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Chapter 1

General Introduction

Public policies aim to achieve key macroeconomic objectives, including price and exchange rate stability, economic growth, and full employment of production factors (Tinbergen, 1956). Among these goals, employment remains a cornerstone, as it provides income, supports consumption, and fosters social integration and economic activity (e.g., Layard et al., 2005). At the individual level, unemployment also reduces future employment prospects and well-being, particularly among young people (Gregg and Tominey, 2005; Sullivan and Von Wachter, 2009; Schmieder et al., 2023). Over the past decades, numerous policies have been implemented to reduce labor market frictions, with some developed in response to economic crises and others targeting structural unemployment. Yet, despite widespread recognition of the importance of employment, economists continue to debate the most effective strategies to promote it.

Designing effective policies to foster employment is challenging without a deep understanding of firms' behavior. Firms ultimately determine hiring decisions and wages. It is, therefore, essential to analyze how firms determine their employment levels in light of their production functions and the broader economic environment (Hamermesh, 1993). In particular, the taxation of firms can have far-reaching consequences for the economy, as firms contribute to a significant share of national tax revenue (OECD, 2021). Whether taxes are levied on capital, labor, profit, or value-added can shape firms' decisions in distinct ways. Yet, most empirical studies have focused on corporate income or payroll taxes, leaving much to explore regarding alternative tax structures and their broader economic effects. Chapter 2 and 3 contribute to filling this gap by investigating the effects of different forms of business taxation. In these

chapters, I leverage fiscal policy variations to analyze how businesses respond, particularly in terms of employment decisions, thereby effectively "opening the black box" of firms.

In Chapter 2, I deepen our understanding of how taxes influence firms' behavior and offer empirical insights into input-based taxation, a topic frequently discussed in theoretical literature but underexplored empirically. To do so, I assess the impact of a major reform of the local business tax in France. This reform significantly reduced labor costs by shifting the tax burden from both labor and capital to capital alone. I use rich administrative sources, creating a comprehensive dataset for the study. This allows me to link workforce data with firms' capital stock and tax payments. I estimate 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 shifts employment patterns, with capital-intensive firms experiencing faster employment growth than labor-intensive firms. Liquidity-constrained firms primarily drive investment effects. Regarding employment, I identify two potential mechanisms explaining these differences in growth. Firms replace low-skilled workers with fewer high-skilled workers, generating lower employment growth. Another explanation is that firms might have different tax sensitivity. Since business tax payments can be deducted from the corporate tax base, firms that do not pay corporate taxes – because they are not profitable – bear the total cost of the business tax and are thus more responsive to the reform. My study supports the theoretical argument that taxing inputs like labor and capital distorts firm behavior and creates inefficiencies (Diamond and Mirrlees, 1971a).

In Chapter 3, with Emilie Sartre (University of Nottingham) and Sebastian Siegloch (University of Cologne), we examine the impact of France's local business tax when it was levied solely on capital (2003-2010). By leveraging detailed information on wages, occupations, hours worked, and capital stocks—including distinctions between long- and short-duration assets—we track firm-level responses to staggered business tax changes across municipalities. Using event study models, we estimate an elasticity of -2.62 with respect to the net-of-tax rate, reflecting the mechanical increase in tax payments. However, we find no evidence that firms adjust their capital stock, labor inputs, or wage structures in response to the tax hikes. This null effect

holds across various asset types and labor metrics, and persists even among capital-intensive firms and smaller firms facing a larger tax burden. These results suggest that, in the short to medium term, business tax increases do not lead to disinvestment or labor adjustments, highlighting the rigidity of firm input responses and the potential irreversibility of capital allocation decisions.

While firms' hiring decisions and responses to taxation are crucial determinants of employment, fostering job growth also requires considering the supply side of the labor market. Understanding how individuals respond to labor market policies is essential for designing effective interventions. In the final chapter of this dissertation, I shift focus to a specific set of policies addressing labor market frictions: Active Labor Market Policies (ALMPs). In Chapter 4, a collaboration with Valentina Melentyeva (Tilburg University) and Eloïse Menestrier (INED), we highlight that overlooking gender differences in policy evaluations can lead to inaccurate conclusions about a policy's effectiveness. We evaluate a subsidized employment policy for disadvantaged youth in France. The goal of the program was to assist those struggling with the transition from school to work, experiencing unemployment, and residing in deprived areas. We begin with a descriptive analysis of participants' outcomes, highlighting the importance of estimating the effects separately for cohorts (in terms of age at participation) and gender. Turning to the program's effect, we estimate that subsidized employment is more effective for men than women, with mothers benefiting the least. We identify two reasons for such discrepancies: young female participants are more likely to sort into lower-quality jobs, and are more likely to have a child during the program. Our results have strong policy implications for future policy evaluations and designs. First, if the objective is to provide quality jobs that allow for skill accumulation, gender heterogeneity in job types should be considered. Second, gender heterogeneity is an important feature of comprehensive policy evaluations, as average estimates might hide heterogeneous treatment effects and thus can lead to wrong policy conclusions.

