standard deviation

of 0.199 (see in Appendix Table A.4). Second, the prediction power of these two is limited compared to the labor share: they hardly predict the tax change between 1998 and 2001 (see in Appendix Figure B.2 and B.3), while the labor share smoothly predicts it (see Figure 3). Table A.5 in Appendix, further shows that the labor share explains more of the variance in the taxes paid than the alternative proxies.

3.2 Model specification and Identification

Dynamic Differences-in-differences approach. My empirical approach leverages cross-sectional variation in firms' labor shares.[13] Hence, the strategy relies on comparing firms with different initial levels of labor intensity in 1998, before and after the reform. The baseline specification is an event study design with an effect window of four leads and eight lags, spanning 1995-2006. Formally, the model is the following:

$$\begin{aligned} \log(y_{it}) = &\sum_{k=1995}^{2006} \beta_k \times \mathbb{1}\{t = k\} \times LaborShare_i + \gamma_i \\ &+ \delta_t \times \sum_{j=1}^{5} \mathbb{1}\{LaborShare_i \in Quintile_j\} + \epsilon_{it} \end{aligned} \tag{2}$$

where $y_{i,t}$ is the outcome variable at the firm level i at time t. β_t measures the effect of having a relatively higher $LaborShare_i$ in year t. $\mathbb{1}\{t = k\}$ is a dummy variable equal to 1 for year k. I normalize the last pre-treatment coefficient, β_{1998}, to zero such that all effects are relative to 1998. γ_i is a firm fixed effect, controlling for firms' constant characteristics over the period, such as location. δ_t is the set of year-fixed effects, controlling for time-varying shocks. I interact the year fixed effects with $\sum_{j=1}^{5} \mathbb{1}\{LaborShare_i \in Quintile_j\}$, the set of dummies capturing firms' quintile rank in 1998. ϵ_{it} is the error term. Standard errors are clustered at the firm level, accounting for correlations over time. I also use a simple Differences-in-differences model to obtain average effects. When doing so, I pool together pre- and post-period observations, respectively. The set of fixed effects remains unchanged.

In the main specification, the bins correspond to the five quintiles of the distribution of labor intensity in 1998. The rationale behind including the interaction between year fixed effects and

[13] This empirical approach has been often used in the policy evaluation literature, see for example Saez et al. (2019) or Harasztosi and Lindner (2019) .

the set of dummies capturing firms' quintile rank in 1998 is that capital-intensive firms may not evolve like labor-intensive firms. So, by using these bins, I aim to bring more comparable firms together. As a result, I estimate the effect of the reform within these bins. The definition of these bins is arbitrary; therefore, I verify that the choice of bins does not drive my results, see Section 4.3.

It is worth noting that in the regression sample, I exclude firms having a labor share in the bottom and top 10^{th} percentiles. Indeed, these firms are very peculiar and can be considered outliers (see Appendix Table A.6).

Identification. The key identification assumption in this differences-in-differences type of regression is that firms with lower labor intensity are a valid estimate of the counterfactual for relatively more labor-intensive firms within a given bin. So, conditional on being within the same bin, relatively less labor-intensive firms and relatively more labor-intensive ones would follow a parallel trend in the absence of the 1999 reform. While this assumption cannot be tested directly, I test whether the parallel trends assumption holds in the pre-reform period. Reassuringly, I cannot reject the presence of differential trends in most of the specifications.

Another concern for identification is potential confounding shocks that overlap the period of interest here: the reduction in hours worked and the development of broadband internet. A significant share of the firms in the study sample have less than 20 employees, implying that they were forced to reduce the number of hours worked only in 2002. I perform additional robustness checks in Section 4.3. Regarding internet development, I also verify that my estimates are robust when including the municipality-by-year fixed effects or Labor-market-by-year fixed effects. However, labor intensity is likely to be orthogonal to the development of the Internet. Additionally, I also show that changes in the business tax rate do not affect my point estimates. The complete set of robustness tests can be found in Section 4.3.

Continuous treatment effects. Recent work of De Chaisemartin and D'Haultfœuille (2018, 2020); Callaway et al. (2024) have highlighted that differences-in-differences estimates with continuous treatment effects could be biased. In my setting, I have continuous treatment effects along the labor intensity of firms. This implies that I need to rely on the continuous

dose assumption. This assumption implies that the treatment effect is linear. Increasing the labor share by 0.1 has the same effect regardless of whether the initial labor share is 0.02 or 0.4. However, if this assumption does not hold in my setting, it is possible that my point estimates are biased.

4 Results

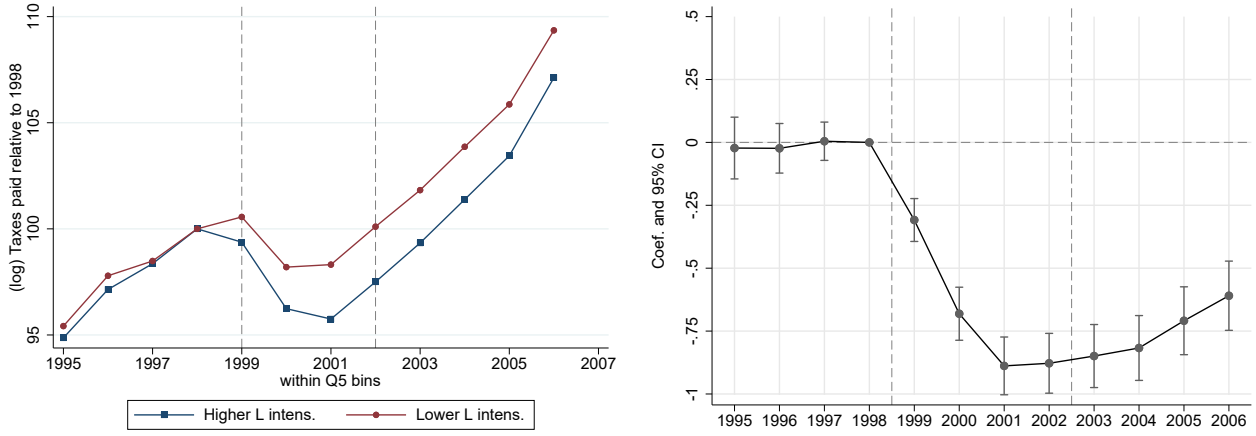
Section 4.1 shows that the labor share proxy explains the tax reduction induced by the reform and reviews the effects on labor, capital and productivity. Section 4.2 then investigates potential heterogeneity in the sample. Section 4.3 reports several robustness checks.

4.1 Baseline effects

"First stage": tax reduction. Before investigating the reform's effect on employment and capital, I verify that the correlations I underline in the previous sections can be interpreted causally. In Figure 4a, I first report averages (relative to 1998) and distinguish two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. I observe that relatively more labor-intensive firms seem to see their taxes reduced more following the reform. I find a similar pattern using the tax payment limited to the business tax; see Appendix Figure B.6.

In Figure 4b, I plot the coefficients β_k resulting from the estimation of the model (2). I normalize the coefficients and set β_{1998} to 0. To causally interpret the post-period coefficients, I first assess the plausibility of the parallel assumption – the absence of pre-trends in this setting. Looking at the pre-period coefficients (1995-1998), I note that the pre-trend is flat, and coefficients are close to 0 and not significantly different from it. Using the event study approach confirms what I observed in the previous figure. Before the reform, taxes paid by firms evolved similarly. After the reform, relatively more labor-intensive firms benefit from a significant tax reduction. I can observe the gradual phasing out of the wage bill from the tax base between the two vertical lines. The tax reduction is limited in 1999, when the tax allowance only amounted to 100,000 Francs, and then there is a more significant drop in 2002 when the

allowance equals 6,000,000 Francs. After 2002, we observe that the tax reduction gradually diminishes over time, likely because firms increased their investments following the reform, thereby expanding the tax base of the post-1999 local business tax. Differences-in-differences estimates (aggregating pre- and post-periods) suggest that a one percentage point increase in exposure to the reform leads to a reduction of 0.71%, this equivalent of a 305 euros additional tax reduction of (see Appendix Table A.7). It is important to note that the taxes paid do not encompass the corporate tax but only the local taxes, with the business tax accounting for the largest share.



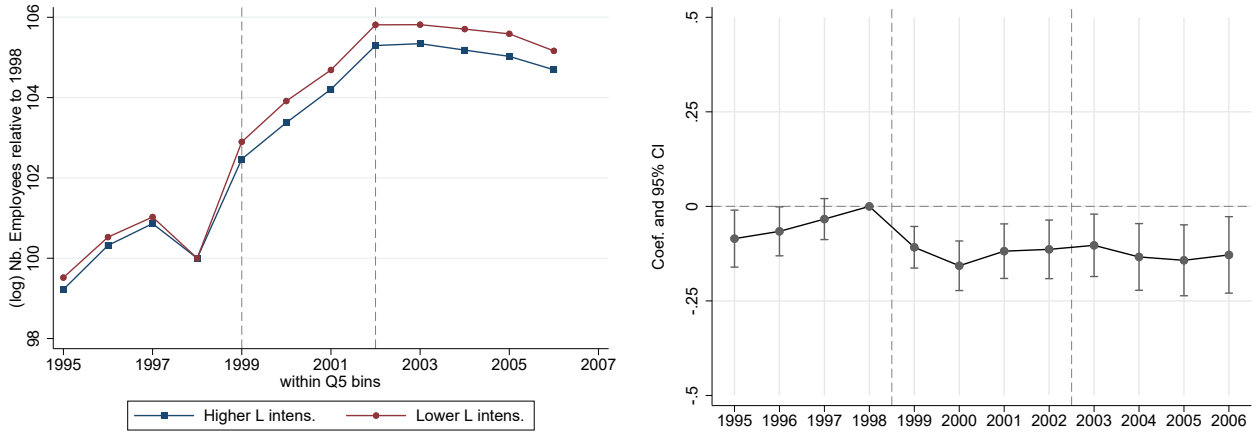
(a) Averages over time **(b) Event Study estimates**

Figure 4: Effects on local Taxes paid

Note: Panel (a) plots the average (log) taxes paid overtime relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the log of taxes paid. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

Effects on labor. I now turn to the effect on labor, presumably the policy's main target, as it aimed to foster employment. By plotting averages over time in Figure 5a, I observe that firms the most exposed to the reform did not increase employment as much as their counterfactual. In Figure 5b, I plot the coefficients resulting from the dynamic differences-in-differences model. Evaluating the pre-period coefficients (1995-1998), I note that the pre-trend is slightly increasing, and coefficients are marginally significant in 1995 and 1996. However, when testing the joint significance of the pre-treatment estimates, I find a p-value equal to 0.147, confirming the absence of a significant pre-trend. Coefficients in the post-period are significantly

negative. These results might be slightly downward biased, given the pre-trend. However, this is unlikely to account for the overall post-reform effects. Differences-in-differences estimates suggest that a one percentage point increase in exposure to the reform leads to a decrease in employment of 0.07% (see Appendix Table A.7). At first glance, this suggests that the reform did not stimulate employment. However, the negative effect on employment could be due to two elements: (1) relatively high labor-intensive firms decreasing employment, or (2) the relatively high labor-intensive firms increase employment at a lower pace. Figure 5b suggests that the second explanation is the most plausible. Relatively more labor-intensive firms do not cut employment *per se* but grow at a slower pace. For relatively less labor-intensive, employment rose by about 6% while for relatively more labor-intensive, it grew by only 5.5% between 1998 and 2002. Section 5.2 discusses potential explanations for these differences in growth.

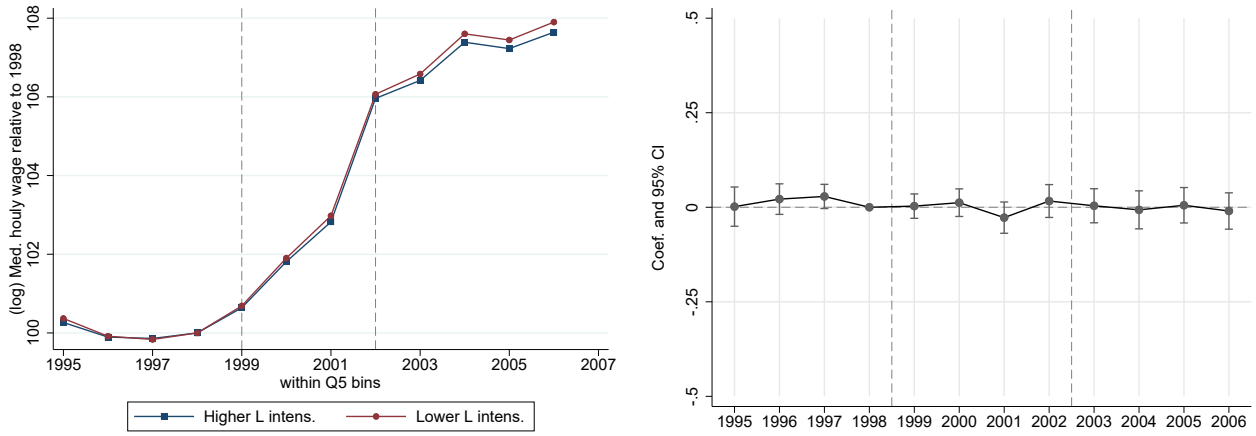


(a) Averages over time **(b) Event study estimates**

Figure 5: Effects on Employment

Note: Panel (a) plots the average (log) number of employees overtime relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the log number of employees. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

As canonical tax incidence models suggest, wages might be affected. Therefore, I investigate the effect of tax reductions on the median gross hourly wage. Although the averages over time suggest a slight decrease after 2002 (see Figure 6a), I do not find a significant effect in the event study (see Figure 6b). The reform did not prompt firms to increase their salaries.



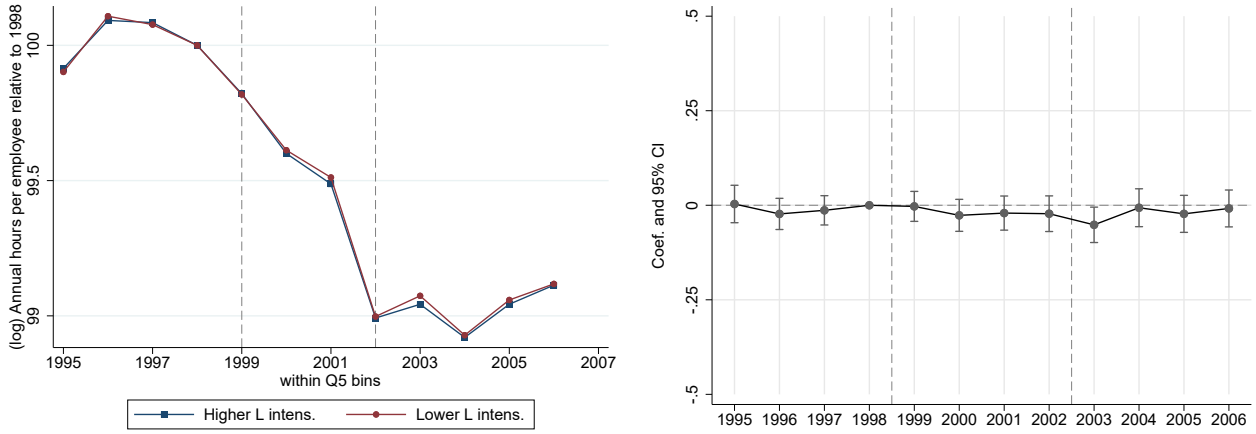
(a) Averages over time **(b) Event study estimates**

Figure 6: Effects on Median hourly wages

Note: Panel (a) plots the average (log) median hourly gross wage overtime relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 3. There is a large increase between 2001 and 2002; this is due to a change in the way wages are reported; this is not problematic for the study as this change affects all firms from 2002 onwards, and I control for year-fixed effects in the model. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the log median hourly gross wage. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

I also investigate whether hours worked per worker vary after the reform (see Figure 7). As labor becomes relatively cheaper, firms could select a group of employees and make them work additional hours. I do not find a significant effect. The coefficient in 2003 is marginally significant and negative. However, the pooled estimate is insignificant (see Appendix Table A.8). The absence of wage or hours adjustment is not very surprising given the context of France; wages are known to be rigid, and hours worked are relatively well regulated. However, it is essential to note the absence of a response in hours worked per worker supports the identification strategy as it suggests that the labor share in 1998 does not predict the reduction in working time implemented between 2000 and 2002. This is, however, not sufficient to claim that this reform is not affecting my point estimates; I, therefore, carry out additional tests in Section 4.3.

Effects on capital. Although the tax reform targets only labor and capital remains taxed after the reform, firms can still use this tax windfall to invest. I, therefore, investigate the effect on the stock of capital defined by the sum of tangible assets and investment in tangible



(a) Averages over time **(b) Event study estimates**

Figure 7: Effects on Annual hours worked per employee

Note: Panel (a) plots the average (log) number of annual hours worked per employee overtime relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the log number of annual hours worked per employee. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

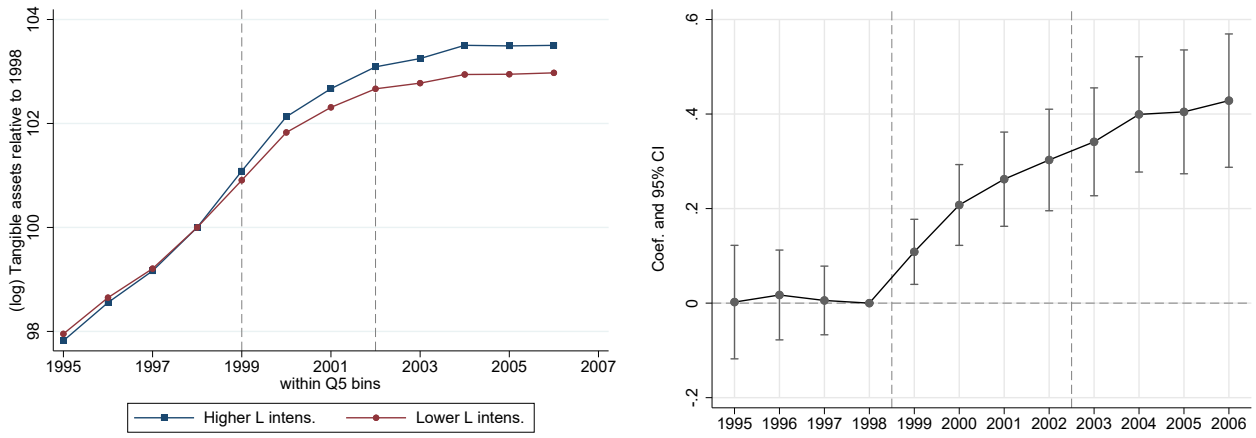
assets.[14] As one can see in Figure 8, the stock of capital of relatively more and relatively less labor-intensive firms evolve similarly in the pre-reform periods. This translates into coefficients close to 0, not significantly different from 0 in the period 1995-1998 in event study estimates. When the reform unfolds, the stock of capital significantly increases. Differences-in-differences estimates suggest that a one percentage point increase in exposure to the reform leads to an increase in capital of 0.3% (see Appendix Table A.7). More precisely, for an additional percentage point in exposure to the reform, firms invest an additional 2,060 euros. Or, expressed in terms of tax reduction, for a one percent reduction in taxes paid, the investment increases by 0.43%.[15] This effect is comparable to the investment responses measured by Saez et al. (2019), who find an elasticity close to 0.49.[16] My estimates are larger than that of Duan and Moon (2024); they find an elasticity to the net of tax of 0.64 in the context of corporate taxation and manufacturers. Tax incentive targeting capital report elasticity to the net of tax much larger

[14] In Appendix Figure B.7, I test the robustness of my results with alternative outcomes definitions.

