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

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Abstract

I assess the real effects of a firm-level tax on production inputs, focusing on the 1999 reform of the French local business tax. This reform excluded the wage bill from the tax base, leaving only capital inputs taxed. Using administrative data, I evaluate the impact of this reform on firms' outcomes. I exploit cross-sectional variation in firms' exposure to the reform based on the labor share in their pre-reform tax base. Applying a dynamic differences-in-differences approach, I show that the reform significantly reduces firms' tax liabilities and boosts firms' productivity. This increase in productivity can be attributed to firms using a large portion of the tax savings for investment, particularly among cash-constrained firms. For a 1% reduction in taxes, tangible assets increase by 0.69%. Wages are not affected. I estimate a negative effect on employment: for a 1% reduction in taxes, employment decreased by 0.13%. Importantly, this reduction does not indicate direct job cuts but reflects differences in employment growth rates between capital-intensive and labor-intensive firms, with capital-intensive firms increasing employment more rapidly. This difference in employment growth can be explained by varying degrees of tax sensitivity depending 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 we tax firms? The structure of business taxes has far-reaching implications for key economic outcomes like investment, employment, and wage growth. In particular, the composition of the tax base – whether it targets profits, labor, capital, or a combination – can influence firms' decisions and overall economic performance. Understanding the effects of taxation on firm behavior is crucial for public economics and policy design. While much of the literature focuses on traditional corporate taxes based on profits, there is less empirical evidence on how taxes directly based on input factors affect firm-level outcomes, particularly when shifting the tax burden between labor and capital.

The theoretical literature has long demonstrated that taxing inputs is distortionary, as it increases their cost (Diamond and Mirrlees, 1971a). Yet, providing systematic empirical tests of this conclusion is challenging. Direct taxes on inputs are scarcely used in the economy and are usually marginal compared to other taxes. Furthermore, obtaining a causal effect of input-based taxes requires finding exogenous and meaningful variation.[3] Studying such a tax is not only academically intriguing but also policy-relevant, as it helps better understand firms' responses to changes in the relative costs of inputs.

In this paper, I use France as my laboratory and take advantage of a reform that changed the tax base of the French local business tax (called *Taxe Professionelle*). Unlike traditional business taxes, the Taxe Professionelle was initially based on the value of capital and labor. This tax accounts for around 30% of firms' overall tax burden and about 1.5% of firms' turnover. In 1999, as the tax was inflating the cost of labor – impeding employment and wage growth – a reform gradually removed the wage bill from the tax base. This reduced the cost of labor mechanically while keeping the cost of capital unchanged. This unique reform allows me to investigate firm responses to input-based taxation.

I exploit administrative-linked employer-employee microdata from social security records. This data encompasses the universe of French private sector establishments and provides detailed information about their workforce (e.g., their wages, occupation, and hours worked). By combining these

[3] Property taxes exist in many countries but usually target buildings and land, not necessarily capital directly used in production, such as machinery and equipment. Payroll taxes are based on wages but usually fund benefit schemes.

records with financial and tax statements, I can observe information related to the workforce, firms' capital stock, and the taxes they pay—an invaluable and unique feature for this analysis. My identification strategy exploits the fact that the reform generated firm-specific cost reductions that were proportionate to firms' labor share. I, therefore, take advantage of persistent between-firm variation in labor share just before the reform. After verifying that this is a reliable measure of exposure, I apply a dynamic differences-in-differences approach, comparing more exposed firms (with higher labor share) to the reform with those that are relatively less exposed (with 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. This allows me to have some variation in exposure without comparing firms with substantially different 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 show that this assumption is plausible using the pre-reform periods.

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 0.5%. Second, I find a strong and positive effect on capital. For a 1% reduction in taxes, the stock of tangible assets increases by 0.69%. 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. Third, the reform does not affect wages or hours worked.

Fourth, I estimate a negative effect on employment – employment decreases by 0.13%. While this might seem surprising at first, it actually reflects differences in employment growth between relatively more capital-intensive and relatively more labor-intensive firms. Capital-intensive firms increase employment more quickly. This disparity is due to differences in the possibility of deducting the business tax payment from the corporate tax base. Firms that do not pay corporate taxes – because they do not make profits – bear the total cost of the business tax on labor, as they cannot deduct it from their corporate tax base. Consequently, their labor demand is more constrained before the reform compared to firms that can offset part of the business tax burden. After the reform, unprofitable firms experience greater relief, inducing 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. Heterogeneity analyses show that the employment effects are larger for the construction and trade sectors. Employment adjustments are especially concentrated among capital-intensive firms – in the lower quintiles of the labor share distribution, as well as smaller firms and very large ones. These subgroups are also overrepresented among the nonprofitable companies and, therefore, further support the idea that the ability to deduct the business tax from the corporate tax base affects firms' employment responses.

Lastly, my estimates allow me to rule out any economically meaningful responses regarding wages and hours worked per worker. The absence of wage adjustment is at odds with the canonical model of tax incidence but in line with recent evidence from the payroll tax literature (see Benzarti, 2024, for a review). Overall, these adjustments increase firms' productivity.

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 demonstrate that my estimates are unlikely to be driven by potential confounding reforms.

These findings offer valuable insights into how input-based taxation shapes firms behavior, particularly in resource allocation between labor and capital, and underscore the importance of considering firm characteristics like profitability and tax sensitivity when assessing the broader economic effects of tax policies. The results show that the tax reform significantly impacts firms by reducing their tax burden, especially for those with higher labor shares, and increasing investment. This substantial rise in capital improves firms' productivity. Although the reform does not affect wages or hours worked, it leads to shifts in employment patterns, with capital-intensive firms experiencing faster employment growth than labor-intensive firms. Liquidity-constrained firms primarily drive invest-

ment effects, while differences in employment growth stem from variations in labor cost sensitivity, influenced by the interaction between corporate and local business taxes.

Related literature. The contributions of this paper to the literature are threefold. First, by studying the effects of this specific business tax (directly based on factors of production), I can empirically test the theoretical conclusions of the taxation models. 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 input factors 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 (input-based). In addition, I demonstrate the importance to take into account 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, wages fall (Suárez Serrato and Zidar, 2016; Fuest et al., 2018).[4] 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., 2021; 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 (Duan and Moon, 2024; Lerche, 2022).

[4]Recent evidence suggests the effect is symmetric, following a tax cut wages increase, at least for manufacturers in Québec (Duan and Moon, 2023).

The only exception is probably Harju et al. (2022), but it is limited to small firms.[5] Studies evaluating the effect of tax incentives targeting labor are even scarcer. In this paper, I use a substantial tax reduction (up to 20% decrease in local taxes paid) and provide evidence of its effect on labor and capital outcomes of firms across all industries. Corporate tax reductions targeting specific groups of workers benefit firms' employees, though not necessarily the intended ones (Carbonnier et al., 2022).

Third, as removing the wage bill from the tax base can be seen as an employer payroll tax cut without benefit linkage, this paper contributes to some extent to the payroll tax literature.[6] I provide evidence not restricted to a specific group of workers, preventing wage distortion and lowering the marginal cost of all workers.[7] My results support recent studies showing that not only employment but also investment can be affected by such tax changes (Saez et al., 2019; Benzarti and Harju, 2021b,a). However, the recent findings of Lobel (2024) suggest that an increase in investment might not be systematic following a payroll tax cut and depend on firms' market power. 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. In line with the existing literature, I gather evidence that the liquidity effects are non-negligible.

Outline. The 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

I first describe the institutional context and the reform of interest and then detail the data sources and the construction of the panel of estimation.

[5] Other studies focusing on one outcome at a time have underlined that investment is highly sensitive to taxes (Maffini et al., 2019; Ohrn, 2018; Zwick and Mahon, 2017). Despite targeting specific capital and industries, these tax incentives can have spillover effects on the local labor market (Curtis et al., 2022; Lerche, 2022).

[6] It is based on the wage bill and paid by the employer directly but does not directly fund social security or retirement schemes.

[7] To my knowledge, Benzarti and Harju (2021a) do so. Still, the tax reduction affects only particular municipalities during a recession.

2.1 The French business tax from 1975 to 2010

Since 1975, a local business tax (i.e., the *Professional tax – PT*) has been levied on establishments having a for-profit activity in France.

Definition of the tax rate. Three sub-national administrative divisions – region, county, and municipality – determine the final rate faced by plants.[8] 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.

The tax base. 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[9] 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, scientific activities are likely to be more affected than services, 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.

[8] 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.

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

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

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

Minimum and maximum tax payments. Contributions are bounded. Minimum and maximum contributions are determined with respect to the ratio of the firm's tax liability over the value-added firms produce. Firms are subject to a minimum contribution of 0.35%. This implies that if the computed tax liability accounts for less than 0.35% of the value added they produce, their tax payment equals 0.35% of their value-added. The maximum contribution is set at 3.8%. The reform in 1999 also affected these caps; see the next section.

Exceptions. Independent white-collar professionals, such as physicians or lawyers, with less than 5 employees, are taxed on their income only. They are, therefore, not affected by the reform.

Deductibility of the tax. 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

Change in the tax base. 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.[10] 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$\times$firm level.[11] The yearly tax allowance spanning 1999 to 2002 is summarized in Table 1. The intention behind this phased allowance was to swiftly alleviate the burden on smaller businesses. 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. Conversely, 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

[10] 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.

[11] 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.

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.

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.

Change in the boundaries of the tax payment. In 1999, the minimum tax payment was raised from 0.35% to 1% of the value-added produced by firms. The maximum tax payment is raised to 3.5%; the ceilings also vary with the firm's sales.[12] These variations are not the paper's main focus as the ceiling affects roughly 4.9% of the firms and the minimum contributions 0.6% of the firms (Fouquet, 2004).

Additional reform and shocks. 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. I first find no effect on hours worked, and in Section 4.3, I report additional tests supporting that my results are not affected by the reform. To my knowledge, there

[12]When the sales are lower than 21 million euros, the celling is 3.5%. When the sales are between 21 and 76 million euros, the rate is 3.8 %, and 4.0% when the sales are larger than 76 million.

is no consensus on how the reduction in working time affected employment (Chemin and Wasmer, 2009).

Additional shocks happened in the early 2000s, first the "dot-com" bubble explosion and the development of the broadband internet. I address this issue by showing that my estimates are similar after controlling for local shock and removing telecommunication industry firms from the sample (see Section 4.3). 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.

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.[13] Thanks to a unique plant identifier (*SIRET*), I can aggregate the panel at the plant$\times$year level. I define the main workforce as the number of workers present in the firm at the end of the year.[14] The corresponding gross wage bill is computed by summing the annual gross wage of these individuals in the main labor force. 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 position. Specifically, I obtain the number of high-skilled and low-skilled workers at the firm level.[15]

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

[14] 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).

[15] The occupation definition is the socio-professional classification (PCS) of INSEE at a 1-digit level. This classification has four main groups when it comes to active workers: executives and higher intellectual professions (class 3), intermediary professions (class 4), employees/clerks (class 5), and blue-collar workers (class 6). I pool classes 3 and 4 under the denomination high-skilled workers and gather groups 5 and 6 into low-skilled workers, as done by Carbonnier et al. (2022) or Caliendo et al. (2015).

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 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%.[16] 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 availability of the business tax statement only started in 1999 at the time of the reform, I do not base the measure of exposure to the reform on these data.

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.

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.[17] 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.[18] 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.

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

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

[18] 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.

Sample of estimations. 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.[19] This has the advantage that I estimate 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. Robustness checks are carried out on different estimation samples.

3 Empirical Strategy

In this part, I describe my empirical strategy. I first define the proxy for the exposure to the policy while providing evidence that this is a good one. Then, I specify the estimation model.