From taxation to subsidized employment programs, my findings emphasize the need to account for heterogeneity across firms and individuals. By employing quasi-experimental methods and rich administrative data, my work provides policy-relevant insights that contribute to designing more effective labor and tax policies.

Contribution

Chapter 2 is my own work.

Chapter 3 is based on joint work with Emilie Sartre, Sebastian Siegloch, and Andreas Lichter. Andreas Lichter contributed to the initial stages of the project but is no longer involved. Emilie and I were responsible for preparing the data. All together, we developed the empirical strategy. I conducted the empirical analysis. We wrote the initial draft together, and I fully revised the latest draft included in this dissertation.

Chapter 4 is based on joint work with Valentina Meletnyeva and Eloïse Menestrier. The research idea was developed jointly. Data preparation was conducted by Eloïse Menestrier and me. We collaborated with Valentina Melentyeva to develop an empirical strategy and conducted the analysis. Together with Eloïse Menestrier, we carried out further heterogeneity analysis. The draft and its revisions were written in collaboration.

Chapter 2

Redesigning taxes to increase employment and economic activity? Evidence from a French Business tax reform

This chapter is based on Gharbi (2025).

2.1 Introduction

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

Providing systematic empirical tests of these theoretical predictions has been challenging for various reasons. First, inputs are usually taxed indirectly, for example, through corporate

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

This paper exploits a unique French business tax reform that changed its tax base, switching from a capital-labor tax base to a tax based solely on capital. The reform was discussed in late 1998 when the debate regarding the national budget started and then implemented in 1999. At that time, the negative impact of the tax on employment and investment was already acknowledged in political circles, and this reform attempted to alleviate these concerns. The reform generated tax reductions for all firms but of different magnitudes depending on the labor share in their pre-reform tax base: the more labor-intensive firms were, the more exposed to the reform. The tax accounted for around 30% of firms' overall tax burden and about 1.5% of firms' pre-reform turnover. The tax reduction induced by the reform was up to 20% for the most exposed.

To explore the impact of the reform on firm-level outcomes, I draw on unique administrative data that includes linked employer-employee microdata, as well as financial and tax records. By merging these data, I have information on the workforce, the capital stock, and the taxes paid for all private firms. I apply a dynamic differences-in-differences approach, taking advantage of firm-specific cost reductions proportionate to firms' labor share. I compare firms that are most exposed to the reform (those with a higher labor share) to firms that are relatively less exposed (those with a lower labor share). Since labor-intensive firms might differ significantly from capital-intensive ones, I refine the comparison by grouping firms into bins based on their labor intensity in 1998, the bins being the quintiles of the overall distribution. I estimate the effect of the reform within these bins. This allows me to have some variation in exposure

while comparing firms with relatively similar production functions. The identification strategy rests on the assumption that, within the same bin, firms with different labor shares would have followed the same trajectory in the absence of the reform. I test the plausibility of the assumption using the pre-reform periods and show that before the reform, firms evolved similarly and did not anticipate the change in the tax base.

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

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

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

Given that I observe no changes in wages or the number of hours worked and that one of the goals of the reform is to increase employment, I then turn to the employment responses of firms. I estimate a negative effect on employment, with an elasticity of 0.11. While this might seem surprising initially, given the existing literature, this negative effect indicates slower employment growth in – more exposed – labor-intensive firms. I identify two potential mechanisms explaining these differences in employment growth. The first explanation is that relatively more

labor-intensive firms replace low-skilled workers with fewer – more productive – high-skilled workers, generating lower employment growth. The estimates suggest that firms roughly replace three low-skilled workers with one high-skilled worker, leading to a net reduction in total employment. The second explanation is that firms might have different abilities to deduct business tax payments from the corporate tax base. Before the reform, non-profitable firms, which could not offset the tax due to their lack of corporate tax liability, bore the full cost of the business tax on labor, constraining their labor demand. After the reform, these firms benefit relatively more from the tax reduction, leading to stronger employment growth. Supporting this mechanism, I find that (i) capital-intensive firms, which increase employment the most, tend to be less profitable and therefore less likely to pay corporate taxes, and (ii) firms that did not pay corporate tax before the reform and thus could not deduct the tax on labor, exhibit the largest employment growth after the reform.