[15] I first compute the elasticity of capital to labor intensity based on the Differences-in-differences estimates, see in Appendix Table A.9, and compute the elasticity of capital to the taxes paid (ξ_τ^K) defined by $\xi_\tau^K = \frac{\xi_K^{Lsh}}{\xi_\tau^{Lsh}}$: the ratio of the elasticity of capital to the labor intensity (ξ_K^{Lsh}) over the elasticity of taxes paid to the labor intensity (ξ_τ^{Lsh}).

[16] Saez et al. (2019) find an increase of 6% in tangible assets for a tax reduction of 12.1%.

from 3.9 or 6.5 (rescpectivily Zwick and Mahon, 2017; Ohrn, 2018).[17] However, their contexts are quite different as the policy targets capital directly and forces firms to invest to benefit from the tax reduction.



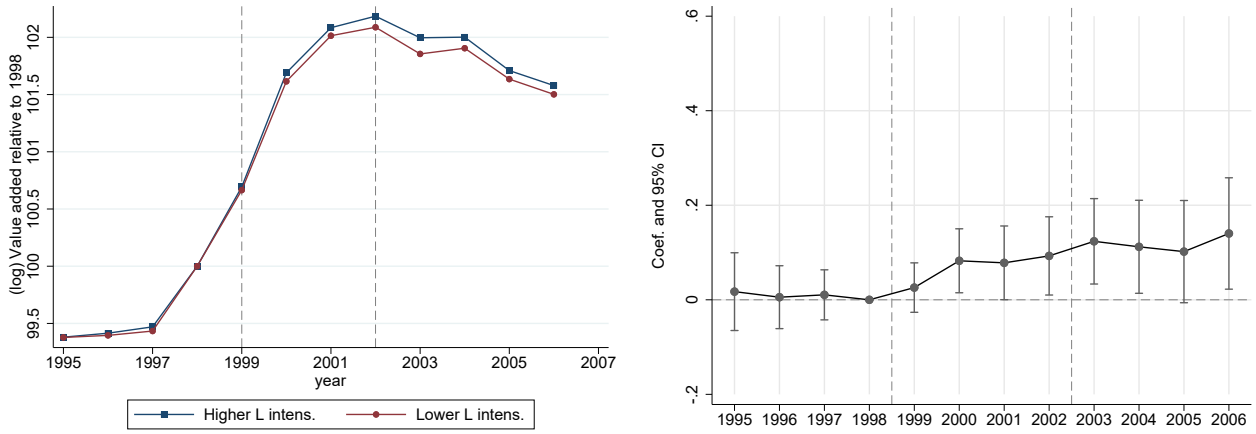
(a) Averages over time **(b) Event study estimates**

Figure 8: Effects on Capital

Note: Panel (a) plots the average (log) tangible assets over time relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the log of tangible assets. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

Effects on productivity. I investigate whether the reform affects the productivity of firms. To do so, I estimate the effect of the reform on the log of value-added; see Figure 9b. I observe a flat pre-trend and a significant increase in the value-added produced in the post-reform period. Productivity increases significantly after the reform. However, differences-in-differences estimates suggest that a one percentage point increase in exposure to the reform leads to an increase in productivity per employee of 0.08% (see Appendix Table A.8). Post-reform coefficients evolve similarly to the coefficients of the regression for the tangible assets; this suggests that the productivity boost is likely the result of the strong investments. I also scaled the effect on value-added, estimating the impact of the reform on the value added produced divided by the number of employees. Figure B.8b in the Appendix displays the results and suggests an increase in labor productivity.

[17]These elasticities to the net of tax correspond roughly to elasticity to the tax rate of 0.053 for Duan and Moon (2024), 3.52 for Ohrn (2018).



(a) Averages over time **(b) Event study estimates**

Figure 9: Effects on Productivity

Note: Panel (a) plots the average (log) Value added divided by the number of employees over time relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the log of the value-added. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

4.2 Heterogeneity

Effects by quintiles. Table 2 reports point estimates of the Differences-in-Differences model interacted with the five quintiles of the labor share distribution in 1998. Focusing on the effect on taxes in Table 2 column (1), I show that all quintiles of the distribution benefited from a significant tax reduction. Effects are approximately constant across quintiles. In column (2), I report the effect on employment: the negative effect is driven by the lower part of the labor share distribution (Q1 and Q2), the firms relatively more capital intensive than the rest of the sample. The effect on tangible assets, on the other hand, is held across the overall distribution, except quintile 1, but still has a positive coefficient. The effect on the value-added is less clear, with point estimates only significant for quintiles 3 and 4. However, when scaling the values added produced by the number of workers, I obtain a positive and significant effect for most quintiles. This latter outcome is reported in Appendix Table A.10 together with additional outcomes.

Table 2: Differences-in-Differences estimates by quintiles of labor intensity

	(1)	(2)	(3)	(4)
	(log) Taxes paid	(log) Nb. Employees	(log) Tangible assets	(log) Value added
1^{st} Quintile Labor Sh.	-0.632***	-0.348**	0.033	-0.052
	(0.242)	(0.160)	(0.204)	(0.190)
2^{nd} Quintile Labor Sh.	-0.812***	-0.143**	0.259***	-0.053
	(0.112)	(0.068)	(0.096)	(0.078)
3^{rd} Quintile Labor Sh.	-0.646***	-0.080	0.385***	0.169**
	(0.117)	(0.070)	(0.105)	(0.080)
4^{th} Quintile Labor Sh.	-0.744***	-0.004	0.254***	0.131**
	(0.096)	(0.056)	(0.094)	(0.065)
5^{th} Quintile Labor Sh.	-0.475**	-0.070	0.522**	0.135
	(0.212)	(0.122)	(0.227)	(0.141)
Firm FE	✓	✓	✓	✓
Q5 x Year FE	✓	✓	✓	✓
Obs.	663924	663924	663909	662827
R2	1	1	1	0.931

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with the quintile ranks of firms. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of taxes paid (1), the log of the number of employees (2), the log of tangible assets (3), and the log of the value-added. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Effects across industries. Table 3 reports point estimates of the Differences-in-Differences model interacted with the industry for the main industries; the other industries can be found in Appendix Table A.11. Focusing on the effect on taxes in Table 3 column (1), I estimate that all industries benefited from a significant tax reduction. In column (2), I report the effect on employment. The construction and trade sectors have a negative effect. This suggests that apart from the labor intensity, the industry also matters for the response in terms of employment. The construction and trade sectors are not more represented in the lower quintiles of the labor share distribution (see Table A.1); therefore, it seems to be another source of heterogeneity. In column (3), we note an increase in investment across all sectors, with a more extensive response for the Hospitality sector. The last column of the Table suggests that the productivity effect is driven by Manufacturers. However, when measuring the value added per worker, I estimate significant and positive effects across all industries. In Appendix Table A.12 column (1), I find a negative effect that is small but statistically significant on the median hourly wage. This effect could result from a change in wages or the composition of the workforce, as firms with the largest reduction in median hourly wage also change employment the most.

Table 3: Differences-in-Differences estimates by industry

	(1) (log) Taxes paid	(2) (log) Nb. Employees	(3) (log) Tangible assets	(4) (log) Value-added
C-Manufacturers	-0.518***	-0.044	0.333***	0.123***
	(0.062)	(0.038)	(0.059)	(0.043)
F-Construction	-1.100***	-0.132***	0.177***	-0.044
	(0.064)	(0.039)	(0.061)	(0.044)
G-Trade	-0.705***	-0.130***	0.434***	-0.016
	(0.061)	(0.036)	(0.057)	(0.042)
I-Hospitality	-0.537***	-0.066	0.0739***	0.078
	(0.077)	(0.045)	(0.079)	(0.053)
Firm FE	✓	✓	✓	✓
Q5 x Year FE	✓	✓	✓	✓
Indus. x Year FE	✓	✓	✓	✓
Obs.	654780	654780	654765	653685
R2	0.921	0.945	0.929	0.932

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with industry dummies. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of taxes paid (1), the log of the number of employees (2), the log of tangible assets (3), and the log of the value added (4). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Effects by firms' size. Table 4 reports point estimates of the Differences-in-Differences model interacted with the firms' size in 1998; additional outcomes can be found in Appendix Table A.13. Focusing on the effect on taxes in Table 4 column (1), I show that no matter the size of the firms, they all benefit from a tax reduction, although the reduction is more substantial for smaller firms. This is not surprising, given that smaller firms stopped paying taxes on labor earlier than larger ones. In column (2), I report the effect on employment. There, I observe that the negative effect is driven by the smaller and the largest firms. In column (3), I estimate an increase in investment across all firms. Smaller firms drive the results with an effect on tangible assets twice as large as the effect for the largest firms. As smaller firms tend to be more cash-constrained than larger ones, these differences could be explained by a difference in initial liquidity. This potential mechanism is further discussed in the next section. Last, in column (4), the results highlight an increase in overall productivity in all firms except the largest one. The labor productivity, however, increases for all firms (see Appendix Table A.12).

Table 4: Differences-in-Differences estimates by size in 1998

	(1)	(2)	(3)	(4)
	(log) Taxes paid	(log) Nb. Employees	(log) Tangible assets	(log) Value-Added
3-9 employees	-0.836***	-0.076**	0.373***	0.107***
	(0.059)	(0.035)	(0.055)	(0.040)
10-19 employees	-0.562***	-0.057	0.218***	0.053
	(0.062)	(0.038)	(0.059)	(0.044)
20-49 employees	-0.491***	-0.075*	0.149**	0.103**
	(0.064)	(0.040)	(0.063)	(0.046)
50+ employees	-0.355***	-0.188***	0.185**	-0.090*
	(0.067)	(0.057)	(0.073)	(0.054)
Firm FE	✓	✓	✓	✓
Q5 x Year FE	✓	✓	✓	✓
Size x Year FE	✓	✓	✓	✓
Obs.	663924	663924	663909	662827
R2	0.920	0.945	0.928	0.931

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with firm-size dummies. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of taxes paid (1), the log of the number of employees (2), the log of tangible assets (3), and the log of the value added (4). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Overall, the heterogeneity analysis highlights that the tax reduction and investment responses are quite homogeneous across the sample. In contrast, the effects on the labor force are more pronounced for relatively more capital-intensive firms, the trade and construction sectors, and very small and large firms.

4.3 Robustness

Bins. As underlined in the previous section, I verify that my point estimates are not the results of bin choices. In Appendix Figure B.9, I report point estimates with different bins: bins using the four quartiles, the eight percentiles, the ten percentiles of the distribution, and no bins. It is important to note here that bins are required to bring together firms with pre-reform similar evolution, particularly in employment. Post-reform estimates with different bins and without bins are in the same ballpark. Only standard errors are affected; the smaller the bins, the larger the standard errors. This is not surprising as variation within smaller bins and the number of observations are mechanically reduced, inducing a loss in precision. Overall,

these suggest that the choice of bins does not drive my results but helps only to bring more comparable firms together.

Measure of labor share. In the main results, I use the labor share reported in 1998. The advantage is that this is as close as possible to the actual exposure to the reform. However, as this is a value at one point in time, it is more prone to mean reversion and potentially more sensitive. I, therefore, test the robustness of my results by changing the period I use to compute the labor share. In Appendix Figure B.11, I report point estimates for all outcomes using the labor share calculated in 1997 and the average labor share in the pre-reform period (1995-1998). The point estimates of employment and taxes paid are hardly affected by a change in the computation of labor share (Appendix Figure B.11a and Figure B.11b, respectively). Regarding tangible assets, pre-reform period coefficients significantly differ from 0 for the two alternative measures. However, we still observe a clear trend break in the pre- vs. post-period, and the post-reform point estimates have similar values to the baseline estimations (Appendix Figure B.11e).

Local shocks. Another concern for identification is confounding shocks that coincide with the reform, but have no visible effect before 1999. So in Appendix Figure B.12, I check the sensitivity of the main results to such shocks and include area-by-year fixed effects at different levels (at the local labor market or the county level) or industry-by-year fixed effects (using 2-digit industry classification), which account for time-varying economic and/or political shocks. Point estimates remain very similar across the different specifications. Systematic local shocks do not seem to threaten the identification strategy. Here, three things are worth noting. First, these industry shocks also control non-parametrically for the explosion of the "dot-com" bubble, as some industries were likely to be more or less affected by this crisis. This mitigates the concern that this shock could have a negative effect on employment. I also check the robustness of my results when excluding the telecommunication sector (see Appendix Figure B.10). Second, including area-by-year fixed effects, especially at the municipal and local labor market level, should mitigate concerns, according to which my estimations would be driven by firms located in areas that were first to access broadband internet. In addition, Malgouyres et al. (2021)

highlights that although the development had started in 1999, the development of broadband internet across France was slow until 2002; only 2% of the cities were covered in 2000. Third, municipality-by-year fixed effects control for a change in the tax rate and, therefore, support that the estimations are not the result of tax rate differences across firms.

Reduction in working time. In 2000, a mandatory reduction in working time was enforced, although it followed a voluntary transition period. From 1998 to 2000, firms were incentivized with subsidies to voluntarily reduce full-time working hours from 39 to 35 per week. In 2000, the change became mandatory for larger firms, and the subsidies ended. For smaller firms (with fewer than 20 employees), the switch to 35 hours became mandatory in 2002. The reduction in working time was uniformly enforced by law throughout France. It was applied differently according to industries and depending on negotiations with the various branch agreements. As this was a major reform in the French labor market, I carry out several tests to ensure that my point estimates are not confounded by the reduction in working hours. Two key points are worth highlighting. First, the reduction in working time reform could undermine my identification strategy if the labor share correlates with the timing of the reduction in working time. I do not observe differences in evolution between relatively low and high labor share firms within bins (see Figure 7a). Second, in the previous section, I found no significant effect on hours worked per worker in the baseline results (Figure 7b).[18] This is already reassuring as it suggests that the labor share is not a good proxy for the exposure to the reduction in working time.

To further address concerns that the reduction in working hours could be a confounding factor, I conduct several tests. I add various control variables that should, at least partially, account for the reduction in working hours in case my results were affected. I control for firms' size in 1998 (Appendix Figure B.13), the hours worked in the previous year (expressed in hours per year or week), and the year of plausible transition.[19] The results remain consistent with the baseline across all outcomes (Appendix Figure B.14). Additionally, I restrict the sample

[18]Only the coefficient in 2003 is significantly different from 0 at 5% confidence level. However, in 2003, all firms have already transitioned to 35 hours per week. This is, therefore, unlikely to be the result of the reduction in working time. In addition, the effect vanishes when estimating the simple Differences-in-differences model (see Table A.8).

[19]To identify the year of plausible transition, I compute the median number of hours worked a week per worker every year and define the year of transition as the first year this median is below or equal to 35.

to firms with fewer than 20 employees, as these firms were required to switch to the 35-hour workweek in 2002. If the reduction in working hours impacted my baseline results, we would expect different results when focusing on smaller firms. However, as shown in Appendix Figure B.15, the estimates remain consistent with the baseline. These robustness checks alleviate some concerns about the reduction in working hours as a potential confounder. Still, they do not definitively prove that the change in working hours is orthogonal to the 1999 reform. To my knowledge, there is no consensus on how the reduction in working time affected employment (Chemin and Wasmer, 2009).

Further robustness checks. I report additional sensitivity checks to ensure that my results are not driven by modeling assumptions. First, as Differences-in-differences models often provide biased standard errors (Bertrand et al., 2004), I verify the robustness of my results by clustering the standard errors at higher levels than the firm level (as suggested by Angrist and Pischke, 2009). In Appendix Figure B.16, I show that the standard errors are hardly affected by a change in the clustering level (at the municipal or the labor market level). Second, I also change the sensitivity of the estimation to the window of the event study, and I find similar effects (Appendix Figure B.17). In

5 Potential Mechanisms

Section 5.1 highlights that the investment effect is driven by firms that were initially cash-constrained. Section 5.2 details potential explanations for the negative effect on employment.

5.1 The role of credit constraints

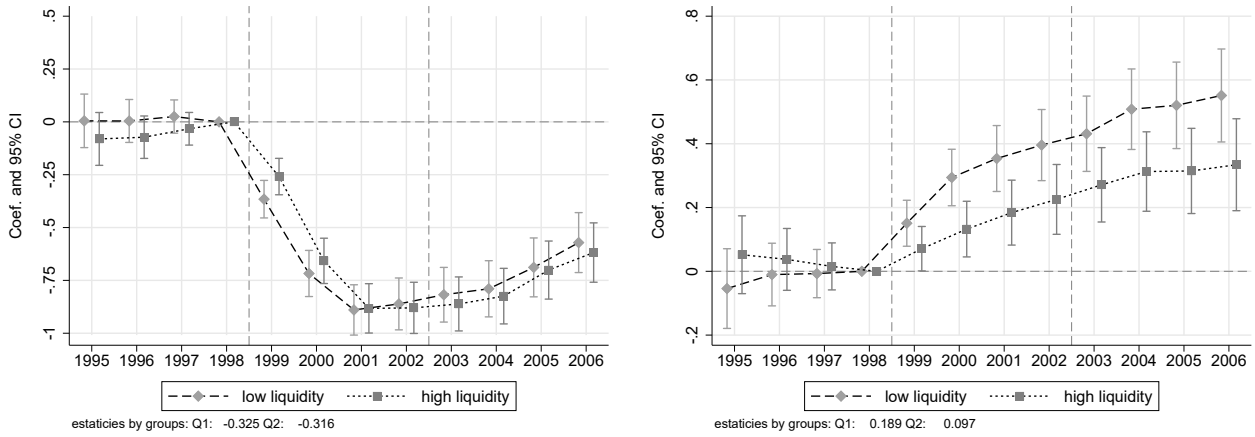
So far, I have demonstrated that following the change in the tax base in 1999, firms benefited from a significant tax windfall and used it to invest at the expense of employees substantially. This generates a significant productivity improvement. Given that the investment generated gains in productivity, this raises the following question: why did firms delay such investments? The reform did not affect the marginal cost of investment, as the taxation of capital before

and after 1999 remained the same. On the other hand, the reform generated substantial tax windfall, particularly for smaller firms.