3.1 Proxying exposure to the reform

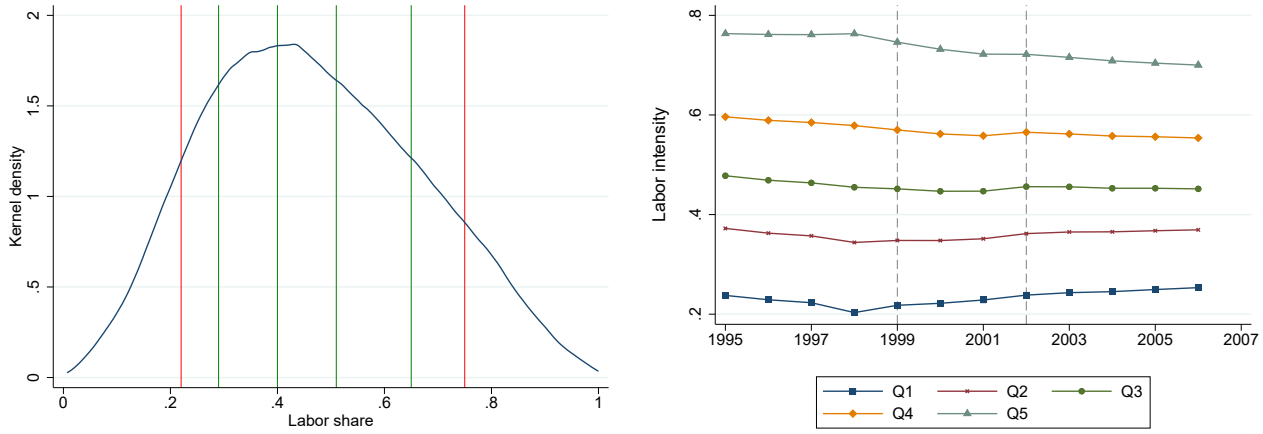
As emphasized in section 2.2, the exposure of firms to the reform depends on three elements: (1) the location of firms determines the business tax rate they face; (2) the production function of firms, i.e., labor-intensive firms are more exposed; (3) the size of firms, i.e., the smaller the firms, the 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.

[19] 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.

Therefore, I proxy the exposure to the reform by the labor intensity in 1998.[20] 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 Figure2a), 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 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**

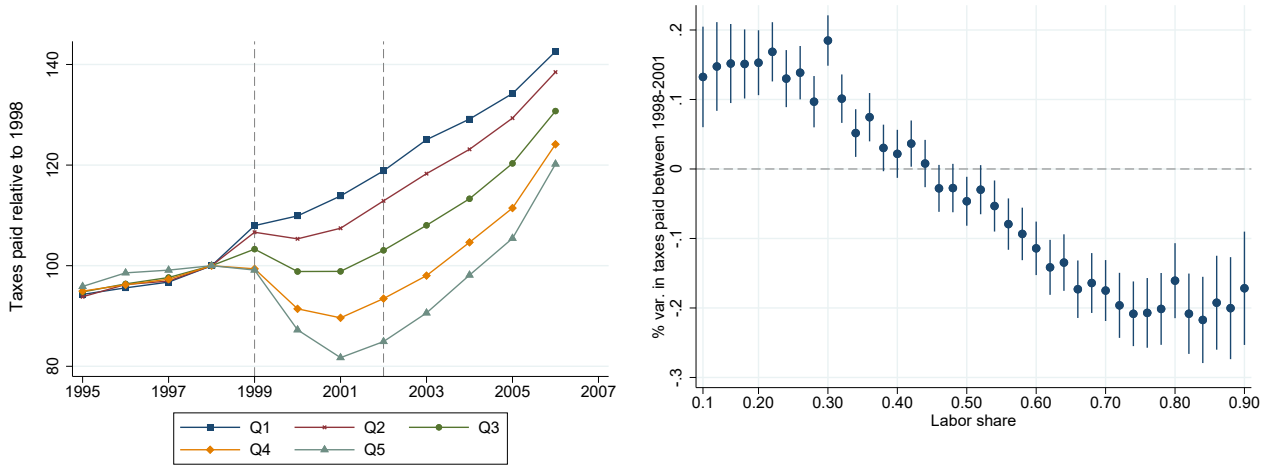
Figure 2: Evolution and distribution of labor shares

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

In Figure 3a, I plot the average taxes paid over time by quintile. Figure 3a shows the taxes paid before the reform evolve similarly across quintiles. But in 1999, when the reform kicks in, the taxes paid by firms in the 5th quintile significantly decrease. I also observe a reduction in taxes paid in the fourth, third, and second quintiles of the labor share distribution, while firms in the first quintile do not show any notable reduction in taxes paid. In Figure 3b, I provide additional evidence that

[20] 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.

this negative correlation holds within quintiles. In Figure B.3, 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.



(a) Evolution of taxes paid by quintiles

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

Figure 3: Correlations 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 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 2 percentage-point bins.

I further describe the sample of estimation in Table 2 and Table A.1 across the 5 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. We note that the tax rate in 1998 does not vary much across quintiles nor the share of industries (except for the construction sector, see Table A.1). These suggest that estimations are unlikely to be driven by differences in tax rates or a particular industry. It is worth noting that within the quintiles, there is still sufficient variation in exposure to the policy.

Table 2: 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)
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.

Alternative proxies. Three potential proxies come naturally: 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 validates 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 Table A.2). 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 Figure B.1 Figure B.2), while the labor share smoothly predicts it (see Figure 3). Table A.3 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.[21] 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 given by:

$$\log(y_{it}) = \sum_{k=1995}^{2006} \beta_k \mathbb{1}_{k \geq 1999} \times LaborShare_i + \gamma_t \times bins_i + \gamma_i + \epsilon_{it} \tag{2}$$

where $y_{i,t}$ is the outcome variable at the firm level i at time t. γ_i is a firm fixed effect, controlling for firms' constant characteristics over the period, such as location. γ_t is a year-fixed effect, controlling for time-varying shocks. I interact the year fixed effects with $bins_i$, 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. $\mathbb{1}_{k \geq 1999}$ is an indicator turning to one from 1999 onward when the reform is implemented. β_k measures the effect of having a relatively higher $LaborShare_i$ in year k. I normalize the last pre-treatment coefficient, β_{1998}, to zero such that all effects are relative to 1998. 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 $\gamma_t \times bins_i$ 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. 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 Table A.4).

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

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*year fixed effects or Labor market*year fixed effects. However, labor intensity is likely to be orthogonal to the development of the Internet. The complete sets of robustness test results 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 no hold in my setting, it is possible that my point estimates are biased.

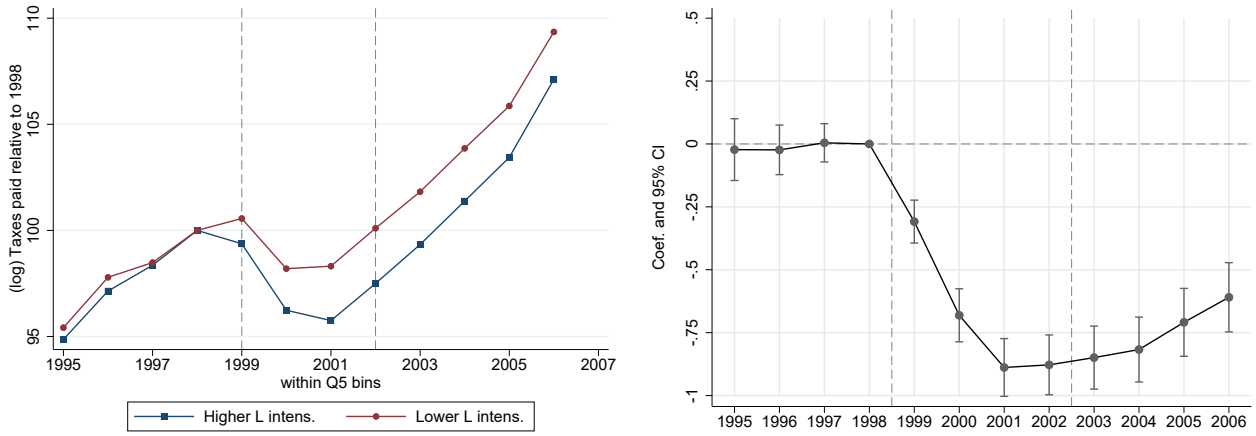
4 Results

In this section, I show the results. First, I show that the labor share proxy explains the tax reduction induced by the reform and review the effects on labor and capital in Section 4.1. I then investigate potential heterogeneity in the sample 4.2 and verify the robustness of my results 4.3.

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.

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. 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 0.5%, this equivalent of a 215 euros additional tax reduction of (see Table A.5).

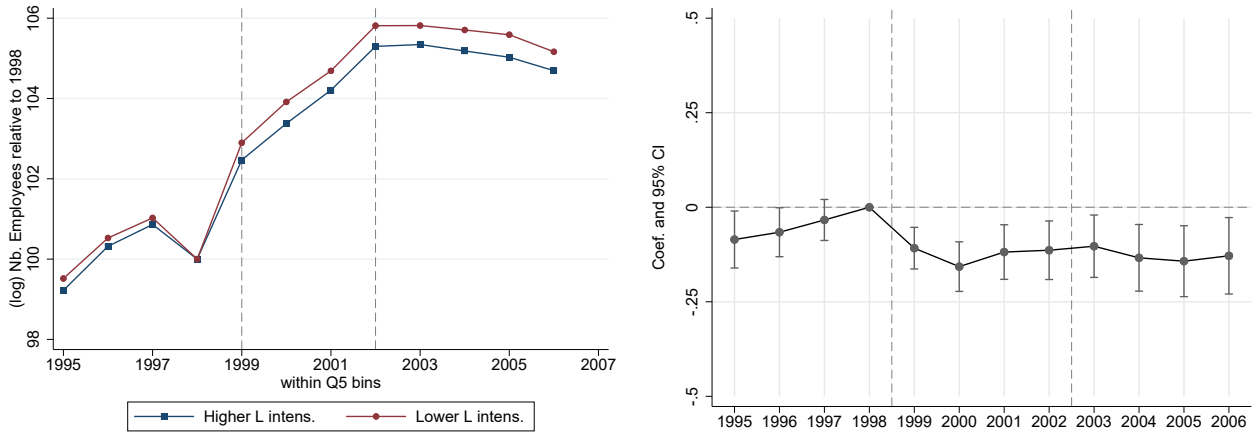


(a) Averages over time **(b) Event Study estimates**

Figure 4: Effects on 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 coefficient resulting from estimating 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. 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. This suggests that the reform did not stimulate employment. 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 Table A.5). Potential explanations for this negative effect are discussed in Section 5.2.



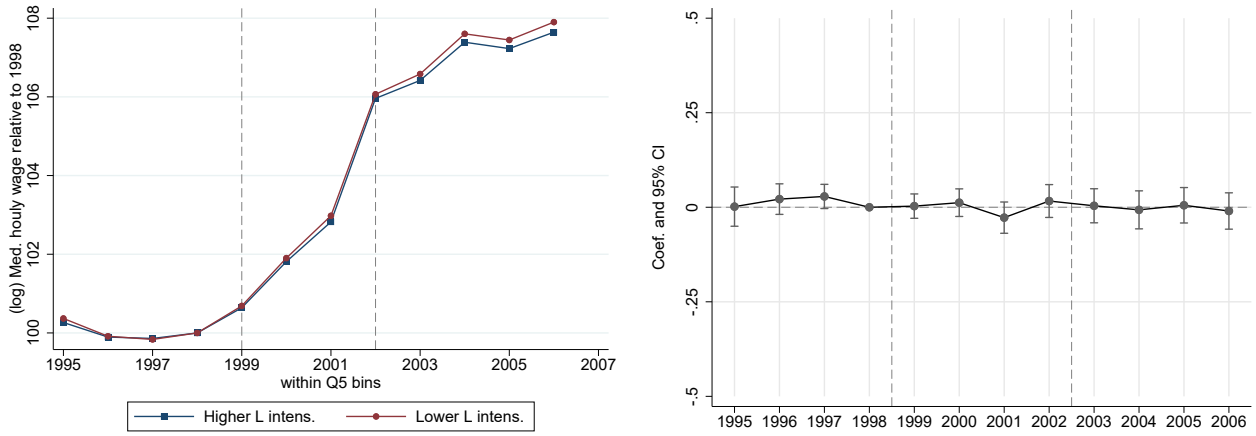
(a) Averages over time **(b) Event study estimates**

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.