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

The contributions of this paper are threefold. First, studying the effects of this specific business tax (directly based on factors of production) allows me to test the theoretical conclusions of the taxation models empirically. The theoretical literature has long highlighted the distortions induced by taxing input factors (Harberger, 1962; Kotlikoff and Summers, 1987). In particular, the production efficiency results of Diamond and Mirrlees (1971a) and Diamond and Mirrlees (1971b) provide a strong case against using taxes on production inputs as part of optimal policies. This result is, however, derived under strong assumptions such as an economy with no profit – or significantly taxed. In this paper, I turn the question around and ask whether

removing taxes on input factors – here labor, improves firms' productivity. I provide empirical insights suggesting that taxing labor inputs directly is not desirable even in a non-perfectly competitive economy with profit.

Second, I contribute to the corporate tax literature. I provide new insight into firms' responses to taxation, opening the black box of firms to evaluate a different type of taxation. In addition, I demonstrate the importance of considering the overall tax liabilities and existing interaction between the different tax bases. Several papers underline the harmful effects of corporate taxation. Most studies highlight that a non-negligible share of the burden is borne by workers when the tax increases and wages fall (Suárez Serrato and Zidar, 2016; Fuest et al., 2018). Recent evidence suggests the effect is symmetric, following a tax cut wages increase, at least for manufacturers in Québec (Duan and Moon, 2024a). We also know that corporate tax changes affect employment (Giroud and Rauh, 2019; Kennedy et al., 2022), but also investment (Mukherjee et al., 2017; Link et al., 2024; Lichter et al., 2025). Most evidence rests, however, on the increase of taxes based on profits. Closer to my setting, are the targeted tax incentives aiming at reducing the cost of a particular input. This strand of the literature has been growing over the last few years. However, there is still limited evidence examining the effect of corporate tax incentives on labor and capital outcomes within the same firms and across various sectors. Existing papers connecting these two outcomes within the same firms have been limited to manufactures (Lerche, 2022; Duan and Moon, 2024b). The only exception is probably Harju et al. (2022), but it is limited to small firms. Other studies focusing on one outcome at a time have underlined that investment is highly sensitive to taxes (Zwick and Mahon, 2017; Ohrn, 2018; Maffini et al., 2019; Chen et al., 2023). Despite targeting specific capital and industries, these tax incentives can have spillover effects on the local labor market (Curtis et al., 2022; Lerche, 2022). Studies evaluating the effect of tax incentives targeting labor are even scarcer; to my knowledge, only Carbonnier et al. (2022) do so. In this paper, I use a substantial tax reduction based on labor share and provide evidence of its effect on labor and capital outcomes of firms across all industries.

Third, by removing the wage bill from the tax base, this paper contributes to the payroll tax literature, as the reform I study can be seen as an employer payroll tax cut without benefit

linkage. Existing studies on payroll taxes typically focus on rate changes or narrow adjustments to the tax base (e.g., targeting specific worker groups), which can distort the labor market and, therefore, provide limited insights into the effects of broader tax base changes (e.g., Saez et al., 2012). In contrast, I offer new evidence that applies universally, avoiding wage distortion and reducing the marginal cost of all workers. My results support recent studies showing that not only employment but also investment can be affected by a reduction in the taxation of labor and that effects are particularly large for firms initially cash-constrained (Saez et al., 2019; Benzarti and Harju, 2021a; Benzarti and Harju, 2021b). I also outline new mechanisms likely to be at work for the payroll taxes: the interaction of the different tax bases is likely to shape firms' responses.

The remaining of this paper is organized as follows: Section 2.2 provides information about the institutional background and describes the data sources; Section 2.3 describes the identification strategy; Section 2.4 presents the results; Section 2.5 discusses the potential mechanisms at work; Section 2.6 concludes.

2.2 Institutional Setting and Data

Section 2.2.1 first describes the institutional context and Section 2.2.2, the reform of interest. Section 2.2.3 details the data sources and the construction of the panel of estimation.

2.2.1 The French Business Tax from 1975 to 2010

Since 1975, a local business tax has been levied on establishments having a for-profit activity in France.[1] Three sub-national administrative divisions – region, county, and municipality – determine the final rate faced by plants.[2] 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 2.C. In my identification strategy, I do not leverage this source of variations (see Section 2.3 for more details).

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

[2] 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.

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[3] 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 2.1 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 2.1 suggests that the exposure to the reform is not the same across industries. For example, the construction sector is likely to be more affected than other activities, given the difference in the shares of the wage bill. However, as we will see in the next section, the 1-digit industry classification hides significant heterogeneity across firms.