Modigliani and Miller (1958) predict that if external financing is more costly than internal financing, cash injections should have a positive effect on capital expenditures.[20] Evidence from the public finance literature highlights that cash injections affect firms' growth potentially due to credit constraints. More recent evidence in payroll tax literature also suggests similar mechanisms (Benzarti and Harju, 2021a,b). To assess whether credit constraints influence the firm-level effects I have identified, I adopt a split-sample strategy similar to the one used by Benzarti and Harju (2021a). Specifically, I sort firms into financially constrained and unconstrained categories based on whether their ratio of liquid assets to total assets in 1998 is below or above the sample median.[21] I still compare firms within quintiles of labor share and further divide each quintile into constrained and unconstrained groups for comparison.

Figure 10 reports the coefficient estimated using the fully interacted baseline model with a dummy indicating whether the firm is financially constrained in the year before the reform. In Figure 10a, we see that whether a firm is initially financially constrained hardly affects the tax reduction it faces. Both cash-constrained and unconstrained firms benefit from a significant and similar tax windfall. This mitigates concerns that the heterogeneous effects of financial constraints could result from a difference in the exposure and tax windfall between relatively high and less labor intensive, compared to unconstrained firms. I further report averages of labor shares by financially constrained status and show that differences in labor shares between relatively high and less labor-intensive are similar within quintiles (see Appendix Table A.14). In Figure 10b, on the other hand, we observe that cash-constrained firms invest significantly more after the reform; the effects are twice as large for the cash-constrained firms. Therefore, these results are consistent with the credit constraint channel. This also translates into higher productivity gains for financially constrained firms (see Appendix Table A.15).[22] However, I do not find significant differences in the effect on employment.

[20] External financing could be costly because of asymmetric information or incomplete contracting.
[21] I use the value of circulating assets divided by the total assets net of depreciation.
[22] Event study estimates for all outcomes can be found in Appendix Figure B.18.



(a) Effects on taxes paid **(b) Effects on tangible assets**

Figure 10: Effects by liquidity constraint

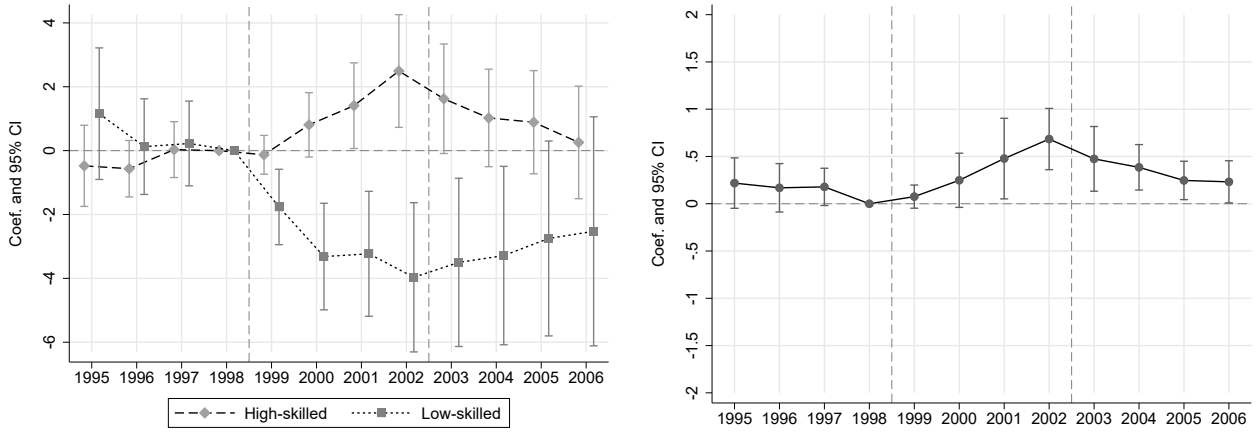
Note: Panel (a) and (b) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) fully interacted with a dummy indicating whether the liquidity of firms is below or above the sample's median. The dependent variable is the log of taxes paid in panel (a) and the log of tangible assets in panel (b). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

5.2 Explaining the negative effect on employment

Several studies find positive effects on employment from corporate tax cuts (Duan and Moon, 2023), payroll tax reductions (Saez et al., 2019; Benzarti and Harju, 2021a), or no effect at all (Harju et al., 2022). Tax incentives targeting capital also tend to boost employment (Curtis et al., 2022; Duan and Moon, 2024), but these policies typically require firms to invest in specific assets to receive benefits, making them more likely to stimulate activity. In contrast, in my case, tax reductions occur regardless of whether firms invest or hire. This section examines two potential mechanisms behind the negative employment effect: substitution between high- and low-skilled workers and differences in tax sensitivity.

Change in the composition of the workforce. By providing a substantial tax windfall and enabling firms to reallocate labor more freely, firms might use this opportunity to adjust their production technology to gain productivity. Regarding the workforce, this could translate into hiring more high-skilled workers while reducing reliance on low-skilled labor. This would explain the negative effect on overall employment if firms substitute a single high-skilled worker for several low-skilled workers. To investigate this mechanism, I sort workers between high-

and low-skilled.[23] For these outcomes, I use the raw numbers rather than log transformations, as some firms may have no workers in one of these categories. Figure 11a shows that before the reform, the number of high- and low-skilled workers evolved similarly across firms. Post-reform, firms relatively more exposed to the reform decrease the number of low-skilled employees significantly. In contrast, the number of highly skilled workers increases only significantly around 2001, when the tax allowance becomes more substantial (see Table 1). Additionally, Figure 11b illustrates that the skill ratio – defined as the number of high-skilled workers relative to low-skilled workers at the firm level – increases post-reform, confirming that this substitution mechanism occurs within firms. I estimate that for a one percentage point increase in exposure to the reform, the number of high-skilled workers increases by 0.01, while that of low-skilled workers shrinks by about 0.03 (see Appendix Table A.17). This roughly indicates that firms replace about three low-skilled with one high-skilled employee, explaining the overall decline in the total workforce.[24]



(a) Effects on workers by skill groups **(b) Effects on the skill ratio**

Figure 11: Effects by skill level

Note: Panel (a) and (b) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the number of high-skilled and low-skilled workers, respectively, in panel (a), and the skill ratio in panel (b) is defined as the number of high-skilled workers relative to low-skilled workers at the firm level. I define high-skilled workers as those who earn more than two times the minimum hourly wages and low-skilled workers as those under (as detailed in Section 2.3). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

[23] I define high-skilled workers as those who earn more than two times the minimum hourly wages and low-skilled workers as those under (as detailed in Section 2.3).

[24] To obtain such numbers, I use the DiD estimates of Appendix Table A.17, compute the related elasticities, and compute the absolute variation based on the pre-reform averages.

Overall, the evidence suggests that firms leverage the reform to improve their production technologies, increasing investment and shifting their labor force toward higher-skilled workers likely to complement the new technology. This aligns with recent findings on the effects of tax incentives targeting capital (Tuzel and Zhang, 2021; Duan and Moon, 2023) and further highlights that tax reforms can accelerate skill-biased technological change, even when the tax reduction does not directly target capital. These results might contribute to our understanding of the mechanisms fostering skill-biased technological change (Acemoglu, 2011), a phenomenon that often exacerbates skill-based inequalities. Additional work is needed to investigate the composition of capital firms invested in.

Differences in tax sensitivity. A potential explanation for this difference in employment growth is the possibility of deducting the business tax payment from the corporate income tax depending on firms' profitability. Profitable firms pay corporate tax and can, therefore, deduct the tax payment related to the local business tax from the corporate tax base. The larger the business taxes paid, the lower the corporate tax payment. On the other hand, unprofitable firms do not pay corporate taxes – as these taxes are based on profits – and fully bear the burden of the local business tax. In a sense, firms not paying corporate taxes should be more sensitive to changes in business taxes, as they will fully incur the cost of it. To show this mechanism, I derive a simple model in Appendix E. This exercise tells us that when facing a tax on labor, firms will reduce labor and capital. However, the more the firm can deduct the wage bill tax from the corporate tax, the less sensitive the firm will be to the tax on the wage bill. This is because the deduction in the corporate tax partially offsets the cost of the wage bill tax. Transferring the conclusions of this model to my empirical setting is straightforward and generates the following predictions: removing the wage bill from the business tax base should increase employment and capital, and, interestingly, firms that can deduct the business tax from their corporate tax should react less to the reform as the change in their production cost will be smaller.

Going back to the data, I plot in Figure 12 the average number of employees over time, distinguishing two groups: the profitable firms paying the corporate tax in 1998 and those that do not make profits and, therefore, do not pay corporate taxes. While firms have relatively

similar evolutions in the pre-reform period, we note that following the reform, the employment growth of firms that are not profitable is larger than that of those that are profitable.[25] This suggests that the interaction between the business tax liabilities and its deductibility in the corporate tax base is a plausible mechanism explaining the growth difference in employment between relatively high and low labor-intensive firms.

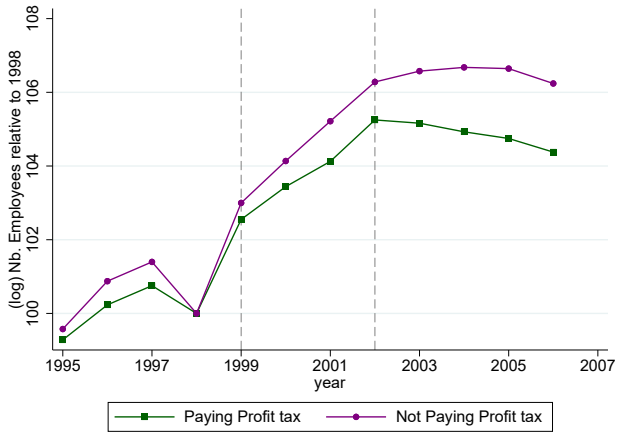


Figure 12: Employment effect by corporate tax liabilities – averages over time

Note: the figure plots the average (log) number of employees over time relative to 1998. I consider two groups of firms according to whether firms are profitable and pay the corporate tax in 1998. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

The heterogeneity analysis has shown that the negative employment effects are driven by (i) capital-intensive firms in the first two quintiles of the labor intensity distribution (Table 2), (ii) firms in the construction and trade sectors (Table 3), and (iii) firms with fewer than ten or more than 50 employees (Table 4). Appendix Table A.16 shows that firms not paying corporate tax in 1998 are more likely to belong to these groups, as they are overrepresented in the construction, trade, and hospitality sectors, have fewer than ten employees, and fall within the first two quartiles of the labor share distribution. Additionally, these firms are concentrated in the lower range of the within-bin labor share distribution. This could explain the strong negative effects observed for these groups and supports the model's insight that firms unable to deduct corporate taxes are more responsive to the reform.

[25] Averages by quintiles are displayed in Appendix Figure B.19, we note that the differences in pre-trends come mainly from the 5th quintiles; firms have otherwise similar paths in the pre-reform period. The first and second quintiles particularly drive the post-reform effects.

6 Conclusion

In conclusion, my study supports the theoretical argument that taxing inputs distorts firm behavior and creates inefficiencies. In particular, a high tax burden on labor can constrain firms' employment decisions, preventing them from hiring and retaining more productive workers. My findings provide additional evidence that reducing taxes on labor affects not only employment but also other relevant margins, such as investment. This aligns with previous literature on payroll taxes (e.g., Saez et al. 2019) and contradicts the predictions of the canonical models of tax incidence as wages remain unaffected. My results also support recent findings on which tax reductions are particularly impactful for cash-constrained firms (Benzarti and Harju, 2021b). By providing a substantial tax windfall and enabling firms to reallocate labor more freely, the tax reform appears to have supported overall productivity growth.

From a business perspective, the reform offers clear advantages: it allows firms to hire more skilled workers, invest in new capital, and achieve greater productivity. These changes could ultimately lead to higher tax revenues. However, the reform has also caused a notable shift in labor demand from low-skilled to high-skilled workers, significantly reducing low-skilled employment and widening the skill wage gap. This may contribute to greater inequalities, highlighting the need for further investigation at the local labor market level and potentially additional redistribution measures to address these disparities.

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A Additional Tables

Table A.1: Characteristics of firms in 1998 across quintiles

	Q1	Q2	Q3	Q4	Q5
Labor share	0.253	0.344	0.455	0.579	0.698
	(0.020)	(0.033)	(0.032)	(0.040)	(0.028)
Tax rate	0.226	0.228	0.229	0.231	0.232
	(0.062)	(0.062)	(0.062)	(0.063)	(0.063)
Nb. Employees	14.560	16.533	16.901	16.778	18.036
	(17.119)	(23.571)	(28.900)	(34.217)	(46.471)
Median hourly wage (in €)	10.093	10.459	10.897	11.494	12.243
	(2.709)	(2.826)	(2.904)	(3.626)	(4.354)
Annual hours per employee	1677.969	1693.080	1726.943	1739.114	1739.034
	(304.870)	(282.127)	(257.830)	(253.238)	(248.240)
Wage bill (in k €)	299.952	351.761	384.382	413.813	474.546
	(389.707)	(536.779)	(693.561)	(928.448)	(1327.454)
Tangible assets (in k€)	889.930	674.928	463.374	302.693	206.406
	(1150.808)	(1019.709)	(829.187)	(667.876)	(574.088)
Tangible assets + Investment (in k€)	974.158	742.777	514.314	339.676	234.231
	(1261.504)	(1124.280)	(921.521)	(751.384)	(647.710)
Taxes paid (in k€)	49.999	48.853	43.344	38.179	36.174
	(68.631)	(71.872)	(66.096)	(59.058)	(56.240)
Value added at factor costs (in k €)	701.929	729.478	694.148	654.748	655.579
	(919.429)	(992.335)	(942.893)	(877.587)	(1239.176)
Observations	6916	13832	13831	13832	6916

Note: This table reports report averages and standard deviations in parentheses of different variables across the five quintiles of the labor share distribution in 1998 in the estimation sample. All monetary values are reported in 2010 euros. The wage bill, the tangible assets, investment, and taxes paid are reported in thousands of euros.

Table A.2: Additional characteristics of firms in 1998 across quintiles

	Q1	Q2	Q3	Q4	Q5
Industries *(Share of plants)*					
C - Manufacturing	0.272	0.256	0.245	0.220	0.194
	(0.445)	(0.436)	(0.430)	(0.415)	(0.395)
F - Construction	0.088	0.123	0.181	0.261	0.296
	(0.283)	(0.329)	(0.385)	(0.439)	(0.457)
G - Trade	0.353	0.411	0.406	0.346	0.291
	(0.478)	(0.492)	(0.491)	(0.476)	(0.454)
H - Transport and Warehousing	0.068	0.048	0.037	0.030	0.030
	(0.252)	(0.213)	(0.188)	(0.170)	(0.170)
I - Hospitality	0.155	0.106	0.067	0.041	0.022
	(0.362)	(0.308)	(0.250)	(0.198)	(0.146)
J - Information and Communications	0.007	0.004	0.006	0.010	0.020
	(0.080)	(0.066)	(0.077)	(0.098)	(0.141)
L - Real Estate	0.031	0.027	0.024	0.029	0.038
	(0.173)	(0.162)	(0.153)	(0.169)	(0.192)
M - Specialized Activities	0.017	0.018	0.022	0.045	0.082
	(0.131)	(0.132)	(0.148)	(0.207)	(0.274)
N - Administrative Services	0.009	0.007	0.012	0.018	0.028
	(0.094)	(0.084)	(0.109)	(0.133)	(0.164)
Firm size *(Share of plants)*					
[3;9] Employees	0.607	0.594	0.591	0.591	0.581
	(0.488)	(0.491)	(0.492)	(0.492)	(0.494)
[10;19] Employees	0.190	0.196	0.223	0.240	0.254
	(0.393)	(0.397)	(0.416)	(0.427)	(0.435)
[20;49] Employees	0.146	0.141	0.130	0.125	0.125
	(0.353)	(0.348)	(0.336)	(0.331)	(0.330)
50+ Employees	0.057	0.069	0.057	0.044	0.041
	(0.232)	(0.254)	(0.231)	(0.206)	(0.198)
Share of profitable plants	0.631	0.651	0.708	0.752	0.752
	(0.482)	(0.477)	(0.455)	(0.432)	(0.413)
Type of workers *(Share of workers)*					
MW earners	0.089	0.098	0.089	0.078	0.067
	(0.156)	(0.150)	(0.137)	(0.125)	(0.116)
Low-skilled workers	0.789	0.804	0.779	0.736	0.685
	(0.228)	(0.204)	(0.207)	(0.227)	(0.250)
High-skilled workers	0.208	0.193	0.218	0.260	0.310
	(0.226)	(0.202)	(0.205)	(0.225)	(0.249)
Observations	6916	13832	13831	13832	6916

Note: This table reports report averages and standard deviations in parentheses of different variables across the five quintiles of the labor share distribution in 1998 in the estimation sample. All monetary values are reported in 2010 euros. The total sales, circulating assets, total net assets, value-added, and gross operating surplus in thousands of euros. Industries and firms' size are reported as the share of plants within quintiles.

Table A.3: Characteristics of the workforce in 1998 across quintiles

	Q1	Q2	Q3	Q4	Q5
Industries *(Share of plants)*					
C - Manufacturing	0.272	0.256	0.245	0.220	0.194
	(0.445)	(0.436)	(0.430)	(0.415)	(0.395)
F - Construction	0.088	0.123	0.181	0.261	0.296
	(0.283)	(0.329)	(0.385)	(0.439)	(0.457)
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	(0.080)	(0.066)	(0.077)	(0.098)	(0.141)
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	(0.173)	(0.162)	(0.153)	(0.169)	(0.192)
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Observations	6916	13832	13831	13832	6916

Note: This table reports report averages and standard deviations in parentheses of different variables across the five quintiles of the labor share distribution in 1998 in the estimation sample. All monetary values are reported in 2010 euros. The total sales, circulating assets, total net assets, value-added, and gross operating surplus in thousands of euros. Industries and firms' size are reported as the share of plants within quintiles.

Table A.4: Descriptive statistics of the different measures of the exposure to the reform

	Mean	Var.	Sd.	p10	p50	p90	Min.	Max.
Labor share in 1998	0.469	0.040	0.199	0.215	0.454	0.749	0.007	1
Tax rate in 1998	0.229	0.004	0.063	0.143	0.225	0.322	0	0.439
Tax rate × Labor share in 1998	0.108	0.003	0.057	0.043	0.099	0.187	0	0.497
Observations	829896							

Note: This table reports the mean, variance, standard deviation, 10th, 50th, and 90th percentiles of the different measures of the exposure to the reform.

Table A.5: Contribution of the different measures of the exposure to the reform to the variation in the taxes paid

	(1)	(2)	(3)	(4)
	(log) Taxes paid	(log) Taxes paid	(log) Taxes paid	(log) Taxes paid
Labor share in 1998	-1.278***	-1.534***		-1.091***
	(0.014)	(0.007)		(0.004)
Tax rate in 1998	0.630***		3.060***	1.017***
	(0.030)		(0.015)	(0.012)
Tax rate × Labor share in 1998	0.818***	1.943***	-4.353***	
	(0.059)	(0.023)	(0.016)	
Firm size	✓	✓	✓	✓
Obs.	829896	829896	829896	829896
R2	0.63734	0.63715	0.63372	0.63726

Note: This table reports the OLS regression of the different measures of exposure to the reform on the log of taxes paid. The size of firms is controlled. The statistics of interest is the R2 here.