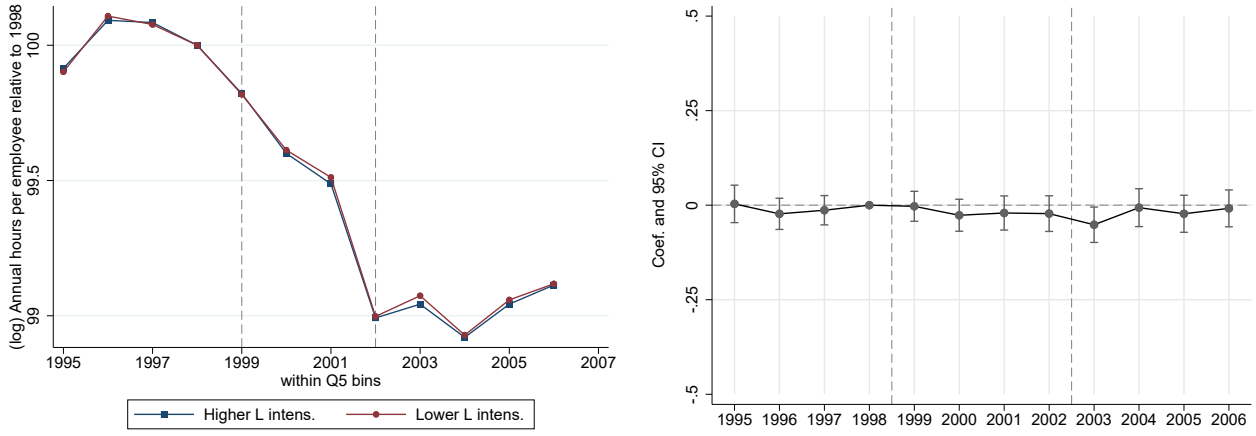
I also investigate whether hours worked per worker vary after the reform (see Figure 7). As labor becomes relatively cheaper, firms could select a group of employees and make them work additional hours. I do not find a significant effect. The coefficient in 2003 is marginally significant and negative. However, the pooled estimate is insignificant (see Table A.6). 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.



(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.



(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.

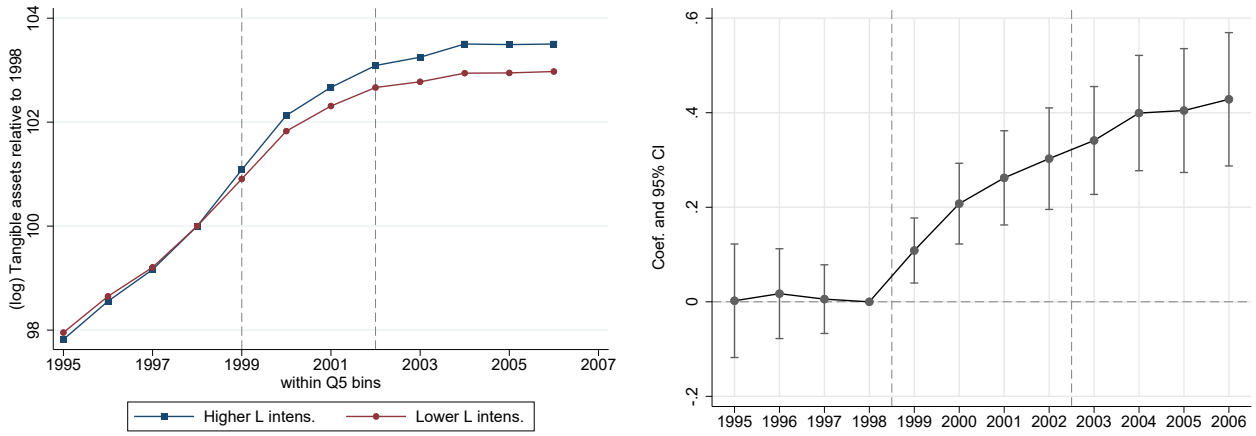
Effects on capital. Although the tax reform targets only labor and capital remains taxed after the reform, firms can still use this tax windfall to invest. I, therefore, investigate the effect on the stock of capital defined by the sum of tangible assets and investment in tangible assets.[22] As one 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 from 1995 to 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 Table A.5). 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 0.69%.[23] This effect is comparable to the investment responses measured by Saez et al. (2019), who find an elasticity close to 0.49.[24] 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 from 3.9 or 6.5 (rescpectivily Zwick and Mahon, 2017; Ohrn, 2018).[25] However, their contexts are quite different as the policy targets capital directly and forces firms to invest to benefit from the tax reduction.

[22] In Figure B.4, I test the robustness of my results with alternative outcomes definitions.

[23] I first compute the elasticity of capital to labor intensity based on the Differences-in-differences estimates, see Table A.7, 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}).

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

[25] 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).

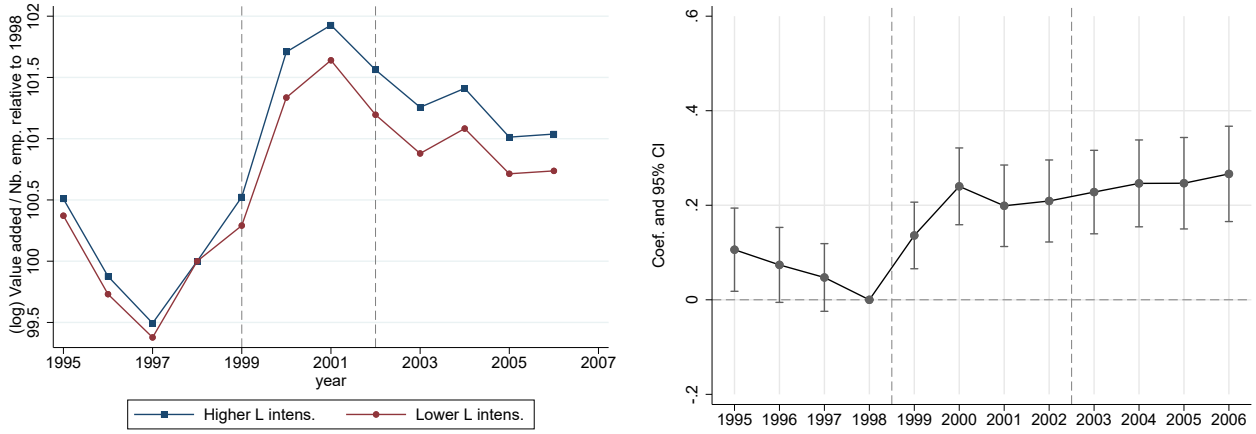


(a) Averages over time **(b) Event study estimates**

Figure 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 effects of the reform on the value added produced divided by the number of employees. Figure 5b displays the results. Productivity increases significantly after the reform. Post-reform coefficients evolve similarly to the coefficients of the regression for the tangible assets; this suggests that the boost in productivity is likely to be the result of strong investment. As the positive effect on the value-added divided by the number of employees could result from the negative effect on employment and the pre-trend are coming form above, one might wonder about the actual effect on the value added. Therefore, I also report the estimates for the log of value added in Figure B.5, where I observe a flat pre-trend and find similar effects.



(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 (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.

4.2 Heterogeneity

Effects by quintiles. Table 3 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 3 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. Additional outcomes are reported in Table A.8.

Table 3: Differences-in-Differences estimates by quintiles of labor intensity

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

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), and the log of tangible assets (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Effects across industries. Table 4 reports point estimates of the Differences-in-Differences model interacted with the industry for the main industries; the other industries can be found in Table A.9. Focusing on the effect on taxes in Table 4 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 2); 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. In Table A.10 column (1), I find a an negative effect which 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: if the highly paid workers are laid off, then the median should drop. It is likely to be driven by a change in the workforce composition as firms with the largest reduction in median hourly wage also reduce employment the most.

Table 4: Differences-in-Differences estimates by industry

	(1)	(2)	(3)
	(log) Taxes paid	(log) Nb. Employees	(log) Tangible assets
C-Manufactures	-0.518***	-0.044	0.333***
	(0.062)	(0.038)	(0.059)
F-Construction	-1.100***	-0.132***	0.177***
	(0.064)	(0.039)	(0.061)
G-Trade	-0.705***	-0.130***	0.434***
	(0.061)	(0.036)	(0.057)
I-Hospitality	-0.537***	-0.066	0.730***
	(0.077)	(0.045)	(0.079)
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.

Effects by firms' size. Table 5 reports point estimates of the Differences-in-Differences model interacted with the firms' size in 1998; additional outcomes can be found in Table A.11. Focusing on the effect on taxes in Table 5 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 as well as 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.

Table 5: Differences-in-Differences estimates by size in 1998

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

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), and the log of tangible assets (3). ***, ** 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 larger firms. Investment responses are particularly pronounced for smaller firms.

4.3 Robustness

Bins. As underlined in the previous section, I verify that my point estimates are not the results of bins choices. In Figure B.6, 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 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 is mechanically reduced, as well as the number of observations, 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 Figure B.8, 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 (Figure B.8a and B.8b, 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 (Figure B.8e).

Local shocks. Another concern for identification is confounding shocks that coincide with the reform, but have no visible effect before 1999. So in Figure B.9, 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. 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. 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. As discussed in Section 2.2, a mandatory reduction in working time was enforced starting in 2000. This reform reduced the weekly working time by about 10%, from 39 to 35 hours worked per week. 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).[26] 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 (Figure B.10), the hours worked in the previous year (expressed in hours per year or week), and the year of plausible transition.[27] The results remain consistent with the baseline across all outcomes (Figure B.11). Additionally, I restrict the sample 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 Figure B.12, 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.

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 Figure B.13, 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

[26] 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.6).

[27] 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.

the event study, and I find similar effects (Figure B.14). In Figure B.7, I reported point estimates when excluding the information and telecommunication from the baseline sample and show that they are not driving the effects. This supports that my estimates are not the result of the end of the "dot-com" bubble.

5 Potential Mechanisms

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 substantially invest at the expense of employees. 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.

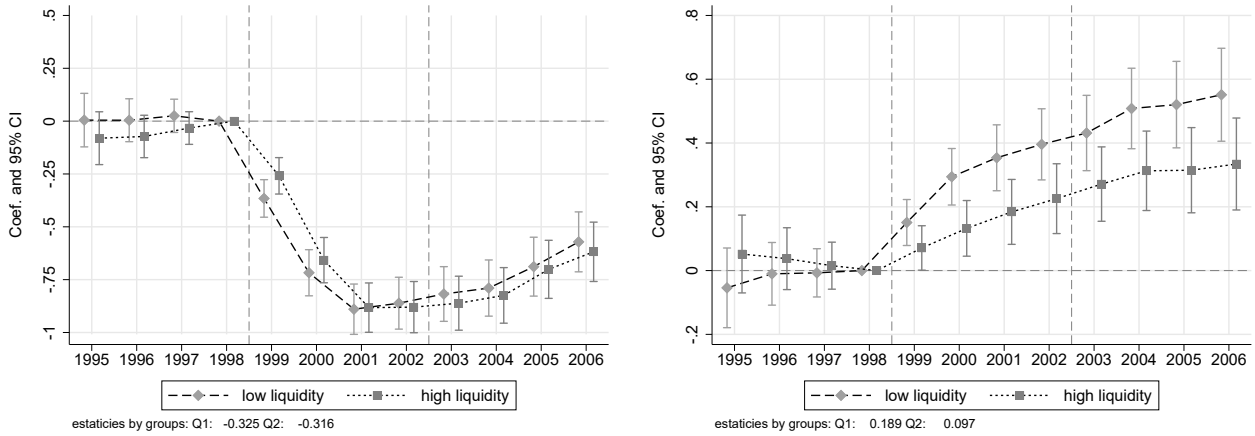
Modigliani and Miller (1958) predict that if external financing is more costly than internal financing, cash injections should have a positive effect on capital expenditures.[28] Evidence from the public finance literature highlights that cash injections affect firms' growth potentially due to credit 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.[29] 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

[28] External financing could be costly because of asymmetric information or incomplete contracting.

[29] I use the value of circulating assets divided by the total assets net of depreciation.

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 Table A.12). 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 Table A.13).[30] However, I do not find significant differences in the effect on employment.



(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 differences in employment growth

Several papers find either a positive effect on employment following a corporate (Duan and Moon, 2023) or a payroll tax reduction (Saez et al., 2019; Benzarti and Harju, 2021a) or no effect at all (Harju et al., 2022). Tax incentives targeting capital usually also boost employment (e.g. Curtis et al., 2022; Duan and Moon, 2024). However, targeted tax incentives such as bonus depreciation and accelerated

[30] Event study estimates for all outcomes can be found in Figure B.15.

depreciation policies are typically granted only for new investments in specific types of assets, and the tax reductions provided by these programs are materialized only when and if a firm invests. This targeted tax incentive is, therefore, more likely to spark the economic activity of firms. In contrast, in the case of tax variations in my setting, taxes are reduced no matter whether firms invest or hire. However, this does not explain why relatively less exposed firms have higher employment growth after the reform – resulting in negative point estimates.