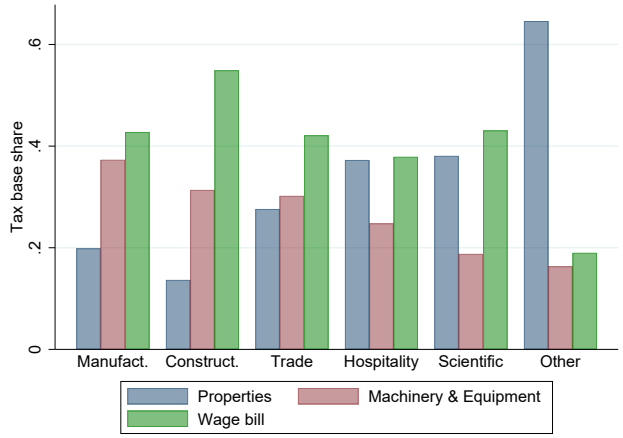


Figure 2.1: Average Shares of the Tax Base Components across Industries

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

The business tax is due whether firms are profitable or not. When firms are profitable, they must also pay the corporate income tax. In this case, the business tax payment can be deducted from the corporate tax base. As a result, the top-up induced by the business tax is

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

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, cannot offset its cost.

2.2.2 The 1999 Reform

As the local business tax was denounced as constraining economic activity, especially employment (Bayard and Balligand, 2019), the national government decided to remove the wage bill from the business tax base gradually. The reform was officially announced in the fall of 1998, when the budget for the upcoming year was voted on.[4] The reform took place over four years. Each year, a fixed amount is deducted from the taxable value of the wage bill. The tax allowance cannot exceed the initial taxable value. The yearly fixed allowance is granted at the municipal-by-firm level.[5] The yearly tax allowance spanning 1999 to 2002 is summarized in Table 2.1. The intention behind this phased allowance was to alleviate the burden on smaller businesses swiftly. For example, a firm with a taxable value of 50,000 € before the reform would end up with a taxable value of 31,477 € in 1999 and 0 € from 2000. A firm with a taxable value of 500,000 € before the reform would stop paying taxes on labor from 2002.

Table 2.1: 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

[4] 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.

[5] 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.

allowance was more likely to fully cover the 18% of the wage bill for these firms. Meanwhile, larger firms had to wait until 2003 before they were relieved of business taxes on the wage bill.

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

Importantly, the national government compensated municipalities for the change in tax collected due to the reform. More precisely, the difference between the amount of tax collected in a given year and what should have been collected had the reform not happened. As a result, the 1999 reform removing the wage bill from the tax base did not affect the resources of municipalities (PROJET DE LOI DE FINANCES, 1999; Bayard and Balligand, 2019). The tax rates were not affected directly by the reform; they are defined independently by municipalities. However, municipalities could adjust their rate during this period if they wanted to.

2.2.3 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.[6] Thanks to a unique plant identifier (*SIRET*), I can aggregate the panel at the plant-year level. I define the main workforce as the number of workers present in the firm at the end of the year.[7] The corresponding gross

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

[7]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).

wage bill is computed by summing the annual gross wage of these individuals in the main labor force. This is the wage bill used to construct the proxy of exposure to the policy. I then derive the median hourly gross wage of employees. To investigate the mechanisms, I identify minimum wage workers and sort the workforce according to their skill level. As occupational codes are not well-reported before 2002, I approximate skill levels using the wage distribution relative to the minimum hourly gross wage. High-skilled workers are defined as those earning an hourly gross wage greater than twice the minimum wage, while low-skilled workers are those earning below this threshold. Appendix Figure 2.B.1 plots the distribution of hourly wages relative to the minimum wage by occupations in 2002. Nearly all clerks and blue-collar workers fall below the two-times-minimum-wage threshold, whereas most high-skilled workers exceed it.

Balance sheet data. I use the FICUS source containing balance sheet and income statement data. This source is based on tax and social contribution statements. This dataset covers the universe of firms, except those in the financial and agricultural sectors. It provides detailed 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%.[8] While the labor force information is reported at the plant level, the financial data are only reported at the firm level. As a result, I will focus on single-plant firms.

Tax statement and local tax rates. I have access to the annual business tax statement for all establishments from 1999-2010. This database details the amount declared for each item of the tax base. However, as the business tax statement is available only starting from 1999 – the year of the reform, I do not base the measure of exposure to the reform on these data. I, however, use them to provide descriptive statistics and supporting evidence that my measure of exposure is a good proxy, see Section 2.3.

I retrieve fiscal information and especially the local business tax rates in the exhaustive administrative panel of French municipalities (*Recensement des Eléments d'Imposition* – REI).