Table A.6: Characteristics of firms in the sample of estimation compared to the tails of the labor share distribution

	(1) Bottom 10%	(2) Main sample	(3) Top 10%
Labor share	0.154	0.463	0.829
	(0.047)	(0.143)	(0.059)
Tax rate	0.231	0.238	0.240
	(0.066)	(0.064)	(0.069)
Nb. Employees	12.421	17.511	24.857
	(13.489)	(33.421)	(120.936)
Median hourly wage	10.906	11.919	14.285
	(5.166)	(7.350)	(10.904)
Annual hours per employee	1592.078	1655.995	1665.107
	(337.790)	(275.022)	(278.877)
Wage bill (in k €)	259.303	419.215	733.522
	(325.923)	(898.140)	(3882.769)
Tangible assets (in k €)	1284.190	534.609	158.794
	(1350.567)	(917.617)	(514.162)
Tangible assets +Investment (in k €)	1384.367	589.403	182.438
	(1460.383)	(1011.446)	(582.019)
Taxes paid (in k €)	51.435	42.946	33.663
	(66.248)	(65.517)	(57.869)
Circulating assets (in k €)	22.271	25.871	27.124
(Inventories + Claims + Cash flow)	(62.055)	(64.597)	(64.752)
Total assets (in k €)	1770.270	1436.204	1370.781
(net of depreciation)	(2932.287)	(2683.027)	(2761.175)
Value added (in k €)	651.967	718.775	759.590
	(884.018)	(1057.980)	(976.087)
Total sales (in k €)	2333.363	2621.737	2272.155
	(3584.814)	(4050.846)	(3682.890)
Gross operating surplus (in k €)	230.512	194.874	350.641
(EBITA)	(878.708)	(1719.810)	(7888.356)
Observations	82992	663924	82980

Note: This table reports report averages and standard deviations in parentheses of different variables across the tails (the bottom and top 10%) of the labor share distribution and the sample of analysis. All monetary values are reported in 2010 euros. The total sales, circulating assets, total net assets, value-added, and gross operating surplus in thousands of euros.

Table A.7: Differences-in-Differences estimates

	(1) (log) Taxes paid	(2) (log) Nb. Employees	(3) (log) Tangible assets
$Post_{99} \times$ LaborShare$_i$	-0.707***	-0.079**	0.301***
	(0.058)	(0.034)	(0.054)
Firm FE	✓	✓	✓
Q5x Year FE	✓	✓	✓
Obs.	663924	663924	663909
R2	0.919	0.944	0.928

Note: This table reports the difference-in-differences estimates. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of taxes paid (1), the log of the number of employees (2), and the log of tangible assets (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.8: Differences-in-Differences estimates - additional outcomes

	(1) (log) Medium Hourly wage	(2) (log) Hours worked per worker	(3) (log) Value added
$Post_{99} \times$ LaborShare$_i$	-0.013	-0.012	0.086**
	(0.030)	(0.014)	(0.030)
Firm FE	✓	✓	✓
Q5 x Year FE	✓	✓	✓
Obs.	663924	663924	662641
R2	0.789	0.558	0.931

Note: This table reports the difference-in-differences estimates. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of median hourly wage (1), the log of the number of hours worked per worker (2), and the log of Value added (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.9: Elasticity to the labor intensity

	(1) Taxes paid	(2) Employment	(3) Capital
ξ_y^{Lsh}	-.3264 ***	-.0367**	.1394***
pvalue	.000	.021	.000
	(4) Median hourly wage	(5) Hours worked	(6) Productivity
ξ_y^{Lsh}	-.0061	-.005	.0400**
pvalue	.422	.399	0.029

Note: This table reports the computed elasticities based on the difference-in-differences estimates. The computation is the following: $\xi_y^{Lsh} = (\exp(\beta/100) - 1) * \overline{LaborShare}$, with β being the coefficient estimated using the non-dynamic differences-in-differences model and $\overline{LaborShare}$ the average labor share in the sample of estimation. The standard errors are displayed in parentheses. The outcomes of interest are the taxes paid (1), employment (2), tangible assets (3), median hourly wage (4), hours worked per employee (5), and productivity (6). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.10: Differences-in-Differences estimates by quintiles of labor intensity

	(1) (log) Med. hourly wage	(2) (log) Annual hours per employee	(log) Value Added per worker
1^{st} Quintile Labor Sh.	0.019	-0.220***	0.305**
	(0.073)	(0.070)	(0.148)
2^{nd} Quintile Labor Sh.	-0.013	-0.054*	0.093
	(0.034)	(0.030)	(0.061)
3^{rd} Quintile Labor Sh.	0.004	0.025	0.240***
	(0.033)	(0.029)	(0.060)
4^{th} Quintile Labor Sh.	-0.029	0.001	0.135***
	(0.028)	(0.023)	(0.048)
5^{th} Quintile Labor Sh.	-0.012	0.059	0.206**
	(0.060)	(0.141)	(0.098)
Firm FE	✓	✓	✓
Q5 x Year FE	✓	✓	✓
Obs.	663924	663924	66282
R2	1	1	0.720

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with quintiles dummies. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of median hourly wage (1), the log of the number of annual hours worked per employee (2), and the log of the value added divided by the number of workers (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.11: Differences-in-Differences estimates by industry

	(1)	(2)	(3)
	(log) Taxes paid	(log) Nb. Employees	(log) Tangible assets
H-Transport & Warehouses	-0.168*	0.007	0.504***
	(0.086)	(0.062)	(0.105)
J-Info.&com.	-0.125	0.203*	0.131
	(0.185)	(0.115)	(0.165)
L-Real Estate	-0.790***	-0.038	0.088
	(0.091)	(0.056)	(0.084)
M-Specialized acivities	-0.766***	-0.054	0.007
	(0.092)	(0.070)	(0.088)
N-Admin services	-0.691***	-0.064	-0.005
	(0.135)	(0.089)	(0.127)
Firm FE	✓	✓	✓
Q5 x Year FE	✓	✓	✓
Indus. x Year FE	✓	✓	✓
Obs.	654780	654780	654765
R2	0.921	0.945	0.929

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with industry dummies. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of taxes paid (1), the log of the number of employees (2), and the log of tangible assets (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.12: Differences-in-Differences estimates by industry

	(1)	(2)	(3)
	(log) Med. houly wage	(log) Annual hours/employee	(log) Value added/employee
C-Manufacturers	-0.035**	-0.003	0.165***
	(0.018)	(0.015)	(0.033)
F-Construction.	-0.058***	-0.020	0.087***
	(0.019)	(0.015)	(0.032)
G-Trade	-0.052***	-0.012	0.113***
	(0.018)	(0.015)	(0.032)
H-Transport & Warehouses	0.017	-0.022	0.163***
	(0.024)	(0.022)	(0.042)
I-Hospitality	-0.060***	-0.006	0.141***
	(0.022)	(0.023)	(0.042)
J-Info.&com.	0.015	-0.068	-0.077
	(0.054)	(0.059)	(0.107)
L-Real Estate	0.051	-0.004	0.357***
	(0.031)	(0.024)	(0.051)
M-Specialized activities	-0.021	-0.016	0.244***
	(0.031)	(0.022)	(0.059)
N-Admin. services	-0.050	-0.011	0.083
	(0.034)	(0.035)	(0.068)
Firm FE	✓	✓	✓
Q5x Year FE	✓	✓	✓
Obs.	654780	654780	653685
R2	0.793	0.559	0.721

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with industry dummies. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of median hourly wage (1), the log of the annual hours worked per employee (2). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.13: Differences-in-Differences estimates by size in 1998

	(1)	(2)	(3)
	(log) Med. houly wage	(log) Annual hours/employee	(log) Value added/employee
3-9 employees	-0.034**	-0.016	0.180***
	(0.017)	(0.015)	(0.030)
10-19 employees	0.013	-0.008	0.109***
	(0.018)	(0.015)	(0.032)
20-49 employees	0.026	-0.007	0.181***
	(0.018)	(0.016)	(0.034)
50+ employees	0.039	-0.041**	0.091*
	(0.025)	(0.020)	(0.049)
Firm FE	✓	✓	✓
Q5x Year FE	✓	✓	✓
Size x Year FE	✓	✓	✓
Obs.	663924	663924	662827
R2	0.790	0.560	0.722

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with firm-size dummies. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of the median hourly wage (1), the log of the annual hours worked per worker (2), and the log of the wage bill (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.14: Labor share by financial constraint

	Financially constrained			Unconstrained			Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Lower L intens.	Higher L intens.	Difference	Lower L intens.	Higher L intens.	Difference	(5)-(6)
Q1	0.235	0.270	-0.036	0.235	0.270	-0.035	-0.001
Q2	0.316	0.372	-0.056	0.316	0.372	-0.057	0.001
Q3	0.427	0.482	-0.056	0.427	0.483	-0.056	0
Q4	0.544	0.613	-0.069	0.544	0.614	-0.069	0
Q5	0.674	0.722	-0.049	0.674	0.721	-0.048	-0.001

Note: This table compares the average labor share across financially constrained and unconstrained firms. Each group is further divided based on whether their labor intensity is above or below the within-bin median. The last column reports the difference between the difference existing within financially contained firms and unconstrained ones. The table presents results across five quintiles (Q1 to Q5), with each quintile showing the mean values for both lower and higher labor intensity firms, as well as the difference between these values.

Table A.15: Differences-in-Differences estimates by financial constraint

	(1)	(2)	(3)	(4)
	(log) Taxes paid	(log) Nb. Employees	(log) Tangible assets	(log) Value Added/ Labor cost
Low liquidity	-0.721***	-0.076**	0.419***	0.170***
	(0.060)	(0.035)	(0.056)	(0.030)
High liquidity	-0.664***	-0.074**	0.205***	0.149***
	(0.059)	(0.035)	(0.055)	(0.031)
Firm FE	✓	✓	✓	✓
Q5x Year FE	✓	✓	✓	✓
LiquidityxYear FE	✓	✓	✓	✓
equality coef.	.033	.913	.000	.000
Obs.	663924	663924	663924	663924
R2	0.920	0.944	0.928	0.745

Note: This table reports the estimated coefficients of a difference-in-differences model fully interacted with the financially constrained dummy. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by one percentage point. The dependent variable is the log of taxes paid (1), the log of the number of employees (2), the log of tangible assets (3), and the log of the value added divided by the labor cost. ***, ** and * denote significance at 1%, 5%, and 10%, respectively.

Table A.16: Characteristics of firms by profitability in 1998

	(1) Not Profitable	(2) Profitable	(3) Differences ((1)-(2))
Industries *(Share of plants)*			
C-Manufacturers	0.173	0.265	-0.091***
F-Construction	0.197	0.187	0.010***
G-Trade	0.443	0.342	0.101***
H-Transport & Warehouses	0.032	0.044	-0.012***
I-Hospitality	0.089	0.070	0.019***
J-Info.& com.	0.005	0.010	-0.004***
L-Real Estate	0.029	0.029	0
M-Specialized act.	0.018	0.040	-0.022***
N-Admin services	0.014	0.014	0
Firm size *(Share of plants)*			
3-9 employees	0.774	0.516	0.258***
10-19 employees	0.146	0.251	-0.105***
20-49 employees	0.052	0.166	-0.114***
50+ employees	0.028	0.066	-0.039***
Quintiles of Labor share *(Share of plants)*			
1^{st} Quintile Labor Sh.	0.156	0.112	0.044***
2^{nd} Quintile Labor Sh.	0.295	0.231	0.064***
3^{rd} Quintile Labor Sh.	0.247	0.251	-0.005
4^{th} Quintile Labor Sh.	0.210	0.267	-0.057***
5^{th} Quintile Labor Sh.	0.092	0.139	-0.046***
Labor Share	0.438	0.474	-0.036***
Sh. of plants below within-bin median Labor Sh.	0.516	0.493	0.023***
Observations	55327		

Note: This table reports average characteristics of firms not liable for the corporate tax in 1998 in column (1) and of firms liable for the corporate tax in 1998 in column (2). Column (3) reports the differences between the two groups and the significativity of the difference using t-tests. ***, ** and * denote significance at 1%, 5% and 10%, respectively. Industries, firms' size and belonging to the quintile k are reported as the share of plants.

Table A.17: Differences-in-Differences estimates by skill groups

	(1) Nb. Low skilled	(2) Nb. High skilled	(3) Skill ratio	(4) Skill wage gap
$\text{Post}_{99} \times$ LaborShare$_i$	-3.420***	1.300**	0.2743**	0.0510*
	(1.123)	(0.619)	(0.1132)	(0.0280)
Firm FE	✓	✓	✓	
Q5x Year FE	✓	✓	✓	
Obs.	663924	663924	663924	660618
R2	0.937	0.897	0.5927	0.9481
Mean Y in 1998	13.25	3.20	0.3558	

Note: This table reports the estimated coefficients of a difference-in-differences model. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by 1. The dependent variable is the number of low-skilled workers (1), the number of low-skilled workers (2), and the ratio of high-skilled workers to low-skilled (3). Low-skilled workers are defined as workers earning less than two times the minimum wage and high-skilled more than two times (see Section 2.3 for more detail). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

B Additional Figures

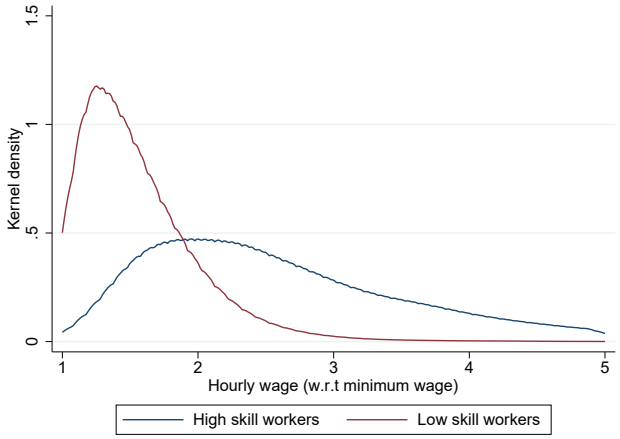
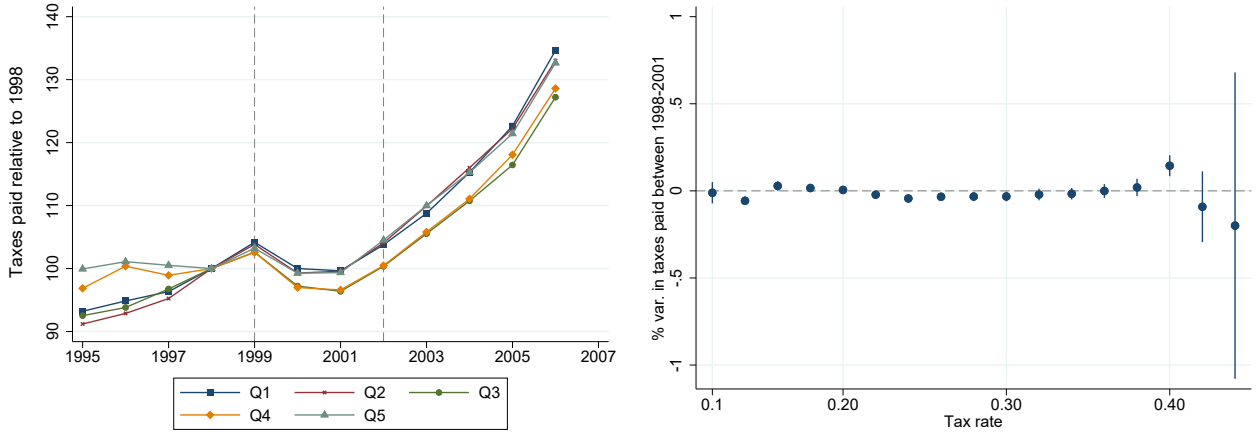


Figure B.1: Distribution of wages by occupations

Note: The figure plots the 2002 distribution of hourly gross wages of workers by occupation. The sample is restricted to workers employed in firms present in my estimating sample

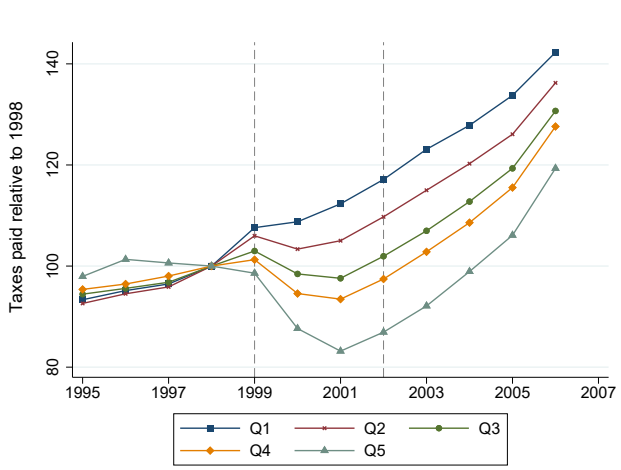


(a) Evolution of taxes paid by quintiles of the tax rate in 98

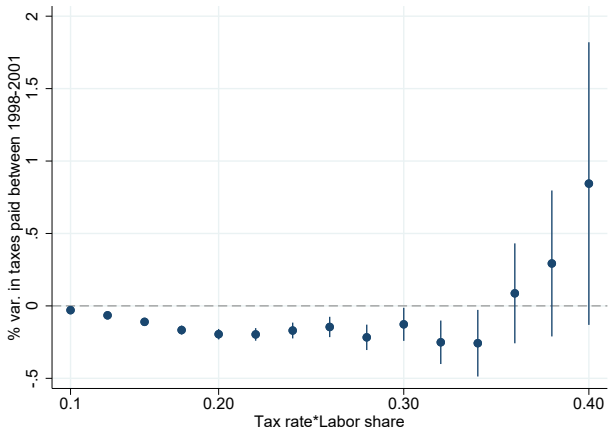
(b) Percentage change in taxes paid between 1999 and 2001 by tax rate

Figure B.2: Correlations between the Tax rate and the Taxes paid

Note: Panel (a) plots the average taxes paid over time for each quintile of the tax rate distribution in 1998. Panel (b) shows the average percentage change in taxes paid between 1998 and 2001 and the related 95% confidence interval across the tax rate distribution in 1998, divided into two percentage-point bins.



(a) Evolution of taxes paid by quintiles of the tax rate×Labor share



(b) Percentage change in taxes paid between 1999 and 2001 by the Tax rate×Labor share

Figure B.3: Correlations between the Tax rate×Labor share and the taxes paid

Note: Panel (a) plots the average taxes paid over time for each quintile of the tax rate×Labor share distribution in 1998. Panel (b) shows the average percentage change in taxes paid between 1998 and 2001 and the related 95% confidence interval across the tax rate×Labor share distribution in 1998, divided into two percentage-point bins.