Differences in corporate tax liabilities. A potential explanation for this difference in employment growth is the possibility of deducting the business tax payment from the corporate income tax. Firms paying corporate tax can 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, firms that do not pay corporate taxes because they are not profitable will 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, 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. The firm faces a tax on the wage bill τ_p (for simplicity, I abstract from the part levied on capital) 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.[31] The firm after-tax profit (Π) is then:

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

where w is the wage and r the interest rate. For simplicity, I normalized the price of output to 1. I assume that the firm is a price taker in the product market and that the non-tax costs of capital are not affected by changes in the corporate tax or the business tax. I assume that labor is perfectly mobile across jurisdictional borders.[32] Consequently, a change in the local tax on wages leaves wages in the competitive sector unchanged. The firm chooses capital K and employment L so that the after-tax

[31] 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 $\rho \in (0; 1)$.

[32] The average French municipality is small.

profit is maximized. Using this model, I show that $\frac{\partial L}{\partial \tau_p} < 0$, this implies that when the tax on the wage bill increases, the labor demand reduces; therefore, employment drops. And under reasonable assumptions, I also show that $\frac{\partial L}{\partial \tau_p \partial \theta} < 0$ – the details of the derivation can be found in Appendix D. This implies that the more the firm can deduct the tax payment of the wage bill tax from the corporate tax base, the less the firm will react to a change in the wage bill tax.

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 11 the average number of employees over time, distinguishing two groups: the firms paying the corporate tax on profit 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 liable for corporate tax is larger than that of those that are liable.[33] 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.

[33] Averages by quintiles are displayed in Figure B.16, 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.

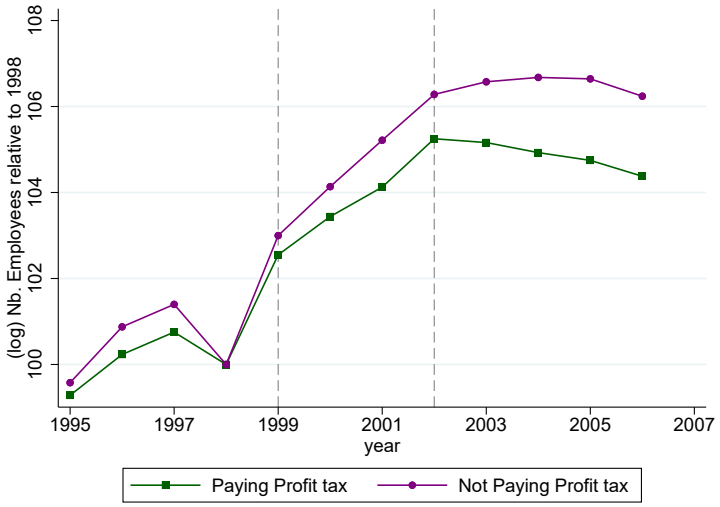


Figure 11: Employment effect by corporate tax liabilities – averages over time

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 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.

Furthermore, the heterogeneity analysis shows that the negative effects on employment are driven by (i) capital-intensive firms, i.e., belonging to quintiles 1 and 2 of the labor intensity distribution (see Table 3); (ii) the construction and trade sector (see Table 4); (iii) firms with less than ten employees or more than 50 (see Table 5). In Table 6, I observe that firms not paying the corporate tax in 1998 are indeed more likely to be in the construction, trade, or hospitality sector. They are also more likely to have less than 10 employees and are more likely to be in the first and second quartiles of the labor share distribution. On top of that, in the Table, I further show that they are more likely to be in the lower part of the within-bin labor share distribution. In other words, subgroups where I estimate strong negative effects tend to be more likely to not be liable for the corporate tax rate and be in the lower part of the with bin labor share distribution. This, therefore, explains why I estimate a strong negative effect for these groups and supports the insights from the model according to which firms not paying the corporate tax respond more to the reform.

Table 6: Characteristics of firms in 1998 by corporate tax liabilities

	(1) Not liable	(2) Liable	(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.

Differences in substitution capacities. An alternative explanation could be that relatively less labor-intensive firms are initially more likely to substitute capital for labor than relatively more labor-intensive ones. As a result, when labor is not taxed anymore, they can adjust more than relatively more labor intensive; they adapt their production function in favor of the relatively cheaper input: labor. In this case, the substitution effect would dominate the income effect. More labor-intensive firms are still limited when it comes to substitution and, therefore, adjust labor at lower levels and increase capital more substantially.

6 Concluding remarks

In conclusion, this study highlights how input-based tax reforms can meaningfully impact firms' resource allocation between labor and capital, ultimately shaping firm-level productivity and growth. By examining the effects of a reform that removed labor from the French local business tax base, I find that reducing tax liabilities on labor led firms to shift resources towards capital investment, significantly increasing tangible assets and overall productivity. Although the reform did not impact wages or hours worked, employment growth diverged between capital- and labor-intensive firms, with capital-intensive firms –particularly those not subject to corporate taxes – showing stronger employment growth.

The findings indicate that the liquidity effects of tax relief enable cash-constrained firms to increase investment, while the interaction between local business tax deductibility and corporate tax sensitivity further influences employment responses. In particular, firms unable to deduct labor costs from corporate taxes saw the most pronounced employment growth, as the reform alleviated prior constraints on labor demand.

These findings underscore the importance of considering firm characteristics – such as capital intensity, size, and tax sensitivity – when designing tax policies to balance employment and investment objectives. By shedding light on how tax base composition affects firms' decisions, these results contribute to a nuanced understanding of business tax reform impacts, providing valuable insights for policymakers aiming to enhance economic performance through targeted tax adjustments.

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A Additional Tables

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

	Q1	Q2	Q3	Q4	Q5
Total sale (in k €)	2713.876	2805.884	2530.117	2281.348	2177.528
	(4093.996)	(4321.265)	(4044.022)	(3768.170)	(3614.608)
Circulating assets (in k €)	20.761	22.943	24.871	27.094	26.912
(Inventories + Claims + Cash flow)	(56.493)	(61.067)	(64.202)	(66.476)	(66.191)
Total assets (in k €)	1406.844	1415.467	1334.594	1254.209	1237.715
(net of depreciation)	(2391.390)	(2634.612)	(2636.237)	(2627.881)	(2632.144)
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)
Gross operating surplus	185.894	192.022	170.401	195.417	170.674
(EBITA)	(564.487)	(969.013)	(725.850)	(3931.095)	(1657.846)
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)
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.2: 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 $\times$ 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.3: 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 $\times$ 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.4: 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.5: Differences-in-Differences estimates

	(1) (log) Taxes paid	(2) (log) Nb. Employees	(3) (log) Tangible assets
$\text{Post}_{99} \times$ 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.6: Differences-in-Differences estimates - additional outcomes

	(1) (log) Medium Hourly wage	(2) (log) Hours worked per worker	(3) (log) Value added/ Worker
$\text{Post}_{99} \times$ LaborShare$_i$	-0.013	-0.012	0.165***
	(0.030)	(0.014)	(0.023)
Firm FE	✓	✓	✓
Q5 x Year FE	✓	✓	✓
Obs.	663924	663924	662641
R2	0.789	0.558	0.745

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 tangible assets (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.7: Elasticity to the labor intensity

	(1)	(2)	(3)
	Taxes paid	Employment	Capital
ξ_y^{Lsh}	-.235 ***	-.0353**	.162***
	(.0132)	(.0147)	(.0339)
pvalue	0.000	.0164	0.000
	(4)	(5)	(6)
	Median hourly wage	Hours worked	Productivity
ξ_y^{Lsh}	-.0061	-.005	. .083***
	(.008)	(.006)	(.0162)
pvalue	.422	.399	0.000

Note: This table reports the computed elasticities based on the difference-in-differences estimates. The computation is the following: $\xi_y^{Lsh} = (\exp\beta - 1) * \overline{LaborShare}$, 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.8: Differences-in-Differences estimates by quintiles of labor intensity

	(1)	(2)
	(log) Med. hourly wage	(log) Annual hours per employee
1^{st} Quintile Labor Sh.	0.019	-0.220***
	(0.073)	(0.070)
2^{nd} Quintile Labor Sh.	-0.013	-0.054*
	(0.034)	(0.030)
3^{rd} Quintile Labor Sh.	0.004	0.025
	(0.033)	(0.029)
4^{th} Quintile Labor Sh.	-0.029	0.001
	(0.028)	(0.023)
5^{th} Quintile Labor Sh.	-0.012	0.059
	(0.060)	(0.047)
Firm FE	✓	✓
Q5 x Year FE	✓	✓
Obs.	663924	663924
R2	1	1

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 labor costs (3). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table A.9: 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.10: Differences-in-Differences estimates by industry

	(1)	(2)	(3)
	(log) Med. houly wage	(log) Annual hours/employee	(log) Value added/employee
C-Manufactures	-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
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.11: 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	✓	✓	✓
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.12: 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

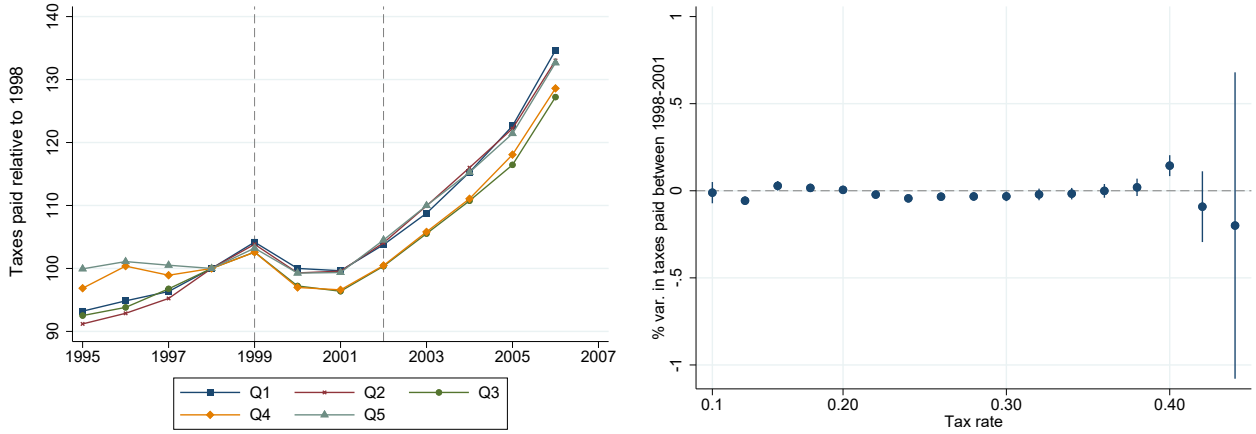
Note: This table compares the average labor share across financially constrained and unconstrained firms. Each group is further divided based on whether their labor intensity is above or below the within-bin median. The last column reports the difference between the difference existing within financially 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.13: Differences-in-Differences estimates by financial constraint

	(1) (log) Taxes paid	(2) (log) Nb. Employees	(3) (log) Tangible assets	(4) (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.

B Additional Figures

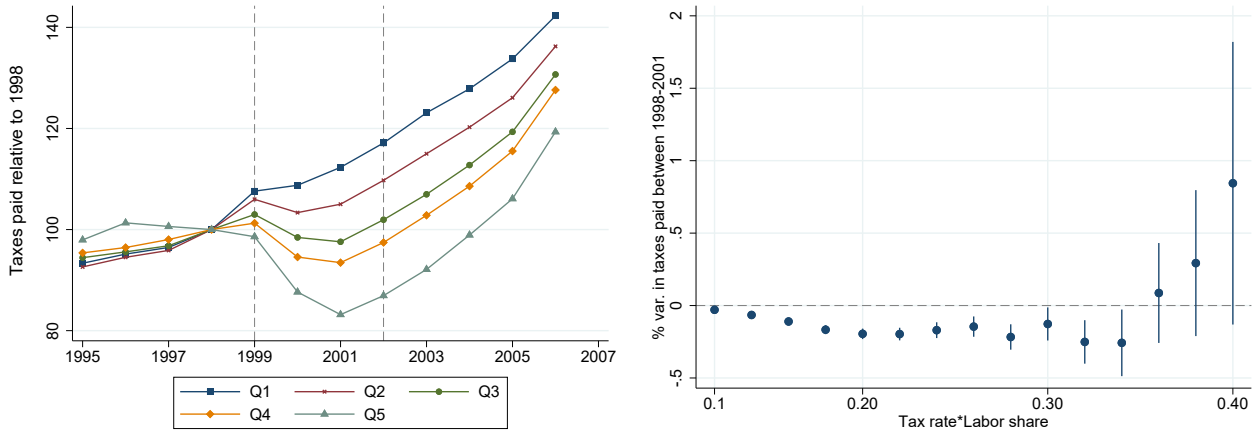


(a) Evolution of taxes paid by quintiles of the tax rate in 98

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

Figure B.1: 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.