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

The observations have been reported at the municipal level every year since the 90s. Each municipality is identified thanks to a unique identifier. This panel contains tax rates of all administrative divisions, the number of tax returns, the tax base, and the amount of collected taxes.

Baseline sample. I select a ten-year panel of establishments from the linked employer-employee data covering 1995-2006. This period restriction enables me to study the effect of the tax base reform in a window of four years before and six years after the reform. Limiting the sample to this period enables me to observe the reform's effect after the complete phase-out of the wage bill from the tax base. I restrict the sample to private and single-plant firms and exclude financial and agricultural sectors. These restrictions stem from the coverage and aggregation level of the financial statements. I verify that plants report positive values of tangible assets and at least three employees every period.[9] 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. Details concerning the construction of the panel of firms can be found in Appendix 2.D.

2.3 Empirical Strategy

This section presents the empirical strategy of the paper. Section 2.3.1 defines the proxy for the exposure to the reform and shows that this is a good one. Then, Section 2.3.2 specifies the estimation model.

2.3.1 Proxying the Exposure to the Reform

As emphasized in Section 2.2.2, 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

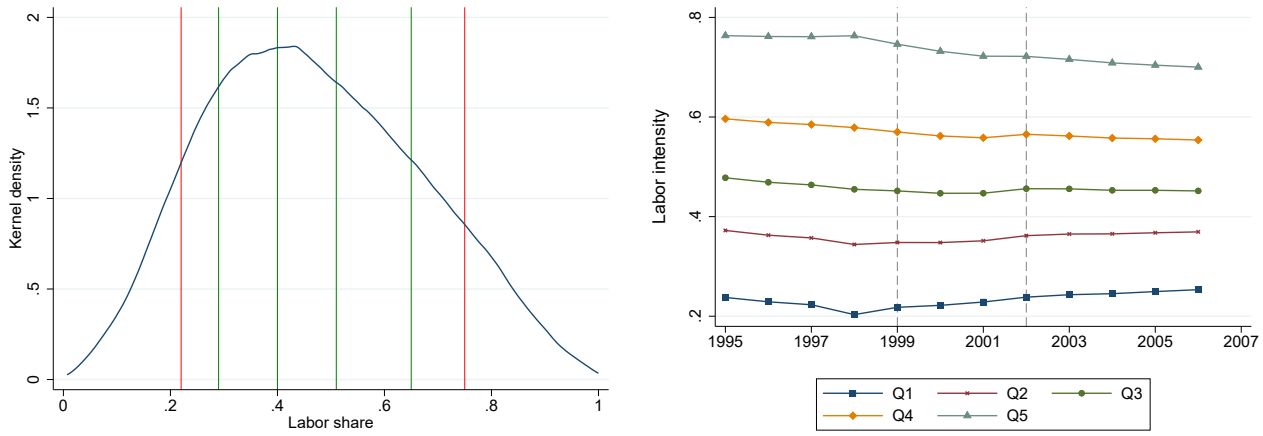
[9] A large number of firms are tiny (0, 1, or 2 employees). For those firms, discrete adjustment of workers would generate extreme employment growth values (e.g., 100 percent for firms growing from 1 to 2). Hence, I drop these tiny firms.

firms, the quicker they stop paying taxes on labor. The ideal proxy of exposure to the reform should accurately predict the tax reduction, provide sufficient variation, and, importantly, be persistent over time.

Therefore, I proxy the exposure to the reform by the labor intensity in 1998.[10] More precisely, I define labor intensity as the share of labor in the tax base in 1998:

$$LaborShare_i = \frac{W_{i,1998}}{W_{i,1998} + K_{i,1998}} \tag{2.1}$$

$W_{i,1998}$, $K_{i,1998}$ are the wage bill and the stock of tangible assets of firm i in 1998. This is a simplified proxy of exposure to the reform. Indeed, the wage bill and the stock of tangible assets of firm i in 1998 are the real values, not the taxable ones. However, this proxy provides sufficient variation (see Figure 2.2a), is persistent over time (see Figure 2.2b), and importantly predicts well the reduction in taxes induced by the reform (see Figure 2.3). I will refer to the ratio defined in equation (2.1) 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**