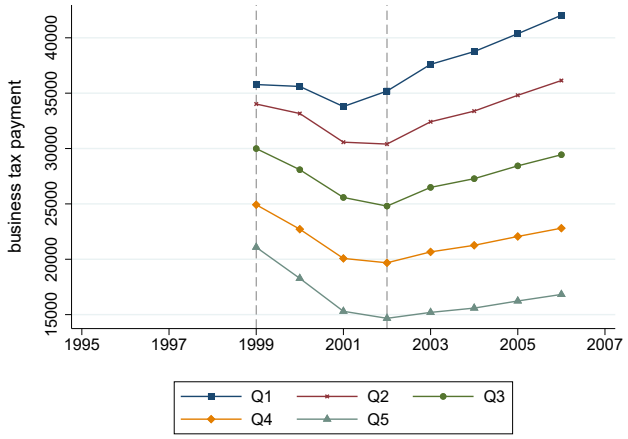


Figure B.4: Percentage change in taxes paid between 1995 and 1998 by 2 pp bins

Note: This figure plots the average tax payment for the business tax over time for each quintile of the tax Labor share distribution in 1998. The average is not normalized; this variable comes from the business tax statement and is therefore available only from 1999 onward.

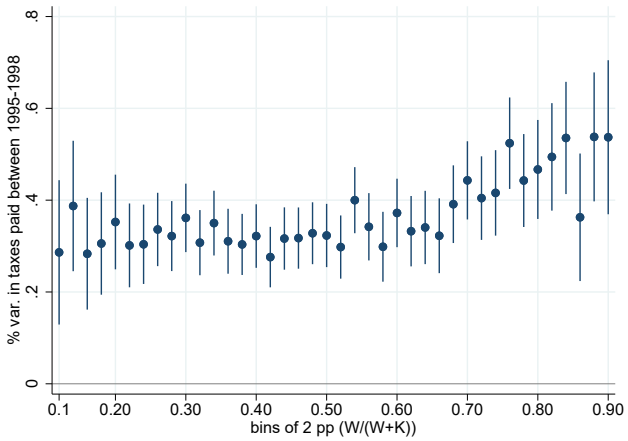


Figure B.5: Evolution of the business tax payments by quintiles of Labor share distribution in 1998

Note: This figure shows the average percentage change in taxes paid between 1999 and 2001 and the related 95% confidence interval across the labor intensity distribution, divided into 2 percentage-point bins.

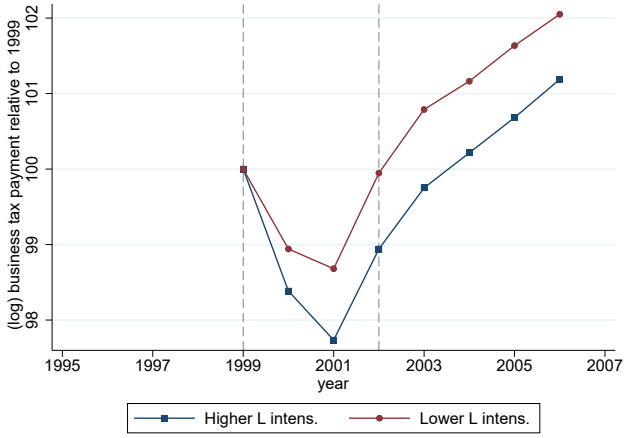


Figure B.6: Averages of the business tax payments over time

Note: This figure shows the average (log) business taxes paid over time relative to 1999. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. This variable comes from the business tax statement and is available only from 1999 onward.

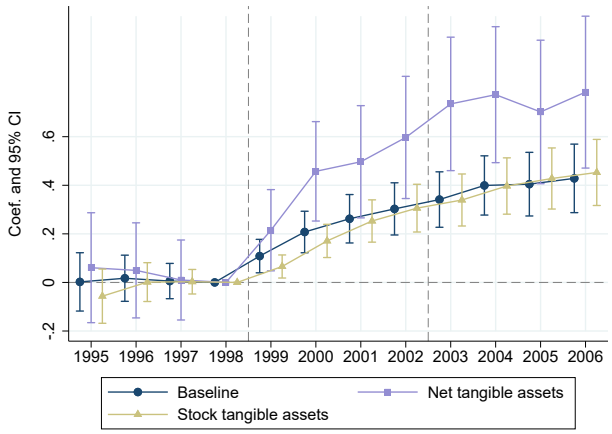
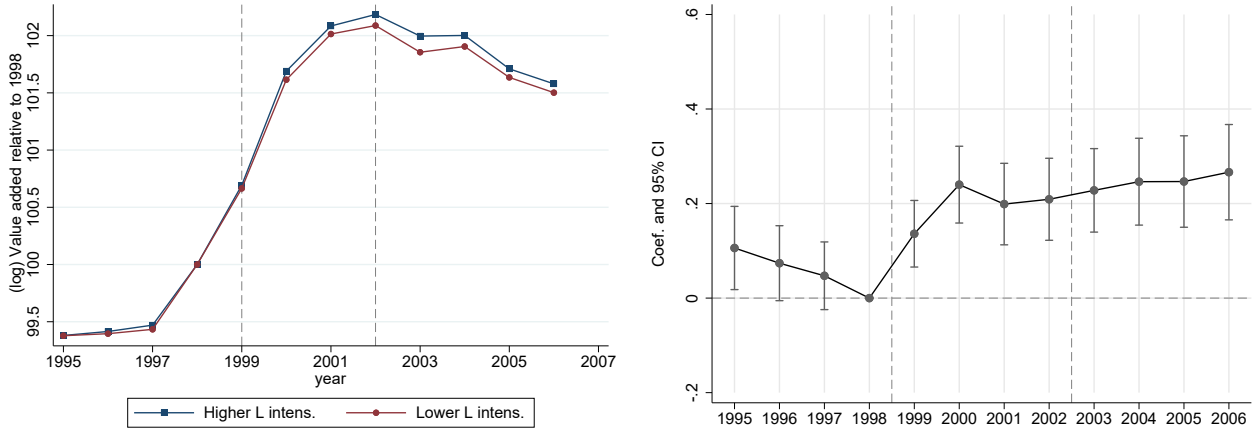


Figure B.7: Effects on Capital - alternative definitions

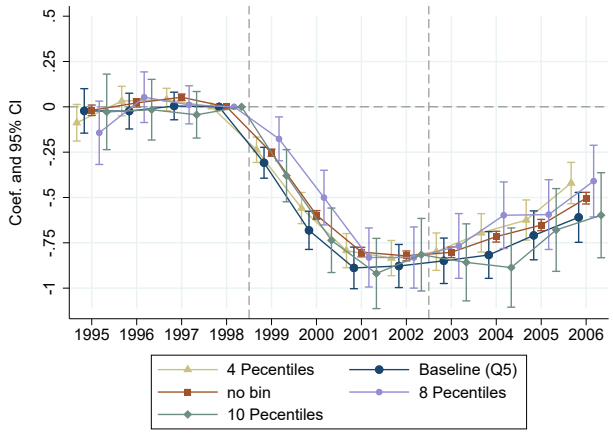
Note: This figure plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variables are the log of stock of tangible assets plus investment (labeled as "Baseline"), the log of stock of tangible assets and investment minus depreciation (labeled as "Net tangible asset"), and the log of stock of tangible asset (labeled as "Stock of tangible asset"). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



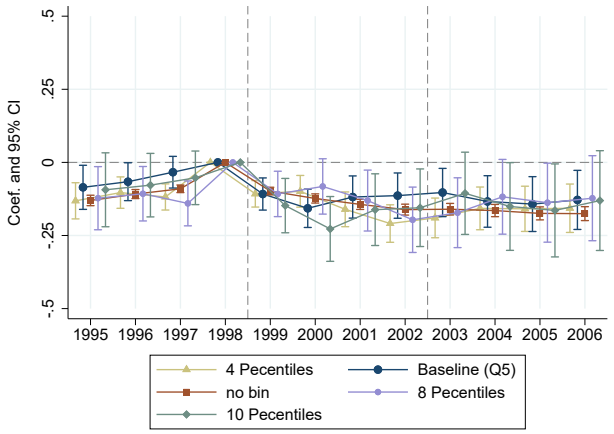
(a) Averages over time **(b) Event study estimates**

Figure B.8: Effects on Productivity per Employee

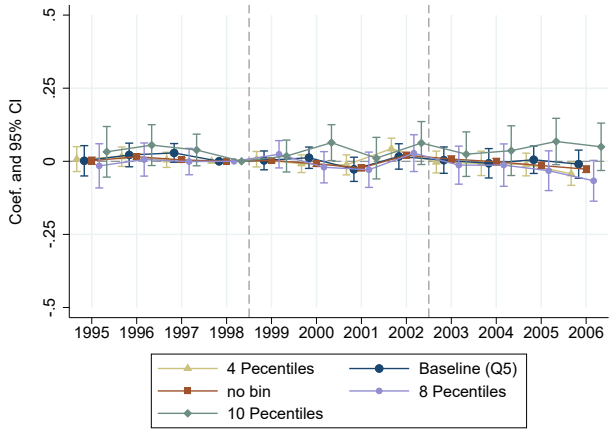
Note: Panel (a) plots the average (log) Value added divided by the number of employees over time relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2). The dependent variable is the log of (log)Value added divided by the number of employees. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



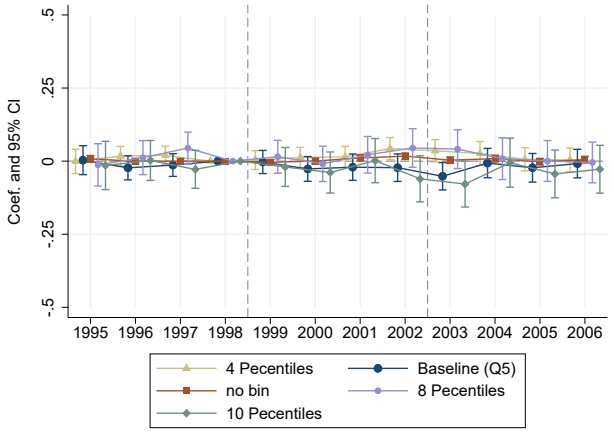
(a) Effect on Taxes paid



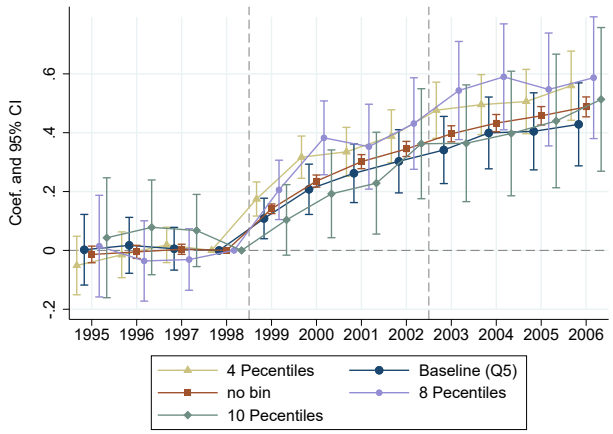
(b) Effect on Employment



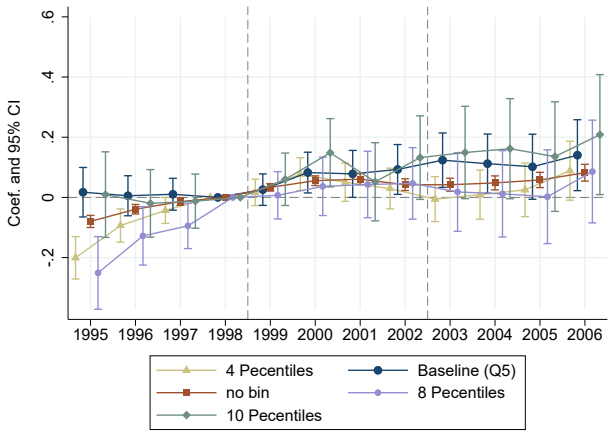
(c) Effect on Median hourly wages



(d) Effect on Annual hours worked per employee



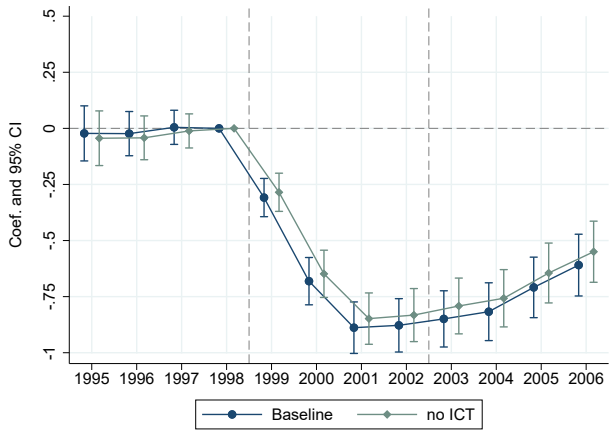
(e) Effects on Capital



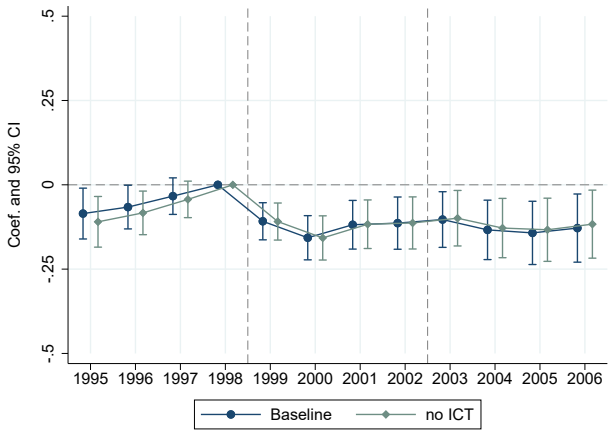
(f) Effects on Productivity

Figure B.9: Robustness: change in bins.

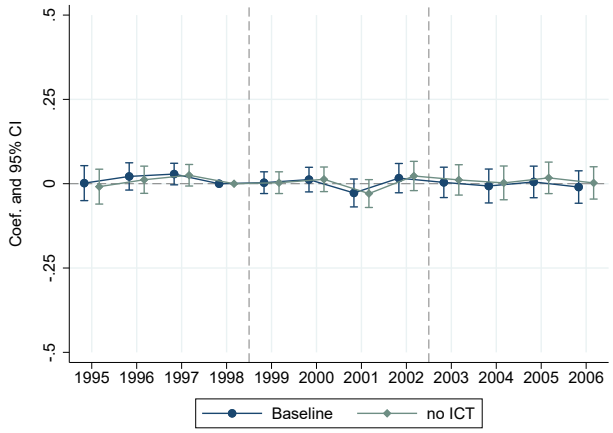
Note: The plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) with different definitions of bins: the four quartiles, the eight percentiles, the ten percentiles of the distribution and no bins. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



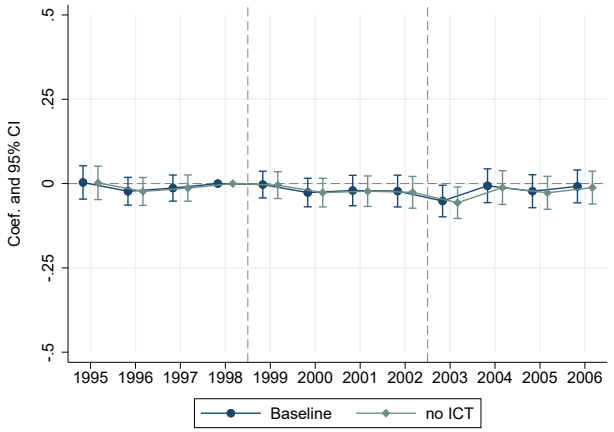
(a) Effect on Taxes paid



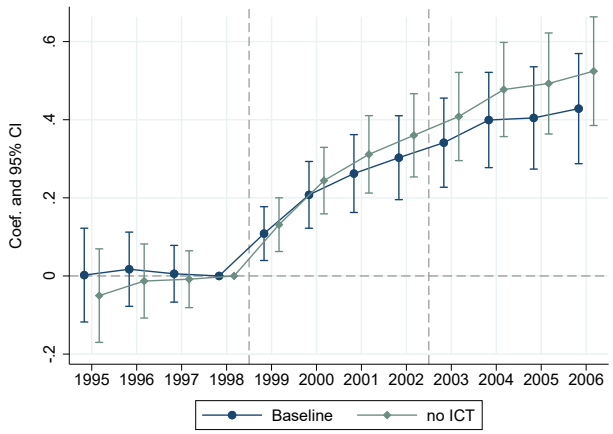
(b) Effect on Employment



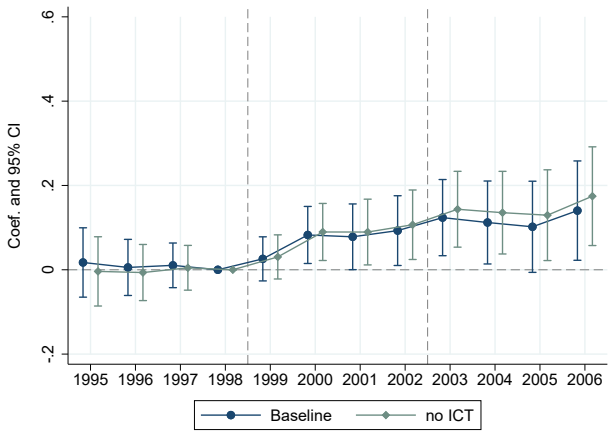
(c) Effect on Median hourly wages



(d) Effect on Annual hours worked per employee



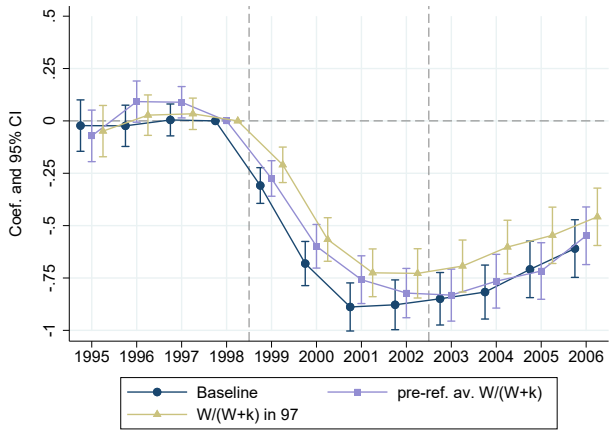
(e) Effects on Capital



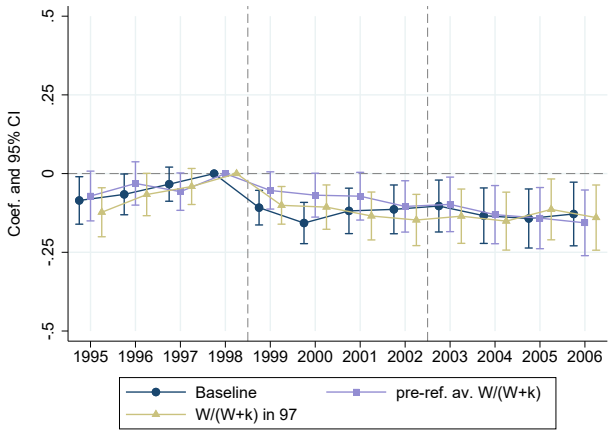
(f) Effects on Productivity

Figure B.10: Robustness: excluding the telecommunication and information sector.