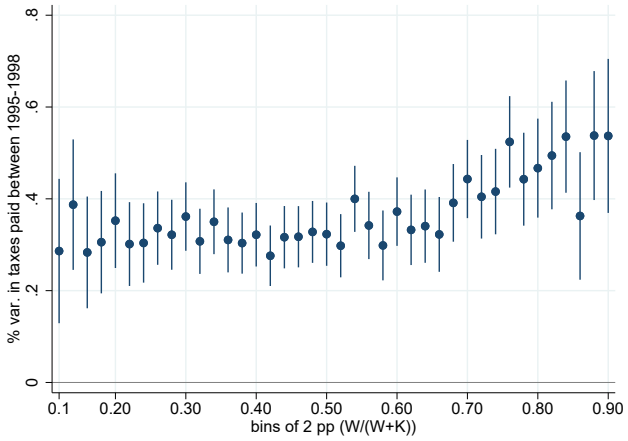
(a) Evolution of taxes paid by quintiles of the tax rate×Labor share

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

Figure B.2: 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.3: Percentage change in taxes paid between 1995 and 1998 by 2 pp bins



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.

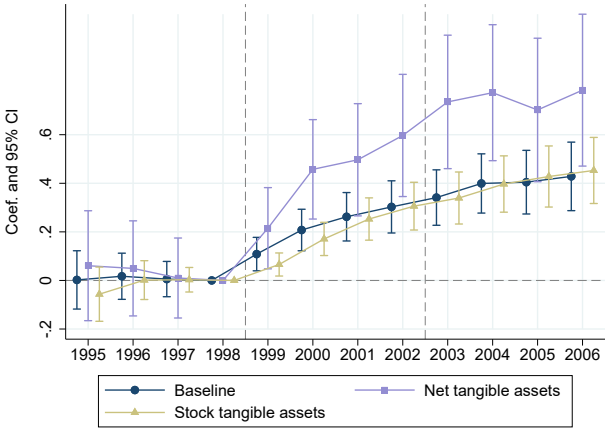


Figure B.4: Effects on Capital - alternative definitions

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.

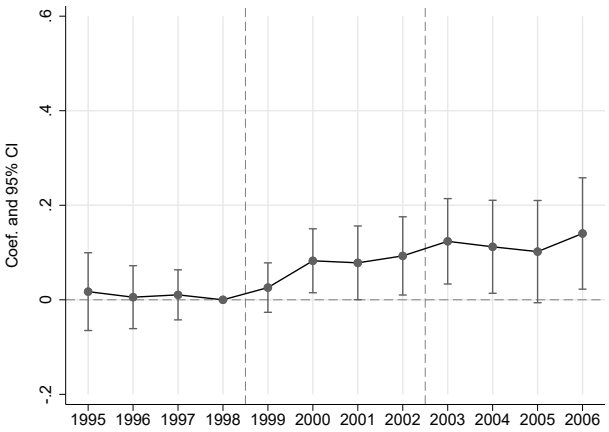
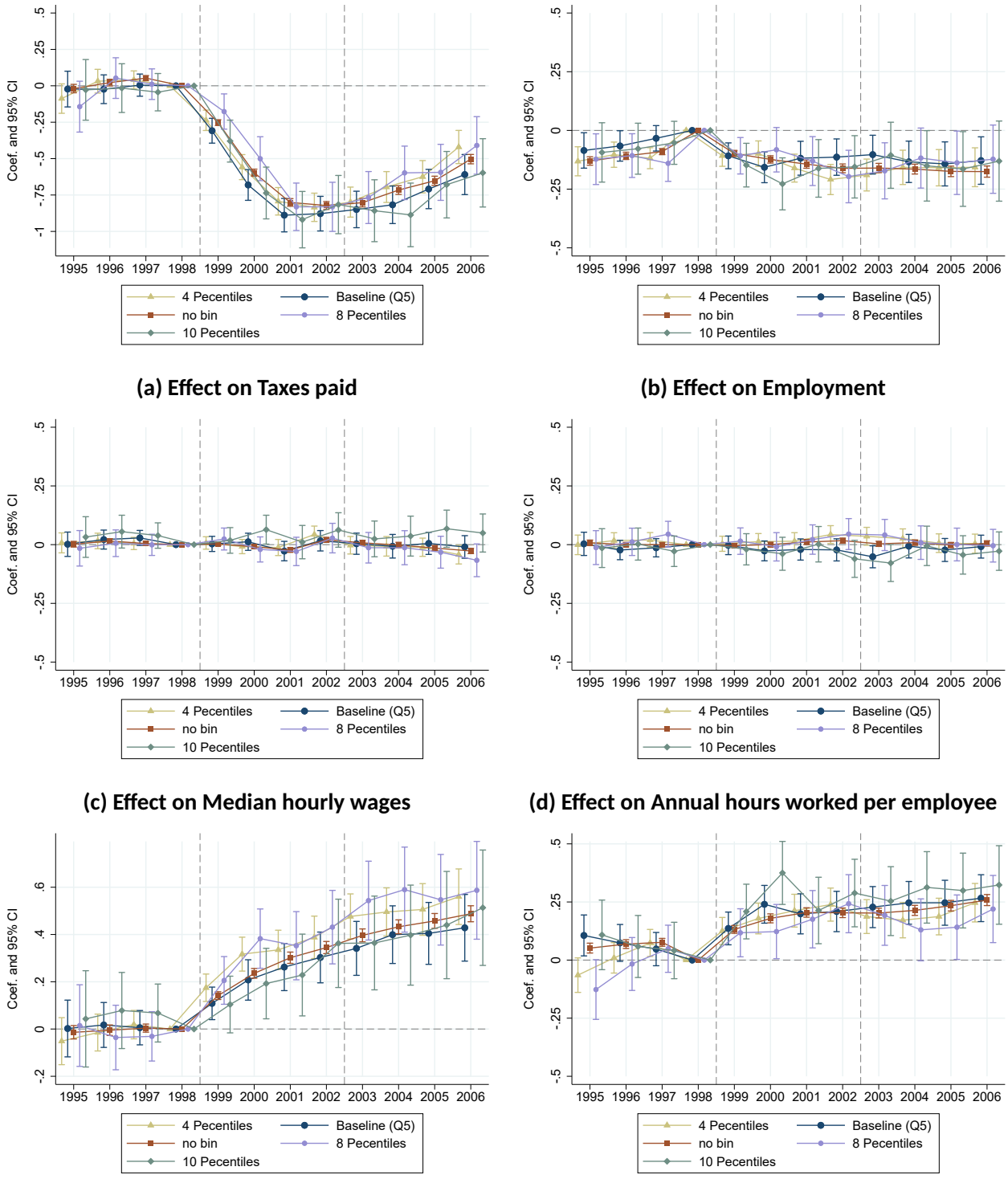


Figure B.5: Effects on Value added

Note: This figure plot 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.



(a) Effect on Taxes paid

(b) Effect on Employment

(c) Effect on Median hourly wages

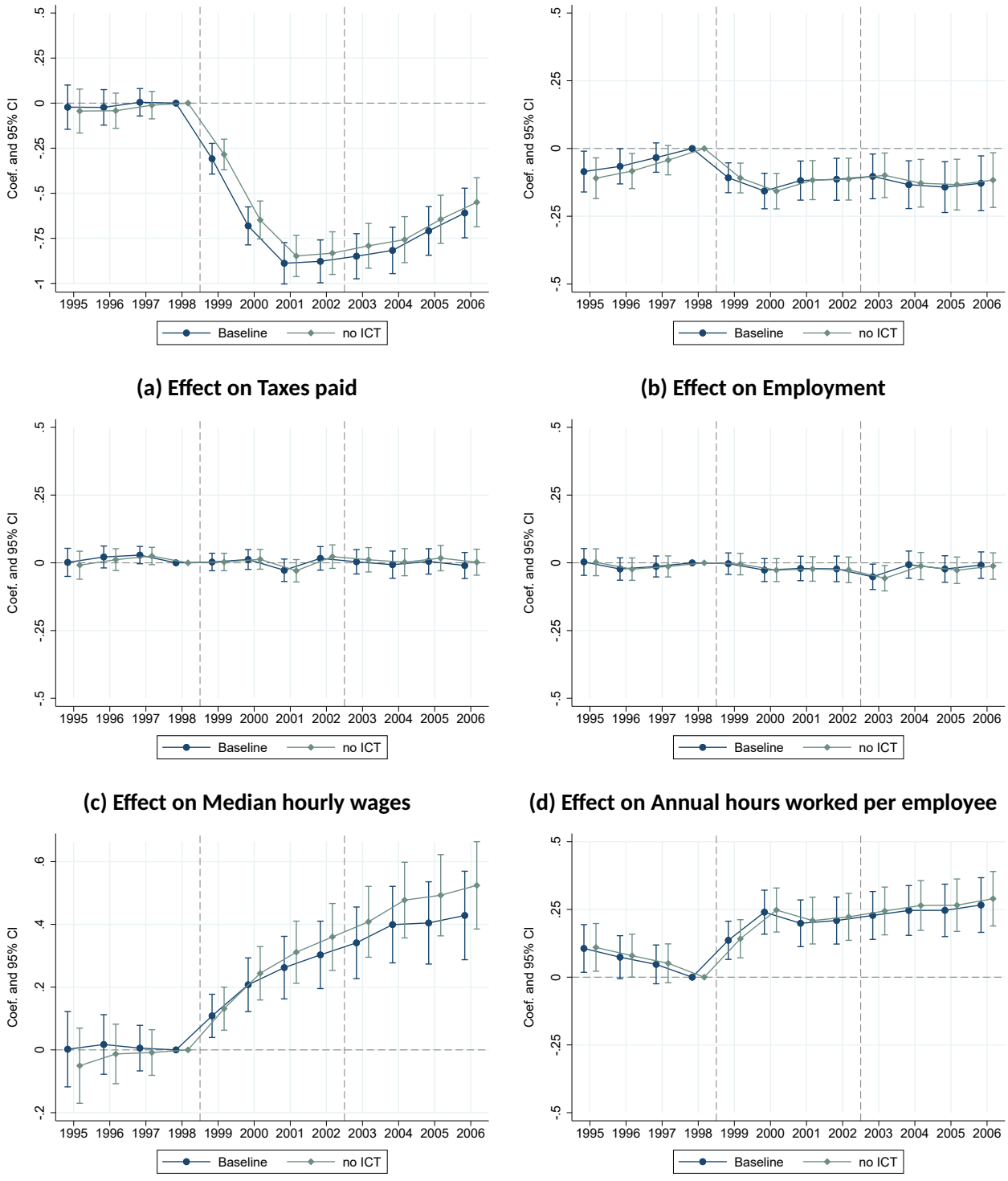
(d) Effect on Annual hours worked per employee