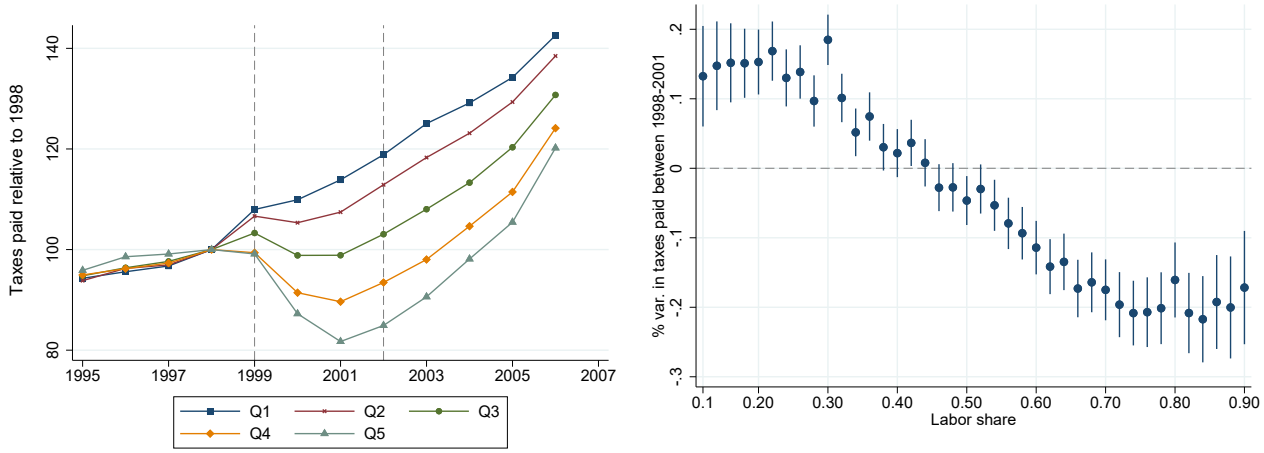
Figure 2.2: Evolution and Distribution of Labor Shares

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

In Figure 2.3a, I plot the average taxes paid over time by quintile, expecting that the more labor-intensive firms, the larger their tax reduction. While the taxes paid before the reform

[10] 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.

evolve similarly across quintiles, in 1999 – at the time of the reform – the taxes paid by firms decrease proportionally to their labor intensity. In the longer run, however, we observe that the taxes paid increase. This can be explained, as we will see in Section 2.4.1, by the fact that capital remained tax and firms invest significantly following the reform. As this variable contains other local tax payments – such as the property taxes, I show in Appendix Figure 2.B.5 that this pattern is indeed driven by a reduction in business tax payments using the business tax statements starting in 1999. In Figure 2.3b, I provide additional evidence that this negative correlation holds within quintiles. In Appendix Figure 2.B.4, I show that this negative correlation only exists in the post-reform period. In Section 2.4, 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 2.3: Relations between Labor Share and Taxes Paid

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

I further describe the sample of estimation in Appendix Table 2.A.1 and Table 2.A.3 across the five quintiles of the labor share distribution 1998. We can see that most labor-intensive firms (in Q5) tend to have more employees than the other firms; they have fewer tangible assets and pay relatively less taxes. Firms in the lower quintile of the distributions pay relatively lower wages. One might worry that this is driving their labor share down and that they are, therefore, defined as capital-intensive. However, the average amount of capital for one worker

is substantially larger for firms in Q1 than in Q5 (about 63 against 11 thousand euros). In addition, it is worth noting that the tax rate in 1998 does not vary much across quintiles nor the share of industries (except for the construction sector, see in Appendix Table 2.A.3). These suggest that estimations are unlikely to be driven by differences in tax rates or a particular industry. I carry out additional robustness checks along these lines in Section 2.4.3. It also is worth noting that within the quintiles, there is still sufficient variation in exposure to the policy.

Alternative proxies of exposure. There are different potential proxies: the tax rate alone, the labor share in the tax base (as defined in equation (2.1)), and the interaction of the former and latter. As highlighted earlier, the ideal proxy should accurately predict the tax reduction, provide sufficient variation, and, importantly, be persistent over time. From these three proxies, only the labor share in the tax base fulfills the three requirements. The business tax rate alone and the interaction of the tax rate and the labor share have limited variation, with standard deviations of 0.063 and 0.057, respectively. In contrast, the labor share has a standard deviation of 0.199 (see in Appendix Table 2.A.4). Second, the prediction power of these two is limited compared to the labor share: they hardly predict the tax change between 1998 and 2001 (see in Appendix Figure 2.B.2 and 2.B.3), while the labor share smoothly predicts it (see Figure 2.3). Table 2.A.5 in Appendix, further shows that the labor share explains more of the variance in the taxes paid than the alternative proxies.