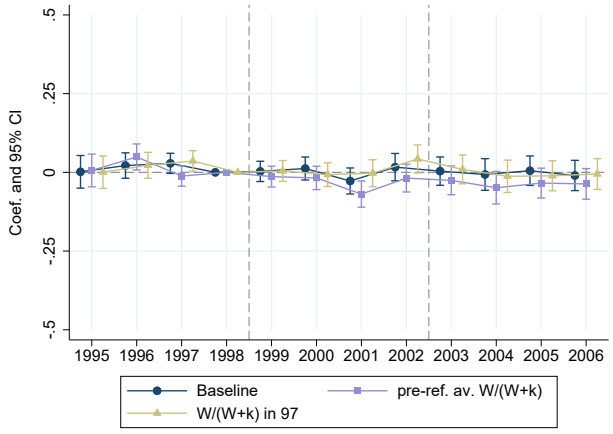
Note: The plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) with different definitions of bins: the four quartiles, the eight percentiles, the ten percentiles of the distribution and no bins. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



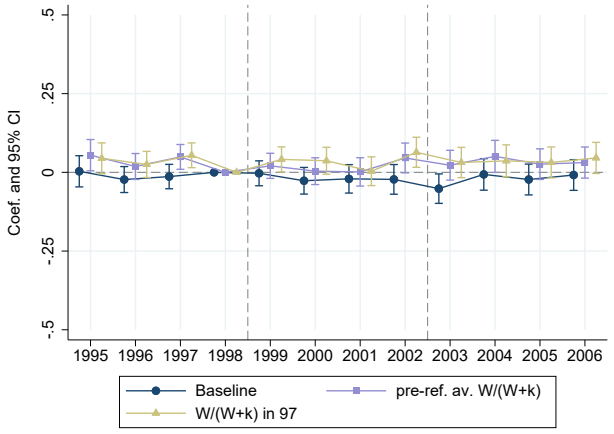
(a) Effect on Taxes paid



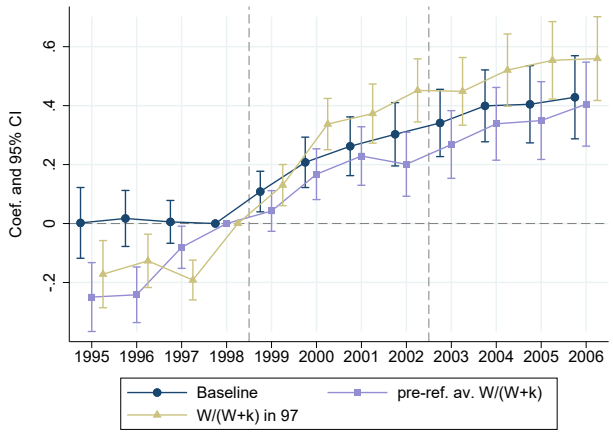
(b) Effect on Employment



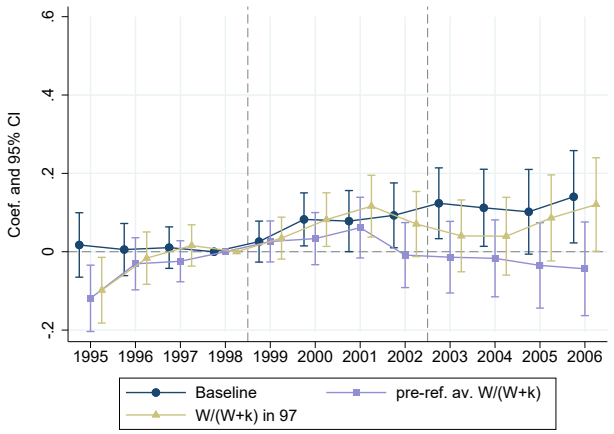
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



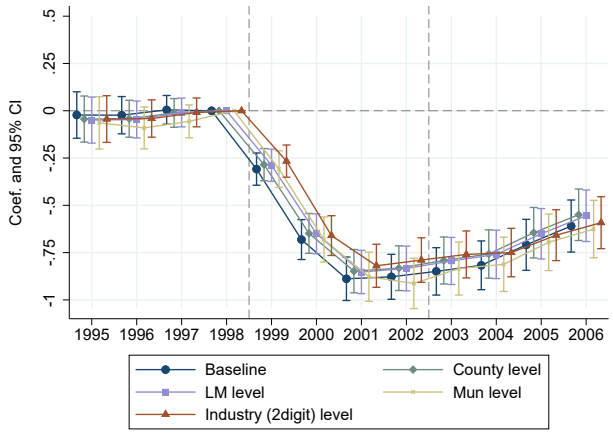
(e) Effects on Capital



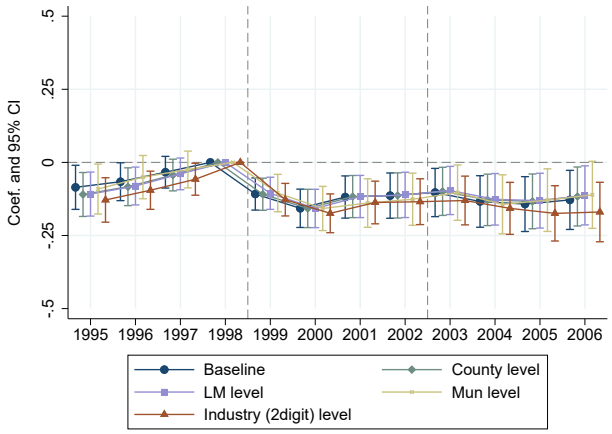
(f) Effect on Productivity

Figure B.11: Robustness: change in the measure of labor share.

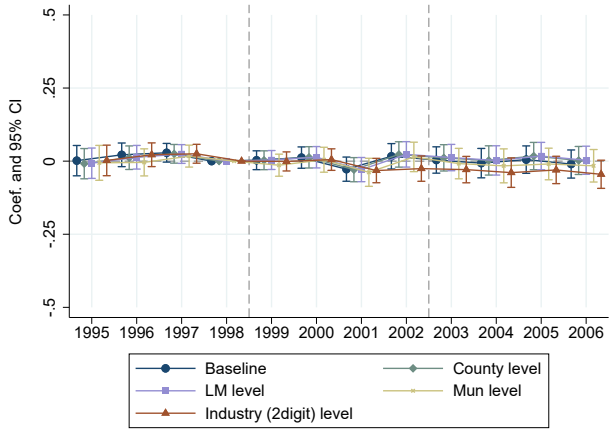
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) with different measures of labor share: the labor share calculated in 1997 – labeled "W/(W+K) in 97", and the average labor share in the pre-reform period (1995-1998) – labeled "pre-ref. W/(W+K)". The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



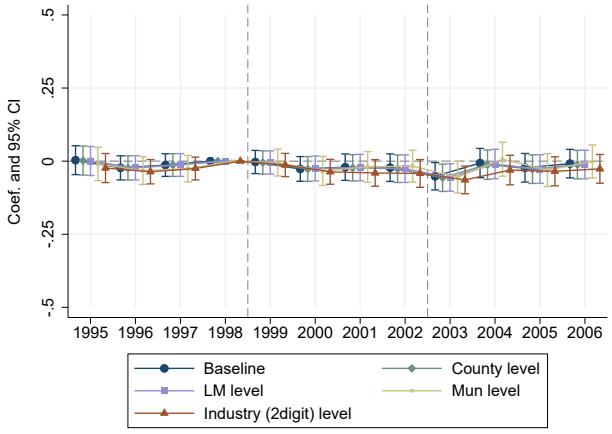
(a) Effect on Taxes paid



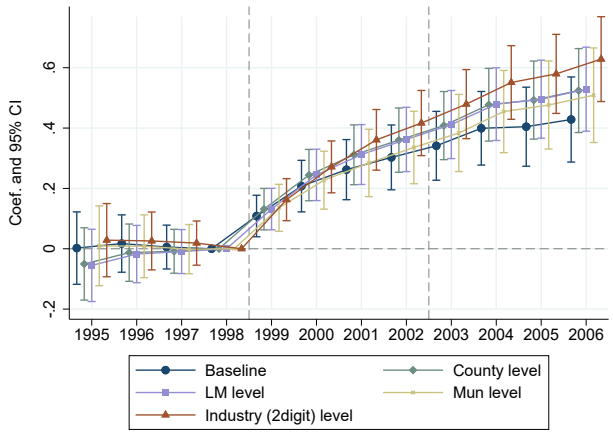
(b) Effect on Employment



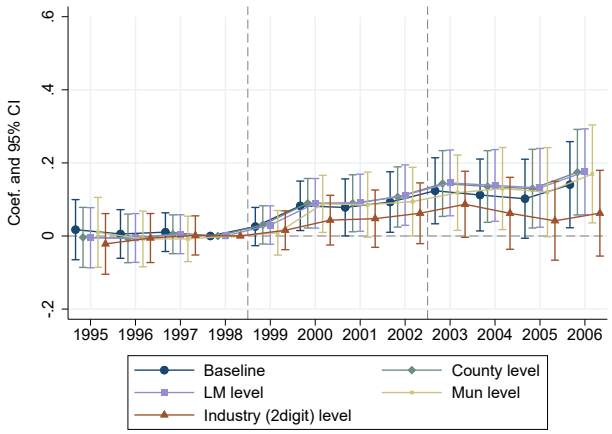
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



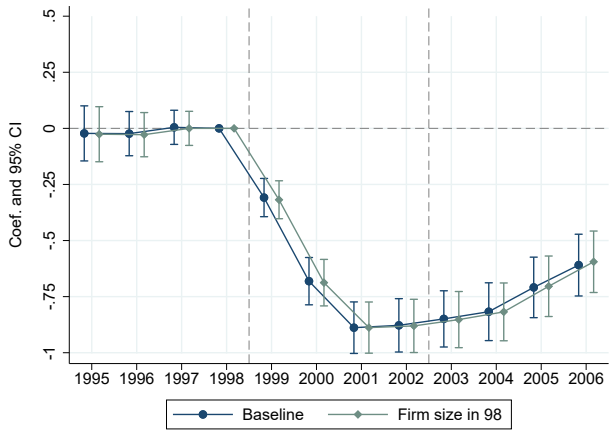
(e) Effects on Capital



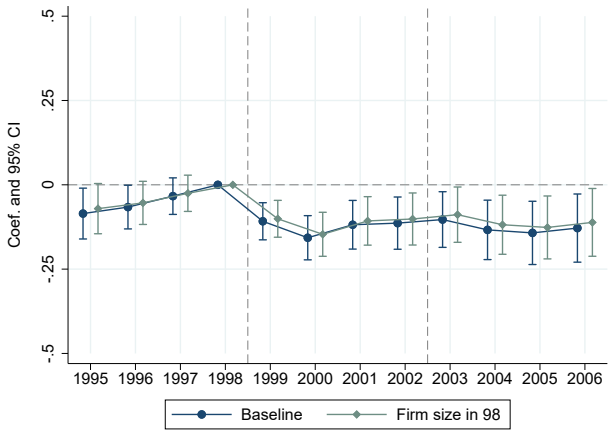
(f) Effect on Productivity

Figure B.12: Robustness: controlling for local shocks.

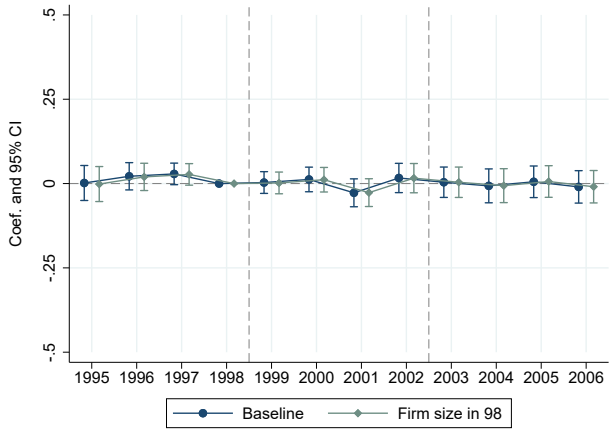
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) and alternative models controlling for the county, labor market (LM), and industry shocks. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



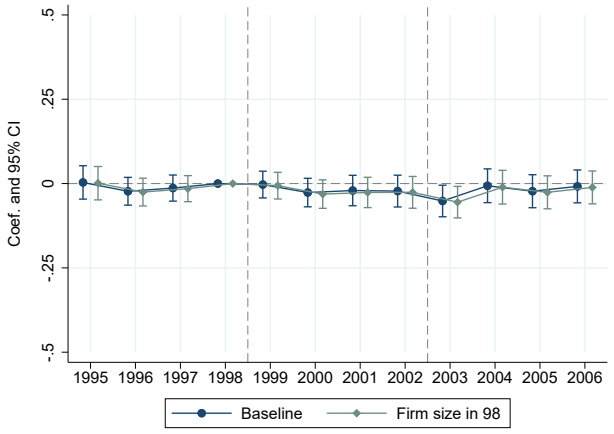
(a) Effect on Taxes paid



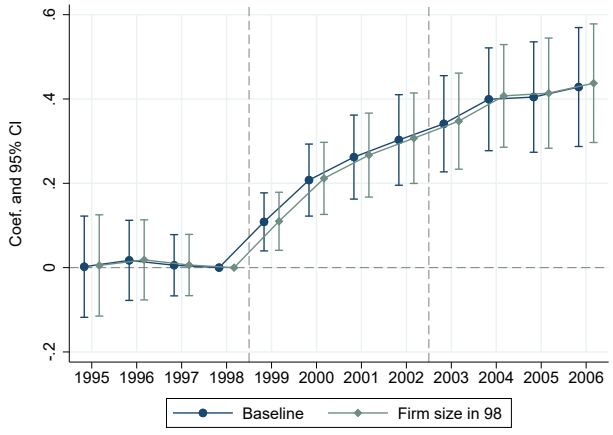
(b) Effect on Employment



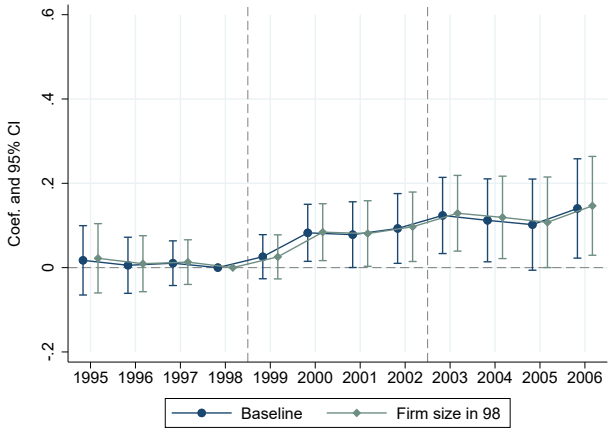
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



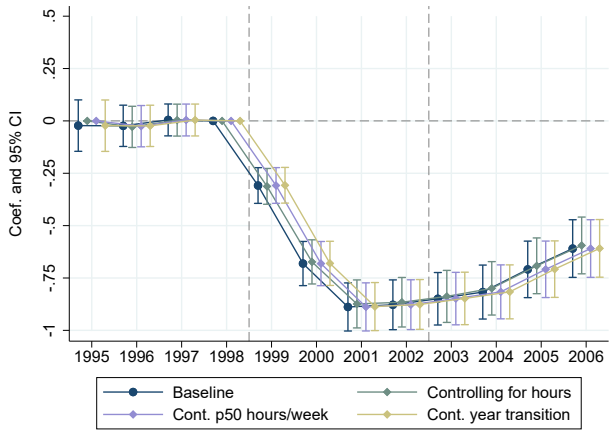
(e) Effects on Capital



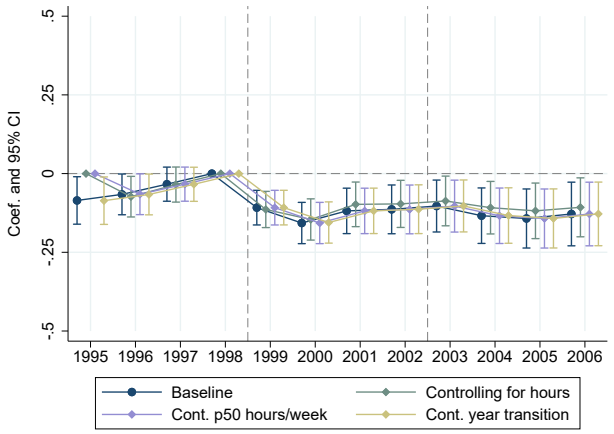
(f) Effects on Productivity

Figure B.13: Robustness: controlling for firms' size.