(e) Effects on Capital

(f) Effects on Productivity

Figure B.6: 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

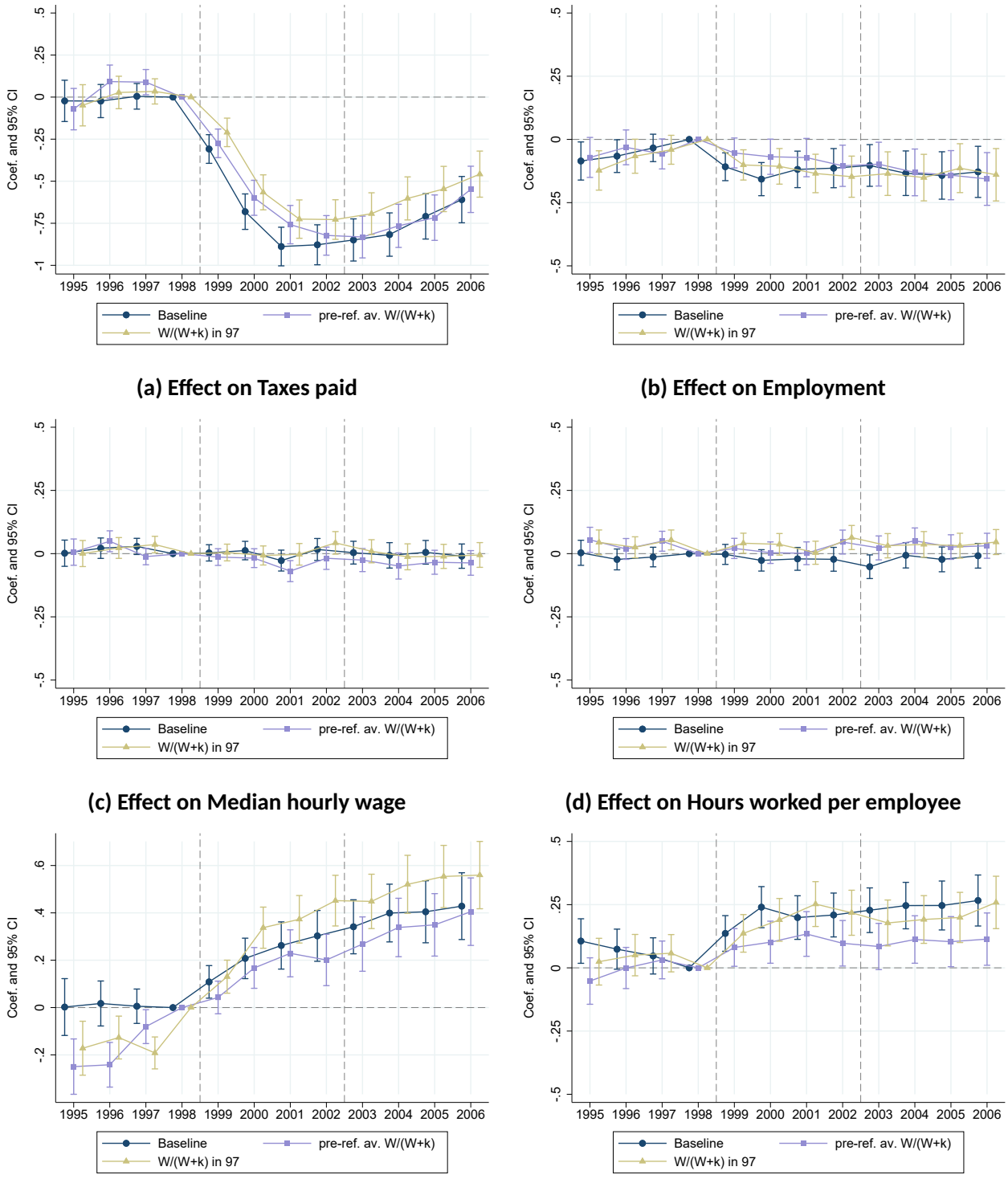
(d) Effect on Annual hours worked per employee

(e) Effects on Capital

(f) Effects on Productivity

Figure B.7: 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

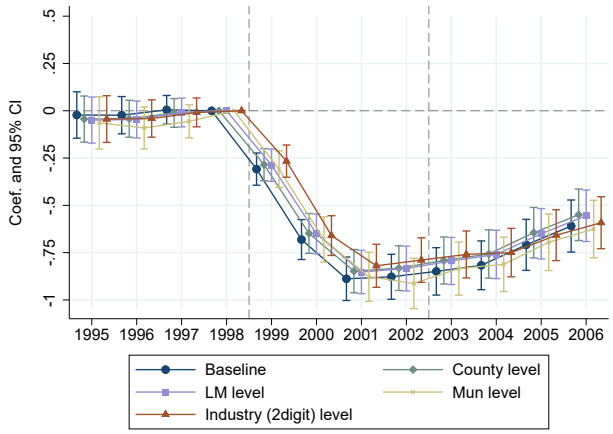
(d) Effect on Hours worked per employee

(e) Effects on Capital

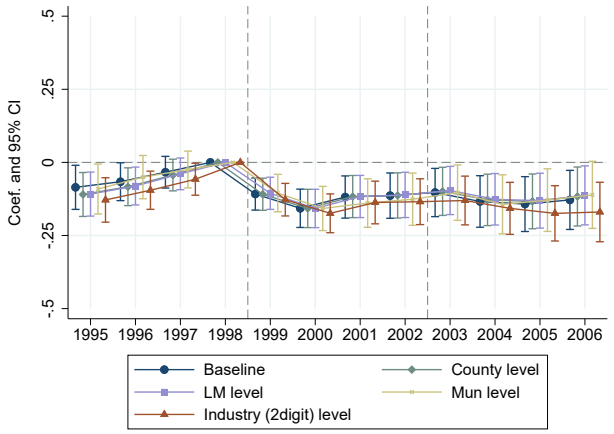
(f) Effect on Productivity

Figure B.8: Robustness: change in the measure of labor share.

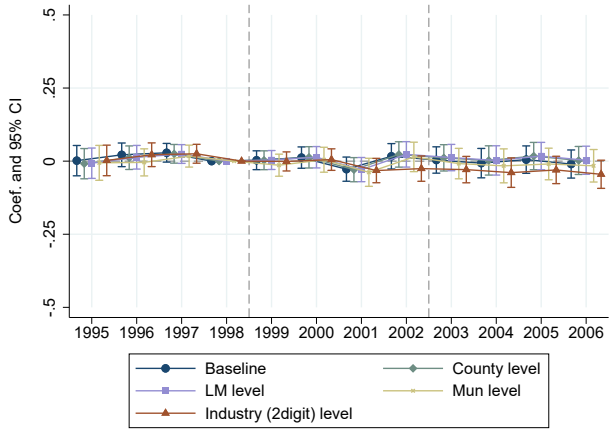
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) with different measures of labor share: the labor share calculated in 1997 – labeled "W/(W+K) in 97", and the average labor share in the pre-reform period (1995-1998) – 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.



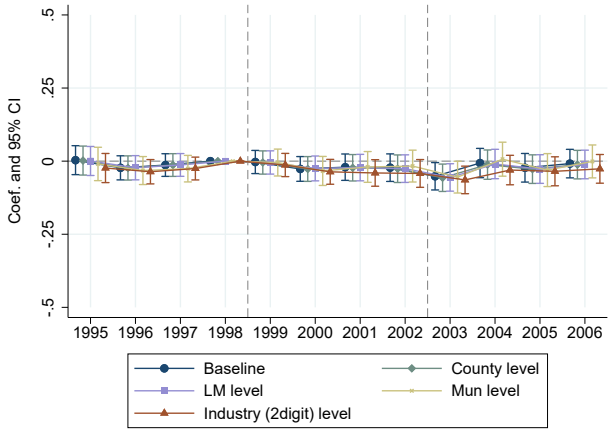
(a) Effect on Taxes paid



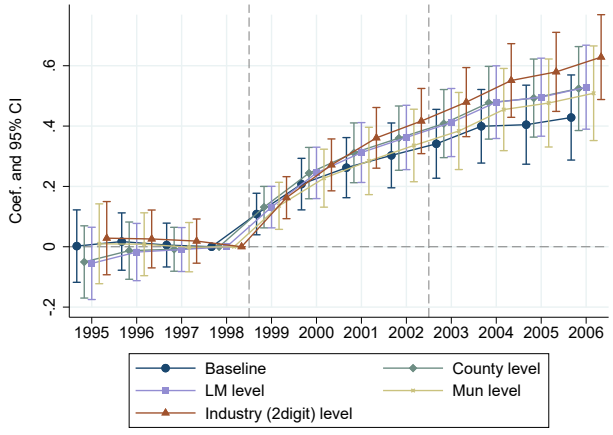
(b) Effect on Employment



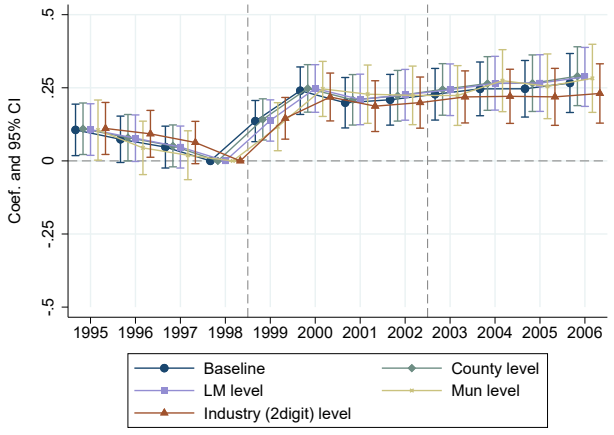
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



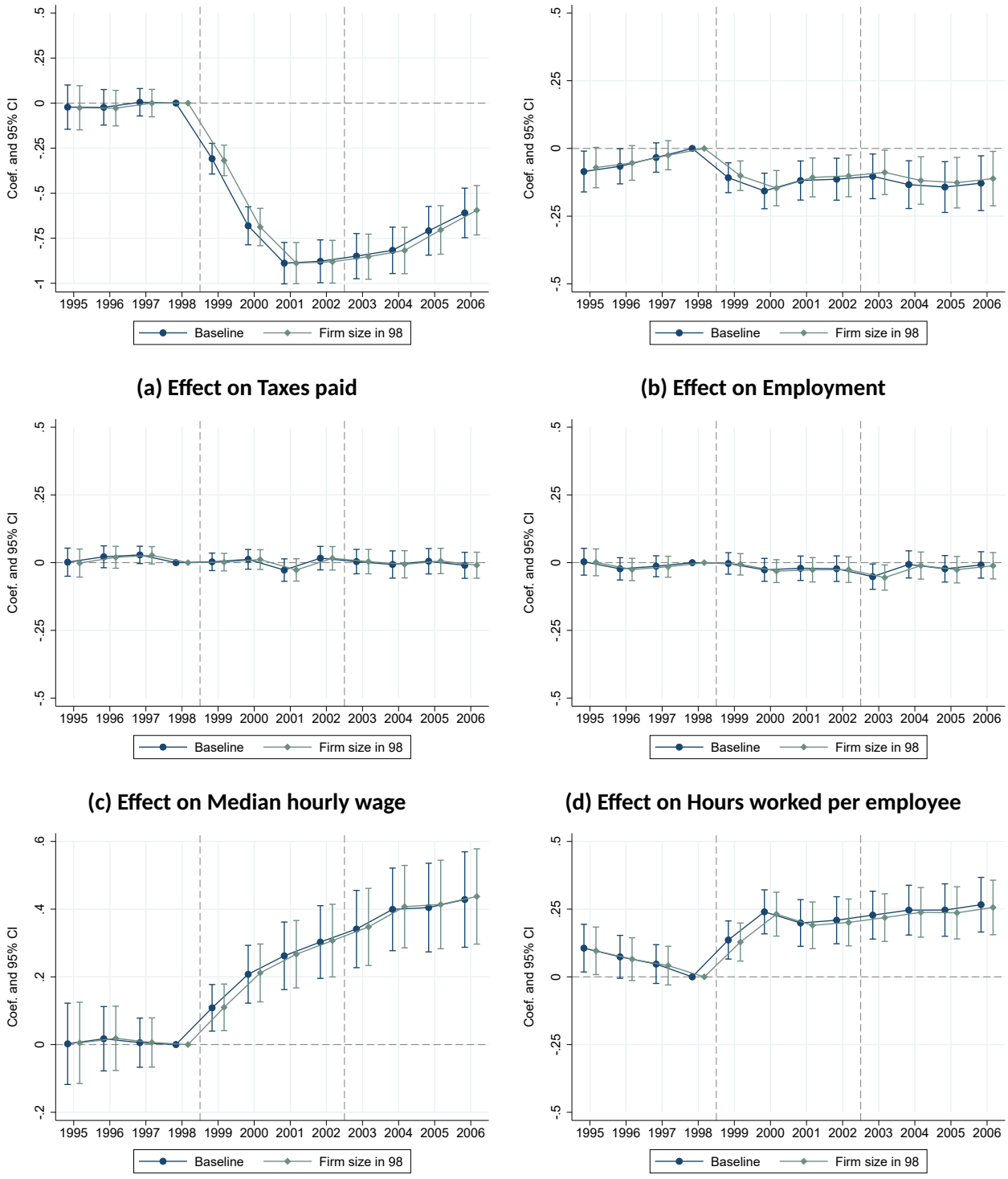
(e) Effects on Capital



(f) Effect on Productivity

Figure B.9: 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

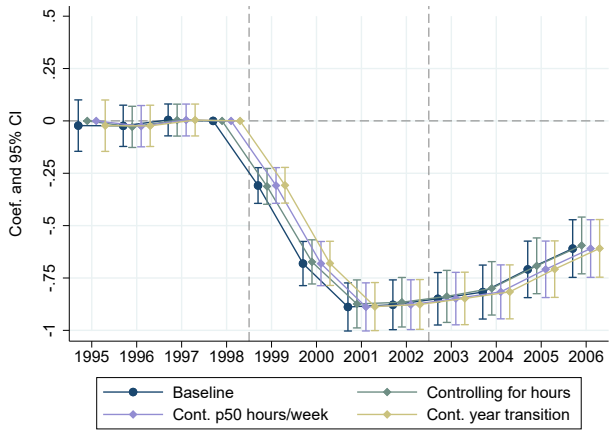
(d) Effect on Hours worked per employee

(e) Effects on Capital

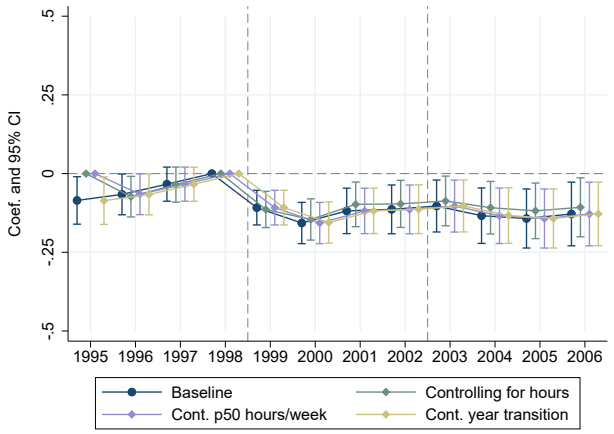
(f) Effects on Productivity

Figure B.10: Robustness: controlling for firms' size.