2.3.2 Model Specification and Identification

Dynamic Differences-in-differences approach. My empirical approach leverages cross-sectional variation in firms' labor shares.[11] 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

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

1995-2006. Formally, the model is the following:

$$\log(y_{it}) = \sum_{k=1995}^{2006} \beta_k \times \mathbb{1}\{t = k\} \times LaborShare_i + \gamma_i + \delta_t \times \sum_{j=1}^{5} \mathbb{1}\{LaborShare_i \in Quintile_j\} + \epsilon_{it} \tag{2.2}$$

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

In the main specification, the bins correspond to the five quintiles of the distribution of labor intensity in 1998. The rationale behind including the interaction between year fixed effects and the set of dummies capturing firms' quintile rank in 1998 is that capital-intensive firms may not evolve like labor-intensive firms. So, by using these bins, I aim to bring more comparable firms together. As a result, I estimate the effect of the reform within these bins. The definition of these bins is arbitrary; therefore, I verify that the choice of bins does not drive my results, see Section 2.4.3.

It is worth noting that in the regression sample, I exclude firms having a labor share in the bottom and top 10^{th} percentiles. Indeed, these firms are very peculiar and can be considered outliers (see Appendix Table 2.A.6).

Identification. The key identification assumption in this differences-in-differences type of regression is that firms with lower labor intensity are a valid estimate of the counterfactual for relatively more labor-intensive firms within a given bin. So, conditional on being within the same bin, relatively less labor-intensive firms and relatively more labor-intensive ones would

follow a parallel trend in the absence of the 1999 reform. While this assumption cannot be tested directly, I test whether the parallel trends assumption holds in the pre-reform period. Reassuringly, I cannot reject the presence of differential trends in most of the specifications.

Another concern for identification is potential confounding shocks that overlap the period of interest here: the reduction in hours worked and the development of broadband internet. A significant share of the firms in the study sample have less than 20 employees, implying that they were forced to reduce the number of hours worked only in 2002. I perform additional robustness checks in Section 2.4.3. Regarding internet development, I also verify that my estimates are robust when including the municipality-by-year fixed effects or Labor-market-by-year fixed effects. However, labor intensity is likely to be orthogonal to the development of the Internet. Additionally, I also show that changes in the business tax rate do not affect my point estimates. The complete set of robustness tests can be found in Section 2.4.3.

Continuous treatment effects. Recent work of de Chaisemartin and d'Haultfœuille (2018), de Chaisemartin and d'Haultfoeuille (2020), and Callaway et al. (2024) have highlighted that differences-in-differences estimates with continuous treatment could be biased. In my setting, I have continuous treatment along the labor intensity of firms. This implies that I need to rely on the continuous dose assumption. This assumption implies that the treatment effect is linear. Increasing the labor share by 0.1 has the same effect regardless of whether the initial labor share is 0.02 or 0.4. However, if this assumption does not hold in my setting, it is possible that my point estimates are biased.

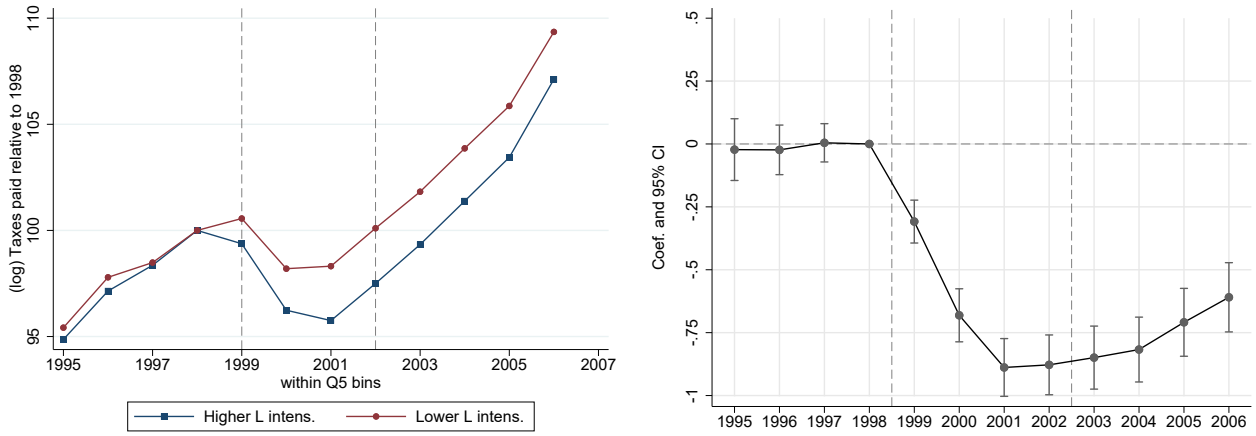
2.4 Results

Section 2.4.1 shows that the labor share proxy explains the tax reduction induced by the reform and reviews the effects on labor, capital and productivity. Section 2.4.2 then investigates potential heterogeneity in the sample. Section 2.4.3 reports several robustness checks.