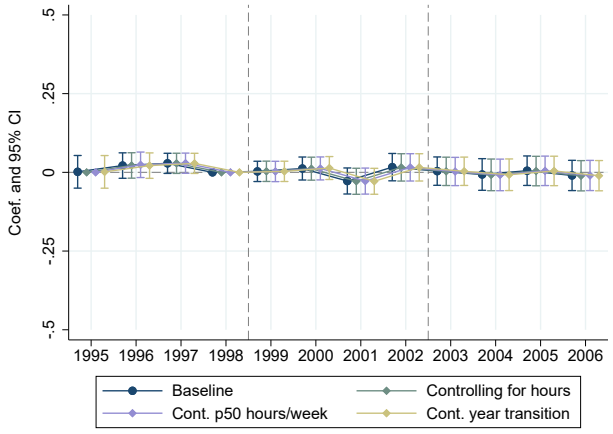
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) and an alternative model controlling for the firm size using seven categories: less than 10, 10-19, 20-49, 50 to 249, 250 to 499, 500 to 999, and more than 999 employees. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



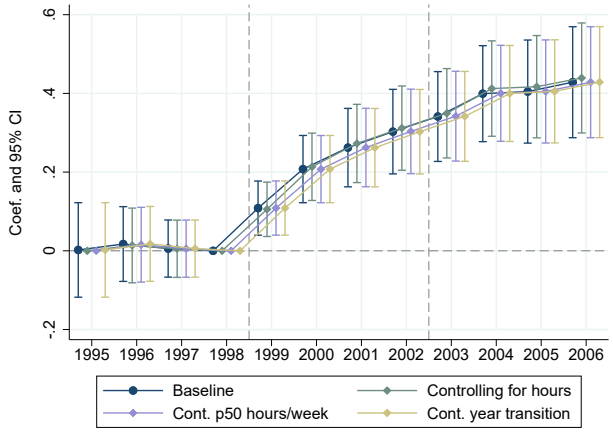
(a) Effect on Taxes paid



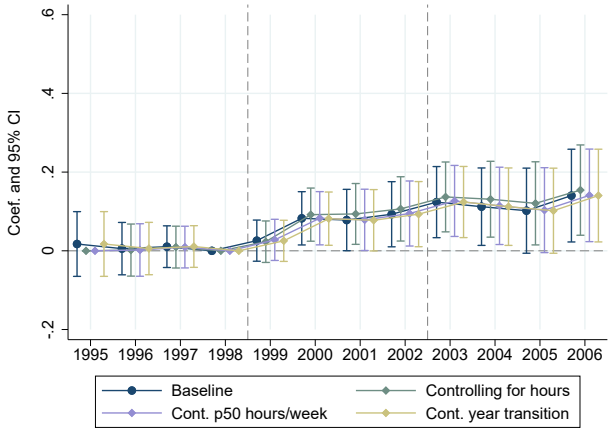
(b) Effect on Employment



(c) Effect on Median hourly wage



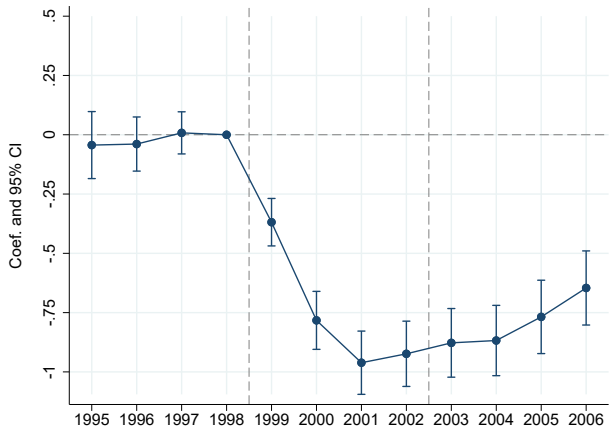
(d) Effects on Capital



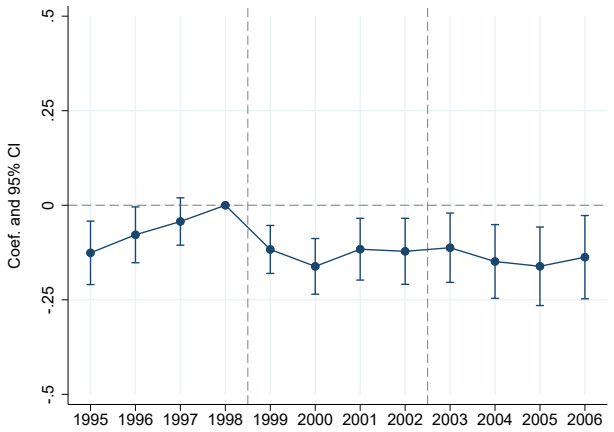
(e) Effect on Productivity

Figure B.14: Robustness: controlling for the reduction in working time.

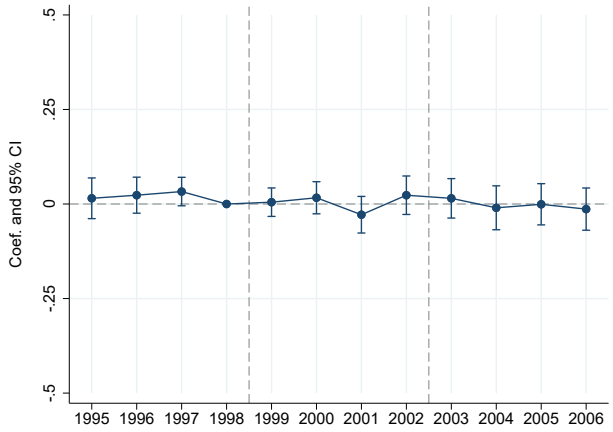
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) and alternative models controlling for hours worked per worker in the previous year, the median hours worked per week per worker in the previous year, and the approximate year of transition to the 35h worked per week. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the annual hours worked per employee in Panel (c), the log of tangible assets in Panel (d), and the log of value-added/number of employees in Panel (e). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



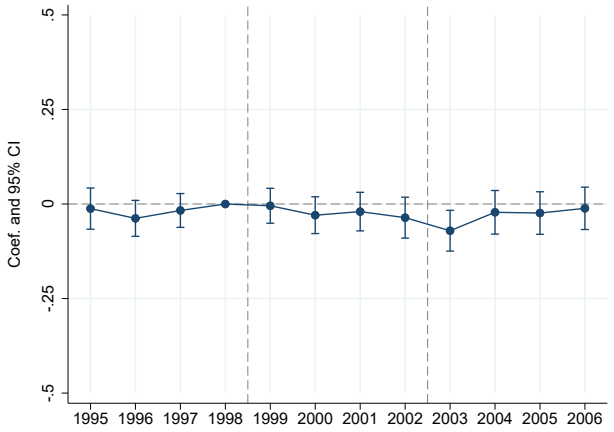
(a) Effect on Taxes paid



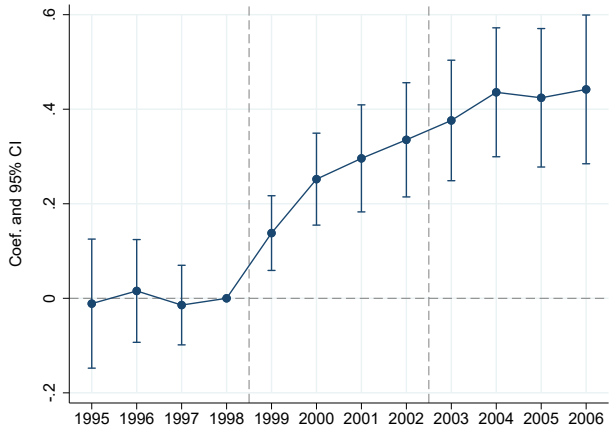
(b) Effect on Employment



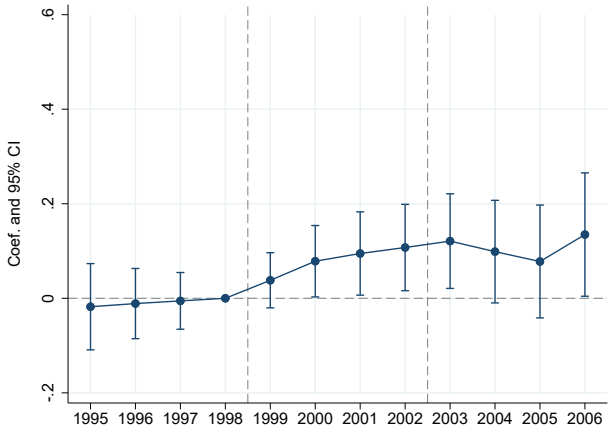
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



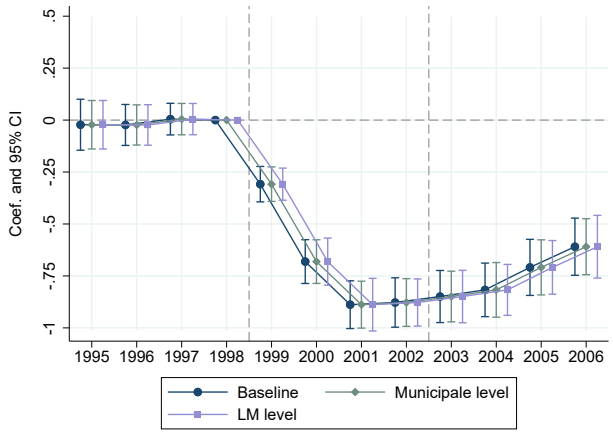
(e) Effects on Capital



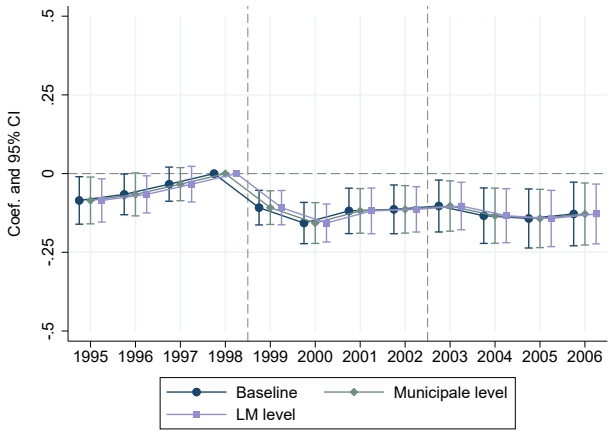
(f) Effect on Productivity

Figure B.15: Robustness: firms with less than 20 employees

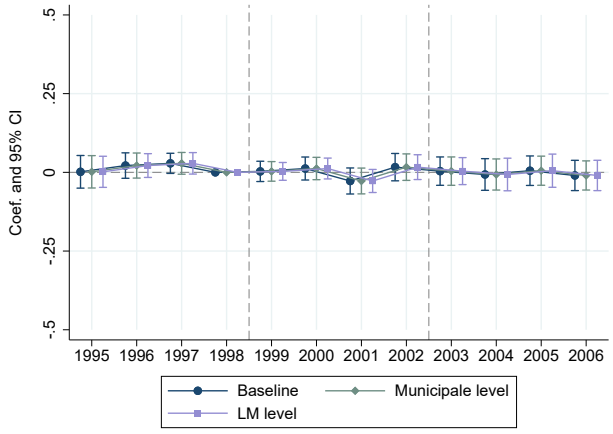
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) restricted to the sample of firms with less than 20 employees in 1998. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



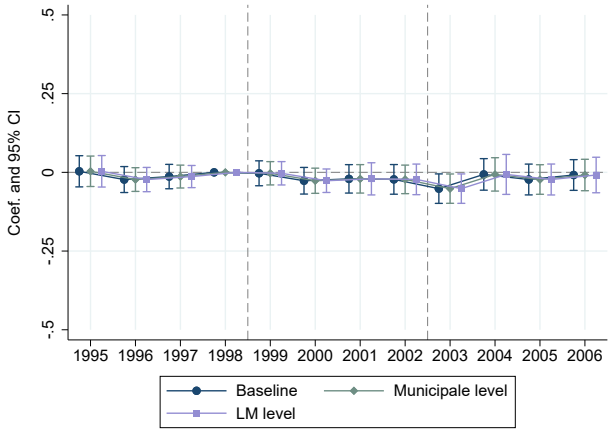
(a) Effect on Taxes paid



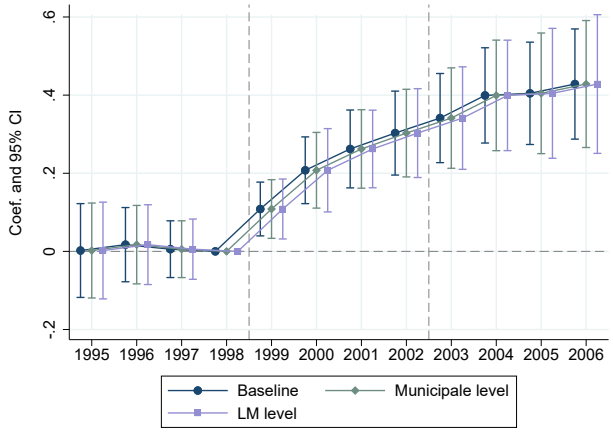
(b) Effect on Employment



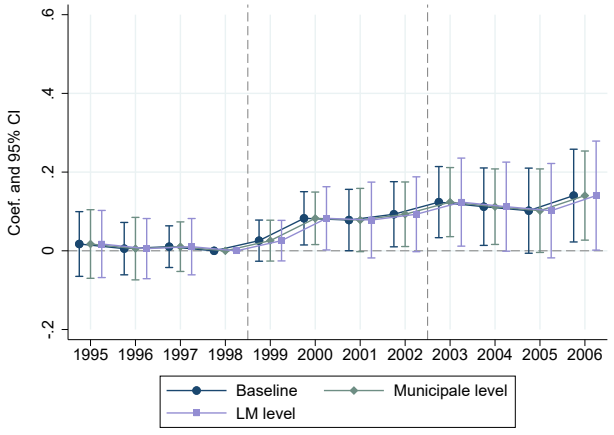
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



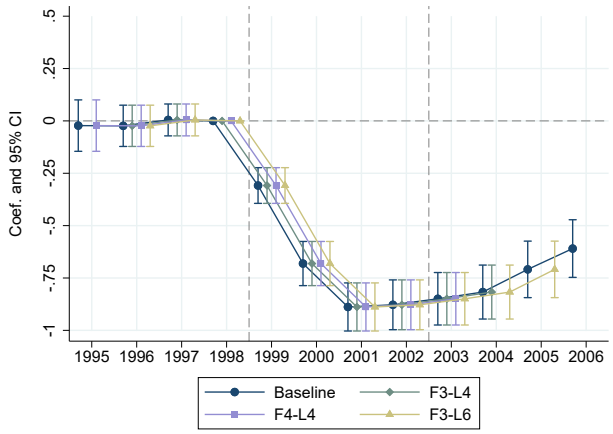
(e) Effects on Capital



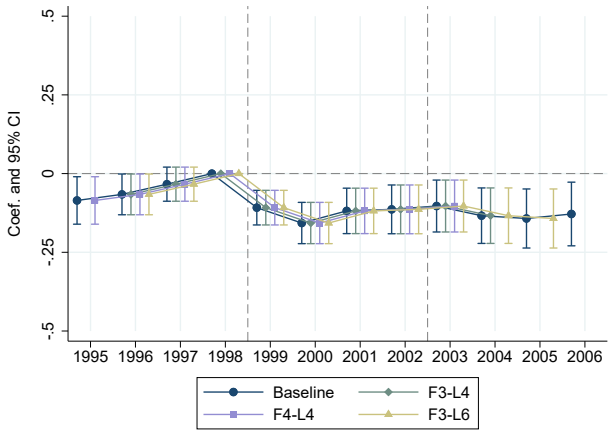
(f) Effect on Productivity

Figure B.16: Robustness: change in the clustering level.

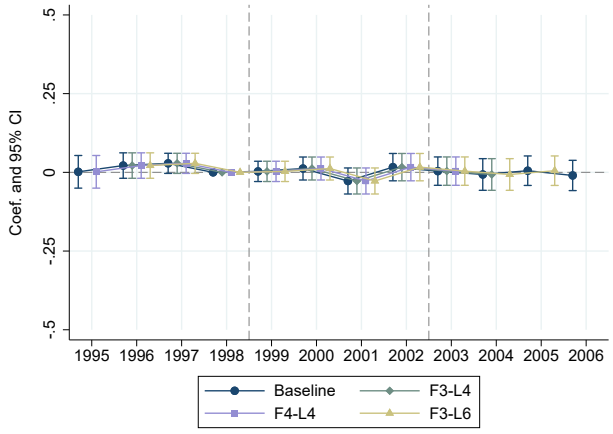
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) and alternative models with standard errors clustered at different levels: the municipality level (n≃36,000) or the local labor market (n≃400). The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



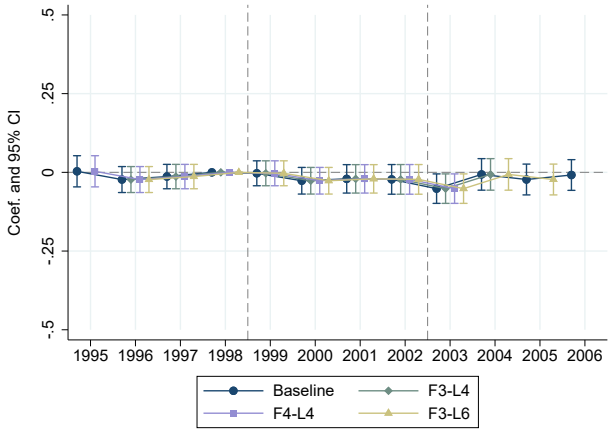
(a) Effect on Taxes paid



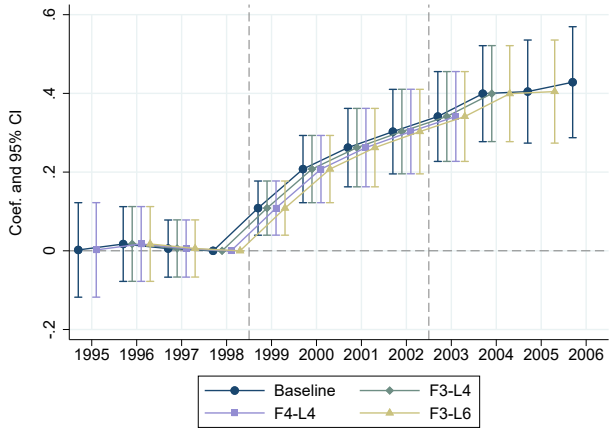
(b) Effect on Employment



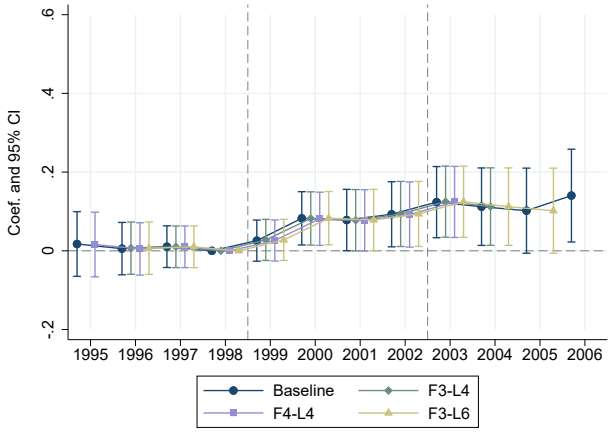
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



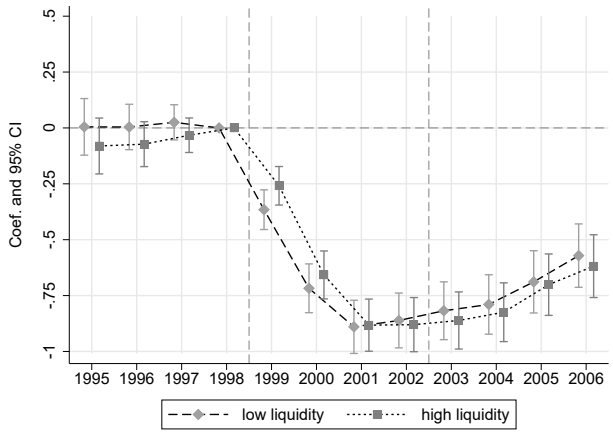
(e) Effects on Capital



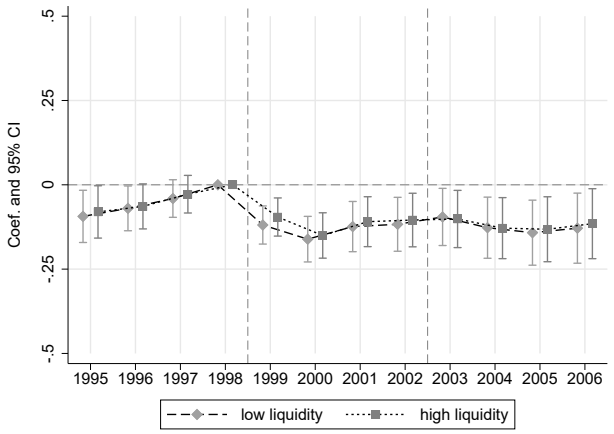
(f) Effects on Productivity

Figure B.17: Robustness: change in the window of estimation.