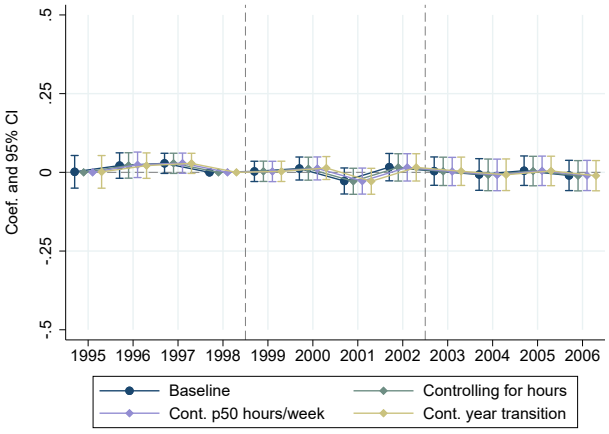
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2) and an alternative model controlling for the firm size using seven categories: less than 10, 10-19, 20-49, 50 to 249, 250 to 499, 500 to 999, and more than 999 employees. 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.



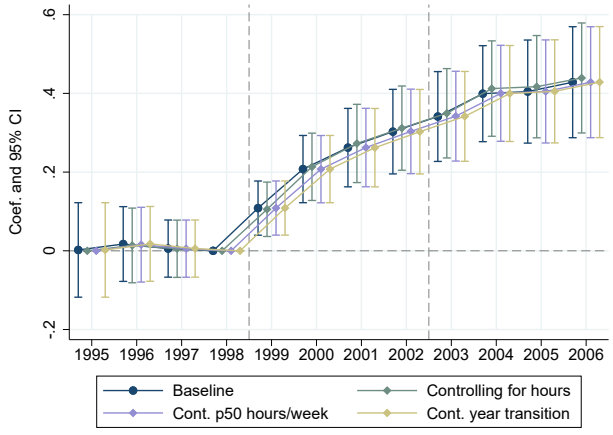
(a) Effect on Taxes paid



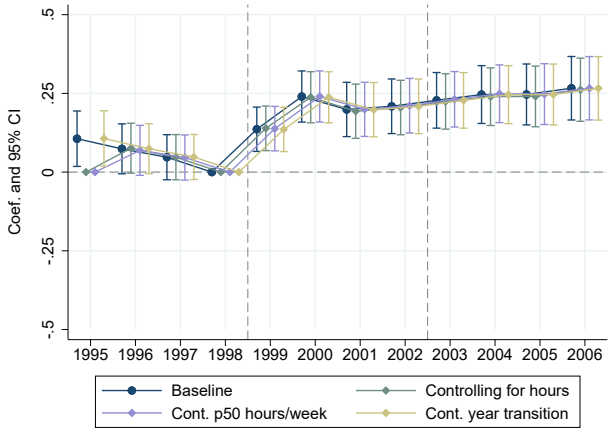
(b) Effect on Employment



(c) Effect on Median hourly wage



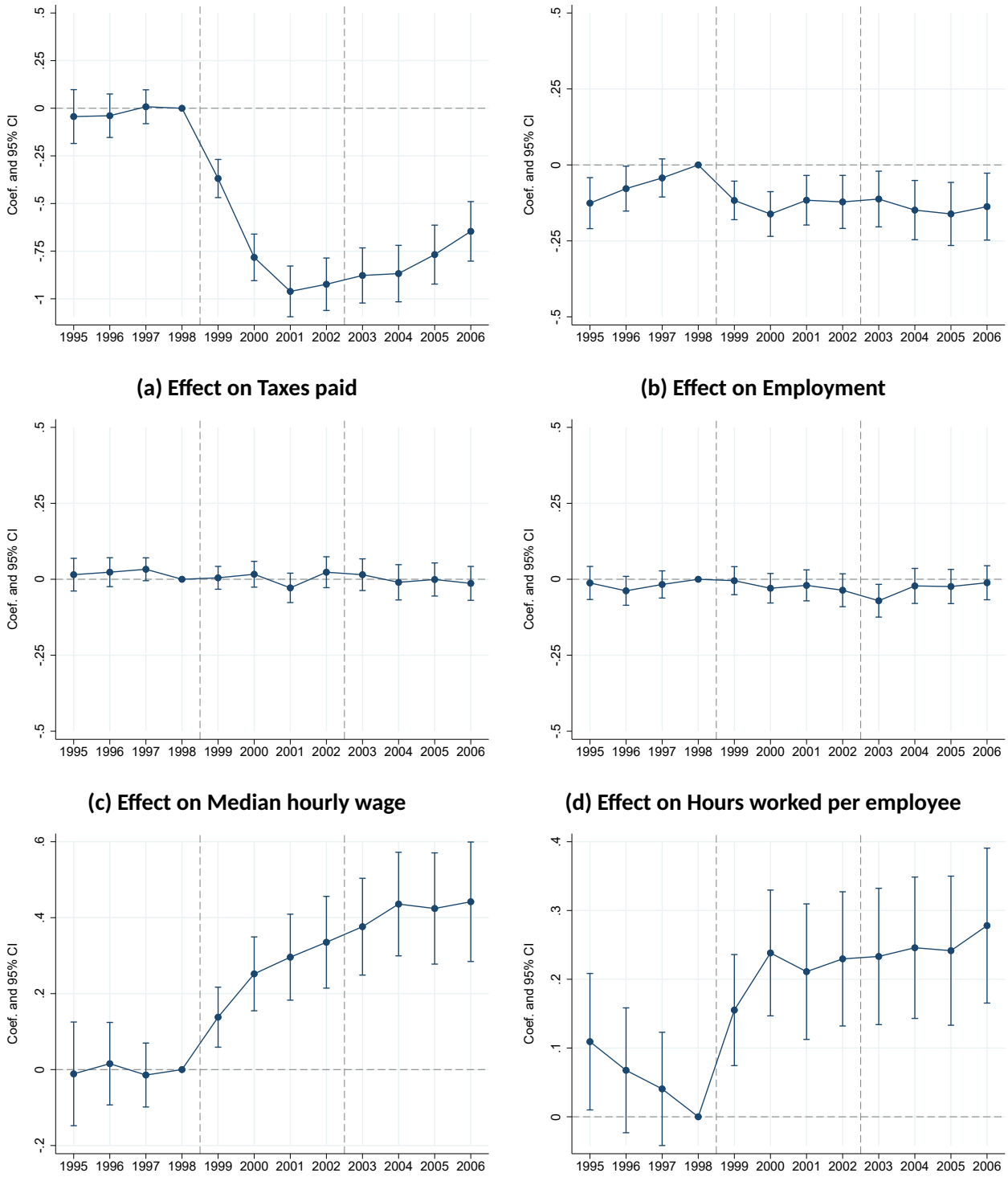
(d) Effects on Capital



(e) Effect on Productivity

Figure B.11: 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

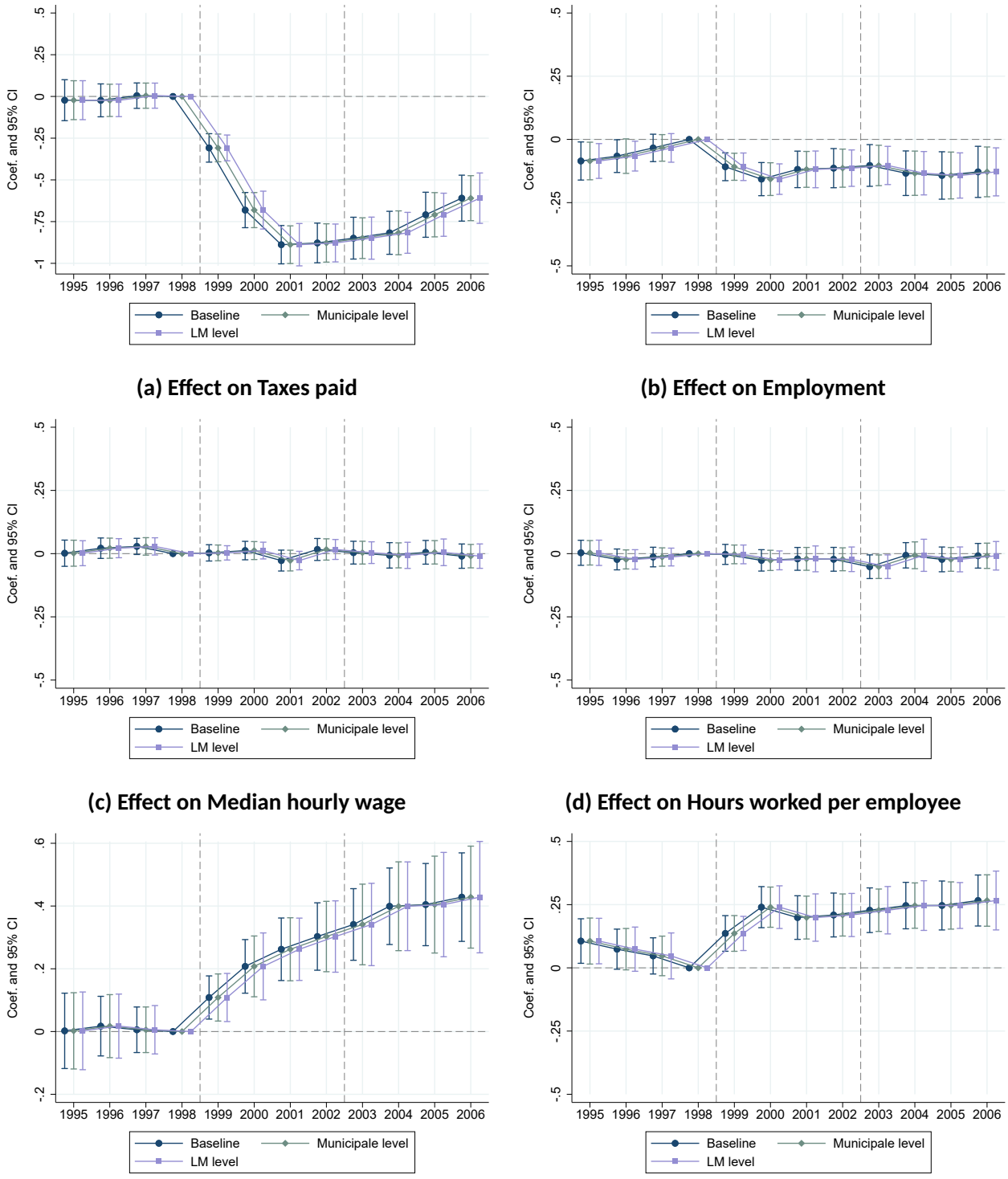
(d) Effect on Hours worked per employee

(e) Effects on Capital

(f) Effect on Productivity

Figure B.12: 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

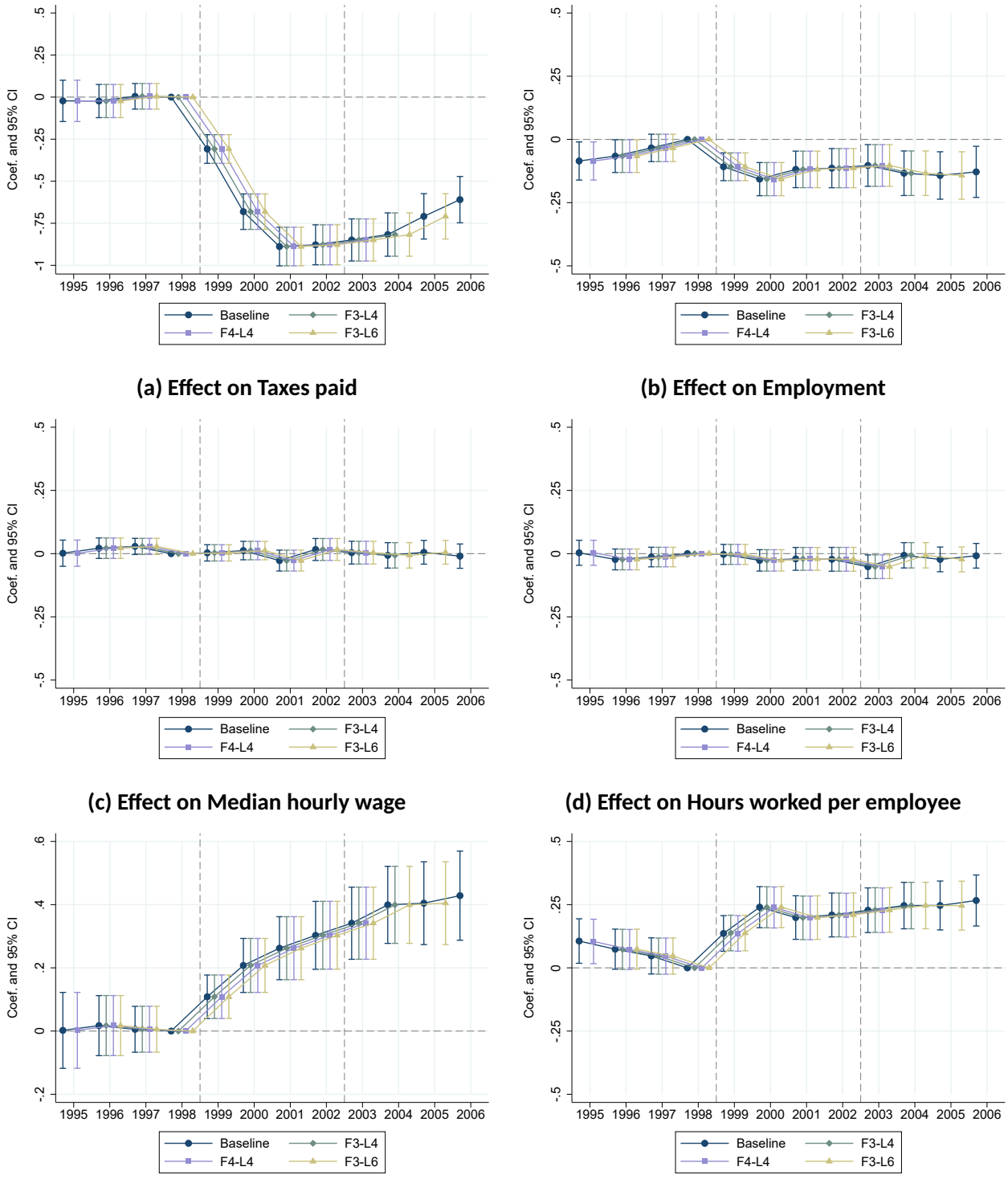
(d) Effect on Hours worked per employee

(e) Effects on Capital

(f) Effect on Productivity

Figure B.13: 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$\simeq$36,000) or the local labor market (n$\simeq$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

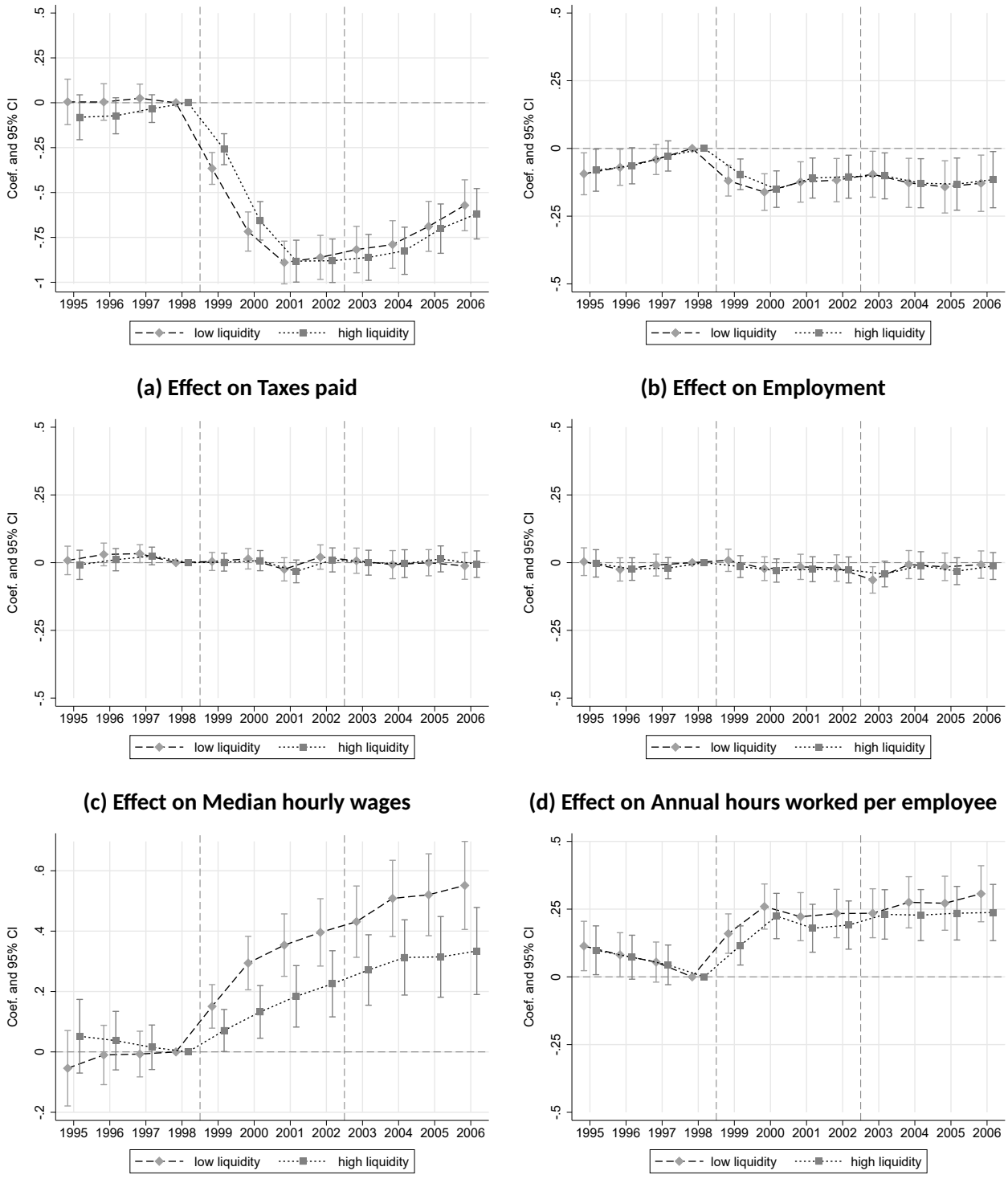
(d) Effect on Hours worked per employee

(e) Effects on Capital

(f) Effects on Productivity

Figure B.14: 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.15: 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.

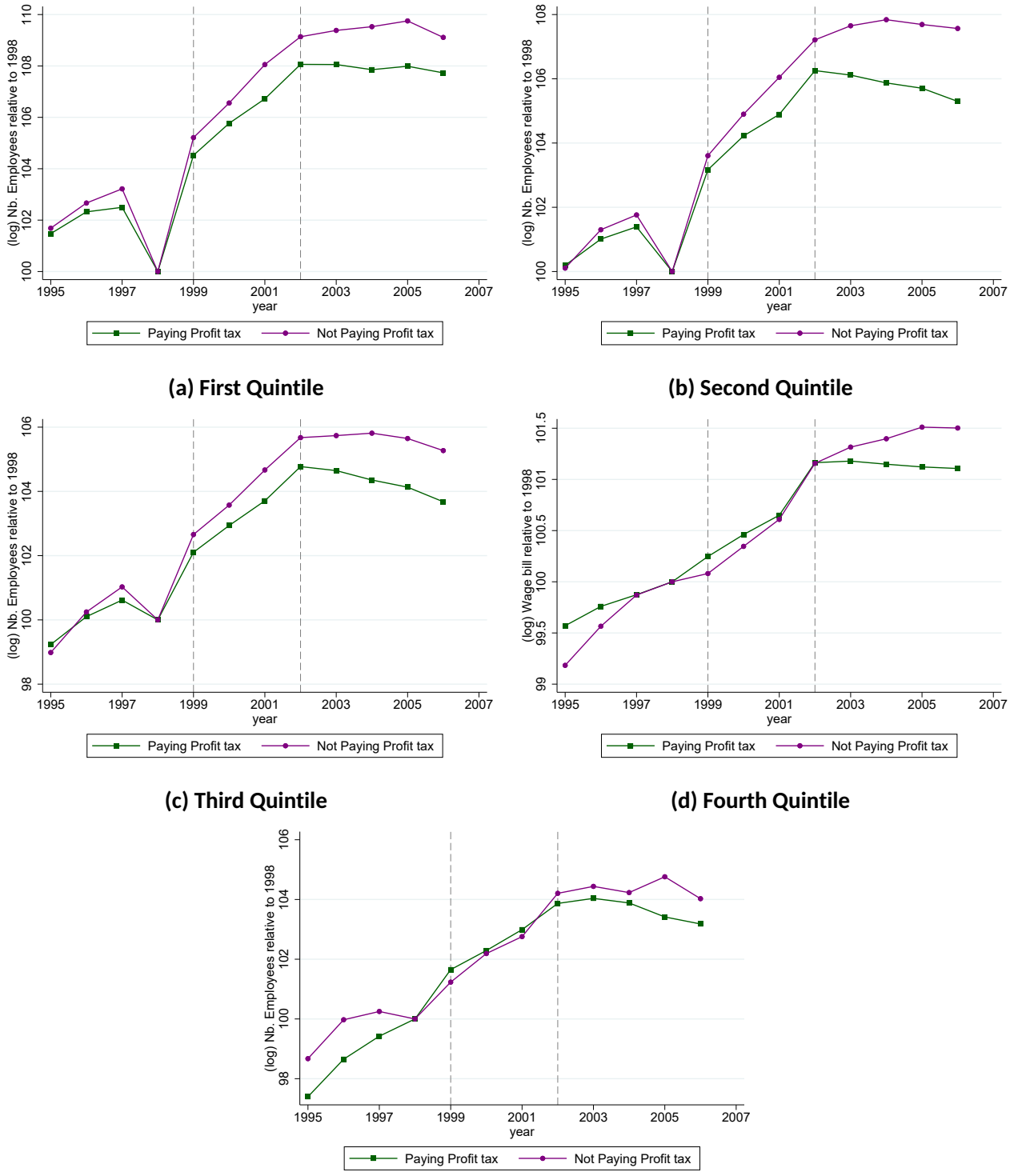


Figure B.16: Average number of employees over time across quintiles by profit tax liabilities

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.

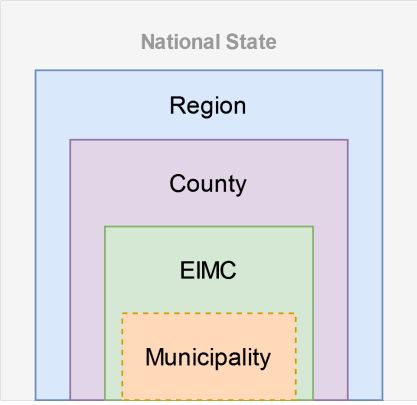


Figure C.1: Administrative divisions

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.[34]

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.

[34]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 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.[35]. Firms' after-tax profit (Π) is then:

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

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:

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

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:

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

[35] 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 $\rho \in (0; 1)$.

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.