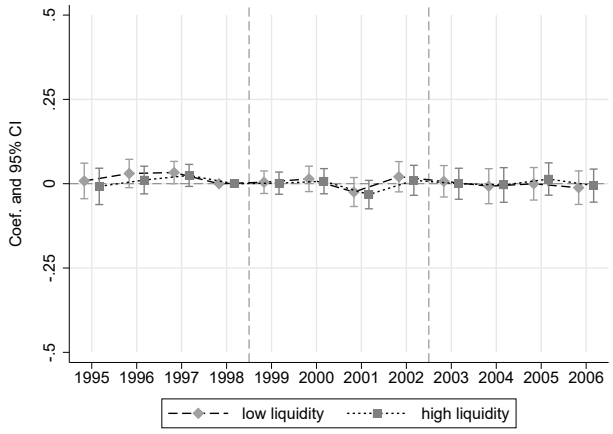
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) and alternative models with different numbers of leads and lags: three leads and four lags (F3-L4); four leads and four lags (F4-L4); three leads and six lags (F3-L6). The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



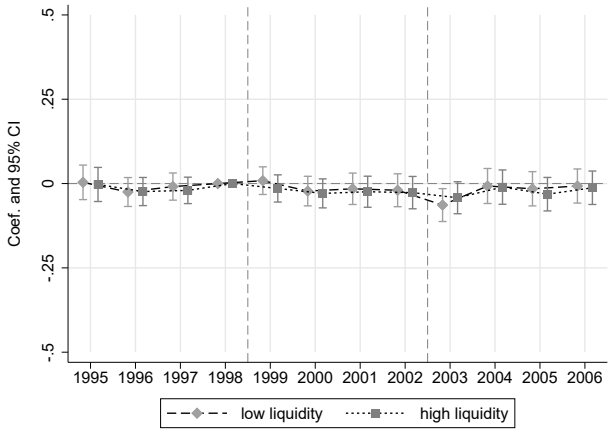
(a) Effect on Taxes paid



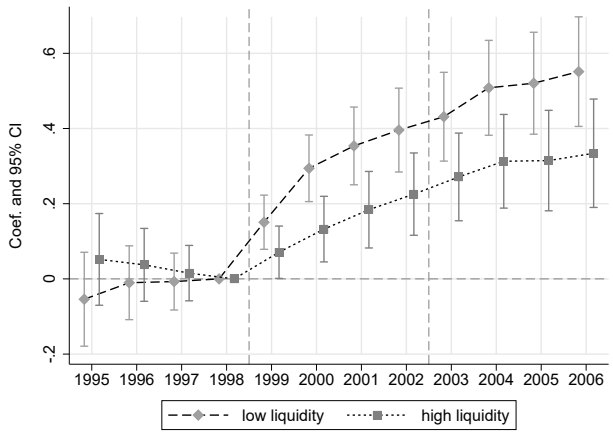
(b) Effect on Employment



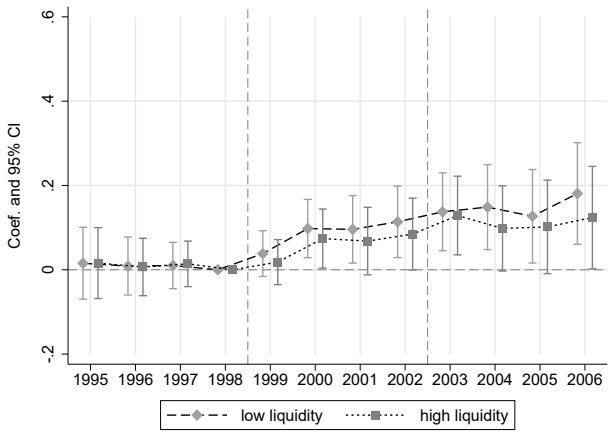
(c) Effect on Median hourly wages



(d) Effect on Annual hours worked per employee



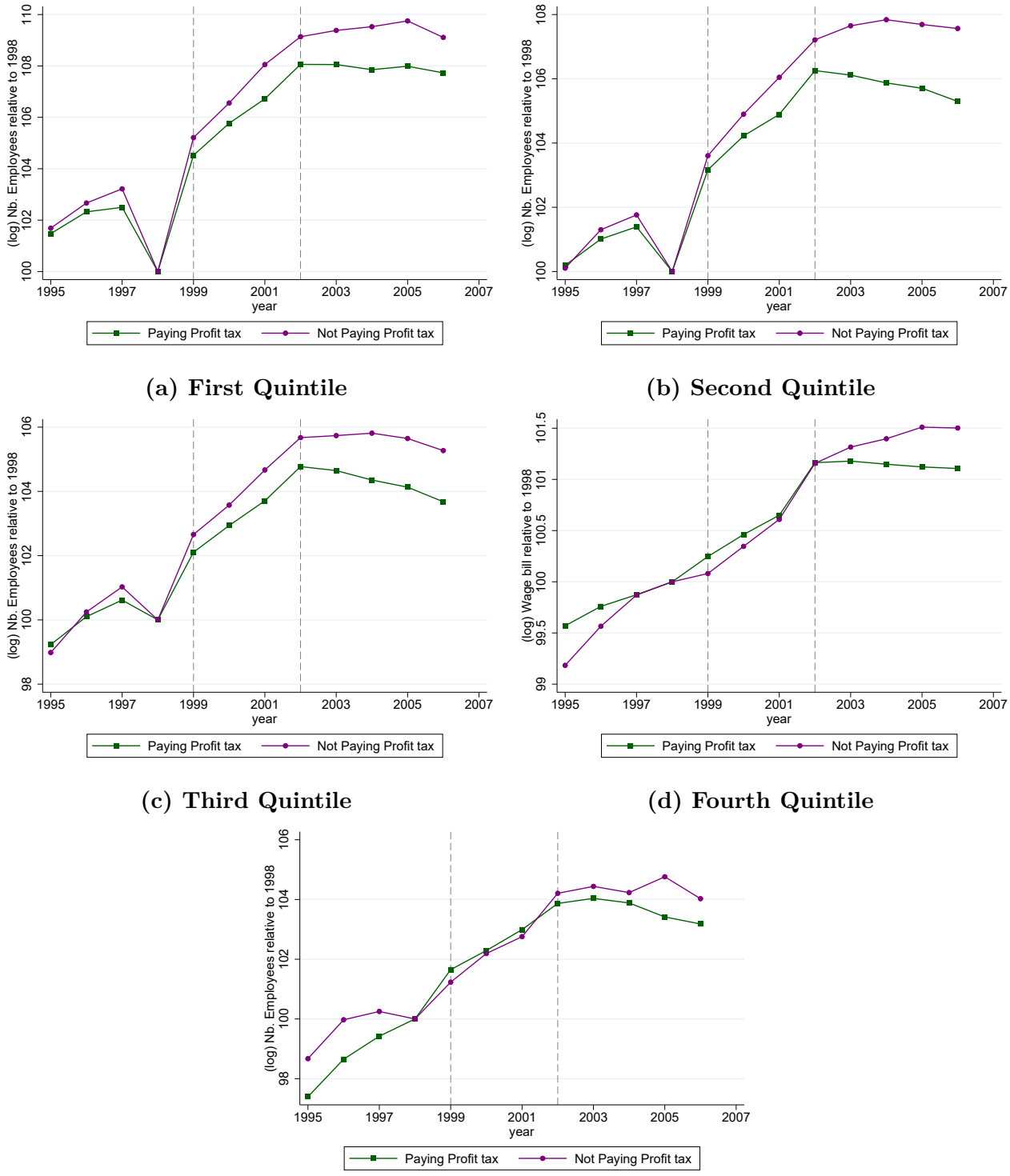
(e) Effects on Capital



(f) Effects on Productivity

Figure B.18: Effects by liquidity constraint

Note: The plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) fully interacted with a dummy indicating whether the liquidity of firms is below or above the sample's median. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



(a) First Quintile

(b) Second Quintile

(c) Third Quintile

(d) Fourth Quintile

(e) Fifth Quintile

Figure B.19: Average number of employees over time across quintiles by profit tax liabilities

Note: the figure plots the average (log) number of employees over time relative to 1998. I consider two groups of firms according to whether firms pay the corporate tax in 1998. The averages for the first quintile of the labor share in 1998 can be found in panel (a), the second quintile in panel (b), the third quintile in panel (c), the fourth quintile in panel (d), and the fifth in panel (e). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

C Definition of the Business tax rate

Three sub-national administrative divisions determine the final rate faced by plants. These jurisdictions divide the national territory into different tiers of local government. Each division has a fiscal power, setting the tax rates levied in their jurisdiction. As a result, economic agents' overall tax is the sum of the rates determined by each tier.

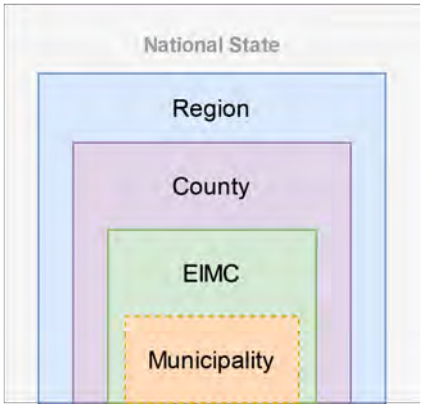


Figure C.1: Administrative divisions

Note: This figure displays the multiple tiers of the French sub-national jurisdiction. The region is the coarsest geographical division, while the municipality is the finest. As municipalities can cooperate with others and create an EIMC, the line separating municipalities from EIMC is a dotted line.

As can be seen in Figure C.1, the largest division is the region (*région*): it gathers about 4-5 counties (*départements*) out of 95 – in mainland France. The smallest tier is the municipality. There are about 36,000 municipalities (*communes*) counting on average 1,400 inhabitants and half of these municipalities count less than 400 inhabitants (see Table C.1). Due to a large number of French municipalities, neighboring municipalities are allowed to cooperate and create or join an Establishment for Inter-Municipal Cooperation (henceforth EIMC – *Etablissement Public de Coopération Inter-Communale, EPCI*) since the '80s. Note that they can only belong to one at the same time. The cooperation implies a transfer to some degree of their fiscal power to the EIMC, harmonizing, at least partly, the local business tax across municipalities belonging to the same EIMC.[26]

The business tax rate firms face is the sum of the tax rates fixed by the municipality/the EIMC, the county, and the region, depending on the plant's location. Table C.2 shows the average business tax rates for the different tiers of local government. It is worth underlining that the bulk of the tax rate is decided at the municipal/EIMC level.

[26]They also share the cost of local public goods such as public transport or waste management. See Tricaud (2019) for more details. There are two types of fiscal cooperation: the single business tax regime and the additive tax regime. Under the former rule, they fully transfer their fiscal power to the EIMC, implying that the business tax rate will be the same for all members. Under the additive tax regime, municipalities keep defining their tax rate, and the EIMC determines a rate that adds up to those of the municipalities. See Breuillé et al. (2018) for more details.

Table C.1: Average municipality in the sample

	Mean	Sd	Min	Max	p25	p50	p75	N
Business tax	10.22	5.09	0	31.1	6.59	9.63	13.02	29,936
Property tax	12.88	5.77	0	59.83	8.53	12.2	16.07	30,817
EIMC	0.69	0.32	0	1	.46	.8	1	30,817
Population	1,398.47	13,684.24	0	2,015,539	196.33	401.27	927.6	30,817
Employees	9.25	35.12	2	5003	3.61	5.77	10.04	30,817
Plants	25.17	471.37	1	79,329	1.43	3.27	10.6	30,817

Table C.2: Local tax Rates by administrative division

	Mean	Sd	Min	Max	p25	p50	p75	N
Municipality/EIMC	11.60	5.45	0	76.8	8	11.1	14.7	36,559
+ *County*	7.81	2.29	0	16.2	6.4	7.3	8.90	96
+ *Region*	2.45	0.67	0	4.3	2	2.3	2.90	22
= **Business tax**	21.86	5.45	0	91.4	17.2	21.1	25.9	36,559

D Construction of the panel of firms

Linked employer-employee data contains key elements that enable me to construct a panel of firms with all the information required. I use the *SIREN* firm identifier and the year to merge the labor force data to the Balance sheet data.[27] In the linked employer-employee data, I also have the municipality where the plant is located. I first harmonize this identifier to the geography of 2010.[28] I then merge the panel of firms with the panel of municipalities; this enables me to apply local controls, particularly the local tax rate, to each establishment. Finally, I merge the tax statements using the plant identifier.

[27]The *SIREN* is the first nine digits of the plant identifier (*SIRET*).

[28]Mergers and splits of municipalities induce a change of identifier and borders. For now, I drop firms and municipalities with such events throughout the study. This represents roughly 180 municipalities among 36,000.

E Conceptual framework

In this section, I use a simple microeconomic model of labor demand to show under which circumstances a tax on labor affects firms' employment and investment decisions. Let there be a representative firm that produces with a production function Cobb-Douglas $q = F(K, L) = K^{\alpha}L^{\beta}$ with factors capital K and labor L and $0 < \alpha, \beta < 1$ and $\alpha + \beta \leq 1$ such that firms make profits. Firms also face a tax on the wage bill τ_p and a corporate profit tax τ_c where a share $\rho \in [0; 1]$ of the capital costs and a share $\theta \in [0; 1]$ of the tax on labor can be deducted from the corporate tax base.[29]. Firms' after-tax profit (Π) can be decomposed in the following way:

$$\Pi = \underbrace{K^{\alpha}L^{\beta} - wL - rK}_{\text{Gross profit}} \overbrace{-\tau_p wL}^{\text{Tax on the wage bill}} \underbrace{-\tau_c \Big[K^{\alpha}L^{\beta} - wL - \rho K - \overbrace{\theta\tau_p wL}^{\text{deductible part of the tax on the wage bill}} \Big]}_{\text{Coporate tax liabilities}} \tag{3}$$

terms provides the following equation:

$$\Pi = K^{\alpha}L^{\beta}(1 - \tau_c) - [1 + \tau_p - \tau_c(1 + \theta\tau_p)]wL - (1 - \rho\tau_c)K \tag{4}$$

where w is the wage and r the interest rate. For simplicity, I normalized the price of output to 1. I assume that firms are price takers in the product market and that the non-tax costs of capital is not affected by changes in the corporate tax rate. The average French municipality is small. Thus, I assume that labor is perfectly mobile across jurisdictional borders. As a consequence, a change in the local tax on wages leaves wages in the competitive sector unchanged. Firms choose capital K and employment L so that the after-tax profit is maximized. The maximization yields the following first-order conditions, determining factor demands:

$$\frac{\partial \Pi}{\partial L} = \beta K^{\alpha}L^{\beta-1}(1 - \tau_c) - [1 + \tau_p - \tau_c(1 + \theta\tau_p)]w = 0 \tag{5}$$

$$\frac{\partial \Pi}{\partial K} = \alpha K^{\alpha-1}L^{\beta}(1 - \tau_c) - (1 - \rho\tau_c)r = 0 \tag{6}$$

The ratio of the two conditions gives:

$$\frac{\beta}{\alpha}\frac{K}{L} = \frac{[1 + \tau_p - \tau_c(1 + \theta\tau_p)]w}{(1 - \rho\tau_c)r} \tag{7}$$

The closed-form expression for the labor demand L is then:

$$L = \left(\frac{\alpha}{(1 - \rho\tau_c)r}\right)^{\frac{\alpha}{1-\alpha-\beta}} \cdot \left(\frac{\beta}{w} \cdot \frac{1}{1 + \tau_p - \tau_c(1 + \theta\tau_p)}\right)^{\frac{1-\alpha}{1-\alpha-\beta}} \tag{8}$$

The closed-form expression for the capital demand K is then:

$$K = \left(\frac{\alpha}{(1 - \rho\tau_c)r}\right)^{\frac{1-\beta}{1-\alpha-\beta}} \cdot \left(\frac{\beta}{w} \cdot \frac{1}{1 + \tau_p - \tau_c(1 + \theta\tau_p)}\right)^{\frac{\beta}{1-\alpha-\beta}} \tag{9}$$

[29] An alternative interpretation of ρ is that it measures the share of capital financed by debt. Payments on debts, i.e., interests, can usually be deducted from the tax base, while equity payments are normally paid from after-tax profits. Therefore, we usually have $\rho \in (0; 1)$.

To simplify notations, let's note $A = (\frac{\alpha}{(1-\rho\tau_c)r})^{\frac{\alpha}{1-\alpha-\beta}} \cdot (\frac{\beta}{w})^{\frac{1-\alpha}{1-\alpha-\beta}}$. The derivative of the labor demand with respect to the professional tax is the following:

$$\frac{\partial L}{\partial \tau_p} = A \cdot \frac{1-\alpha}{1-\alpha-\beta} \cdot (-(1-z\tau_c)) \cdot \left(\frac{1}{1+\tau_p-\tau_c(1+\theta\tau_p)}\right)^{\frac{1-\alpha}{1-\alpha-\beta}+1} \tag{10}$$

This expression is negative; when the professional tax increases, the labor demand decreases. Doing a similar derivation for the capital demand also suggests that the capital is negatively affected by the tax on labor. The rationale is straightforward: higher taxes on wages reduce labor demand, and due to some complementarity between labor and capital, capital is also negatively affected. However, the responses of labor demand to an increase in labor taxes are weaker when firms can deduct a larger amount from the profit corporate tax base:

$$\frac{\partial L}{\partial \tau_p \partial z} = A \cdot \frac{1-\alpha}{1-\alpha-\beta} \cdot \tau_c \cdot \left(\frac{1}{1+\tau_p-\tau_c(1+\theta\tau_p)}\right)^{\frac{1-\alpha}{1-\alpha-\beta}+1} \tag{11}$$

$$\times \left[1-(1-z\tau_c)\cdot(\tau_p)\cdot\left(\frac{1-\alpha}{1-\alpha-\beta}+1\right)\cdot\frac{1}{1+\tau_p-\tau_c(1+\theta\tau_p)}\right] \tag{12}$$

This expression is positive if and only if the second line of the equation (12) is positive, which is equivalent to:

$$\theta > \frac{1}{\tau_c} + \frac{1}{\tau_p}\frac{1-\alpha-\beta}{1-\alpha}(1-\frac{1}{\tau_c}) \tag{13}$$

This condition is met with reasonable assumptions on the tax rates, the production function of firms, and the deduction rate. This exercise tells us that when facing a tax on labor, firms will reduce labor and capital. However, the more firms can deduct the tax on labor from the corporate tax, the less sensitive firms will be to the tax on labor. This is because the deduction in the corporate tax partially offsets the cost of the labor tax. Transferring the conclusions of this model to my empirical setting is straightforward and generates the following predictions:

Prediction 1: removing the wage bill from the business tax base should increase employment and capital.

Prediction 2: firms who can deduct the business tax from their corporate tax should react less to the reform as the change in their production cost will be smaller.

Basically, I should see different responses from firms depending on whether or not they pay corporate profit taxes.