2.4.1 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 2.4a, I first report averages (relative to 1998) and distinguish two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. I observe that relatively more labor-intensive firms seem to see their taxes reduced more following the reform. I find a similar pattern using the tax payment limited to the business tax; see Appendix Figure 2.B.6.

In Figure 2.4b, I plot the coefficients β_k resulting from the estimation of the model (2.2). 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 the coefficients are close to 0 and not significantly different from it. Using the event study approach confirms what I observed in the previous figure. Before the reform, taxes paid by firms evolved similarly. After the reform, relatively more labor-intensive firms benefit from a significant tax reduction. I can observe the gradual phasing out of the wage bill from the tax base between the two vertical lines. The tax reduction is limited in 1999, when the tax allowance only amounted to 100,000 Francs, and then there is a more significant drop in 2002 when the allowance equals 6,000,000 Francs. After 2002, we observe that the tax reduction gradually diminishes over time, likely because firms increased their investments following the reform, thereby expanding the tax base of the post-1999 local business tax. Differences-in-differences estimates (aggregating pre- and post-periods) suggest that a one percentage point increase in exposure to the reform leads to a reduction of 0.71%, this equivalent of a 305 euros additional tax reduction of (see Appendix Table 2.A.7). It is important to note that the taxes paid do not encompass the corporate tax but only the local taxes, with the business tax accounting for the largest share.



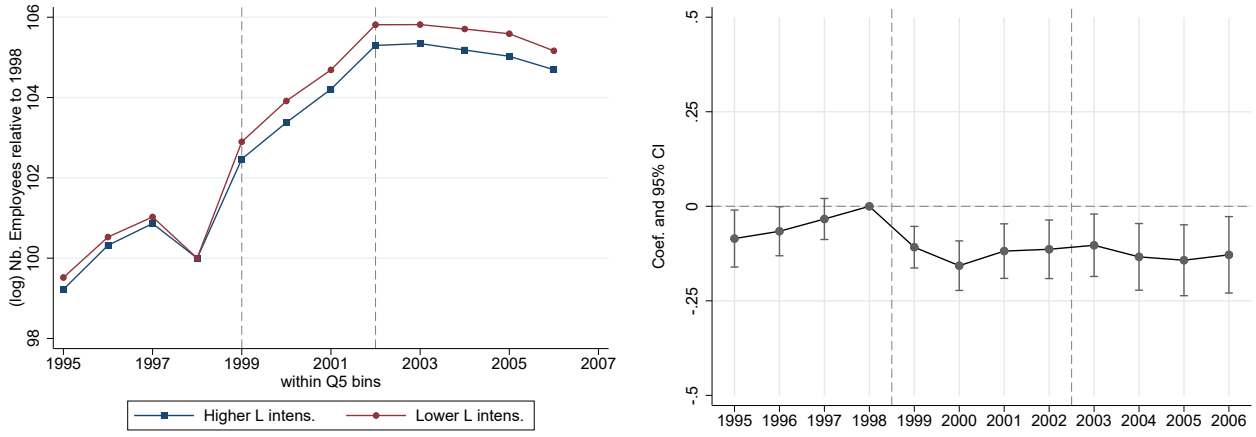
(a) Averages over Time **(b) Event Study Estimates**

Figure 2.4: Effects on Local Taxes Paid

Note: Panel (a) plots the average (log) taxes paid overtime relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 2.3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). 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 2.5a, I observe that firms that were most exposed to the reform did not increase employment as much as their counterfactual. In Figure 2.5b, I plot the coefficients resulting from the dynamic differences-in-differences model. Evaluating the pre-period coefficients (1995-1998), I note that the pre-trend is slightly increasing, and the coefficients are marginally significant in 1995 and 1996. However, when testing the joint significance of the pre-treatment estimates, I find a p-value equal to 0.147, confirming the absence of a significant pre-trend. Coefficients in the post-period are significantly negative. These results might be slightly downward biased, given the pre-trend. However, this is unlikely to account for the overall post-reform effects. Differences-in-differences estimates suggest that a one percentage point increase in exposure to the reform leads to a decrease in employment of 0.07% (see Appendix Table 2.A.7). At first glance, this suggests that the reform did not stimulate employment. However, the negative effect on employment could be due to two elements: (1) relatively high labor-intensive firms decreasing employment, or (2) the relatively high labor-intensive firms increase employment at a lower pace. Figure 2.5b suggests that the second explanation is the most plausible. Relatively more labor-intensive firms do not cut

employment *per se* but grow at a slower pace. For relatively less labor-intensive, employment rose by about 6% while for relatively more labor-intensive, it grew by only 5.5% between 1998 and 2002. Section 2.5.2 discusses potential explanations for these differences in growth.



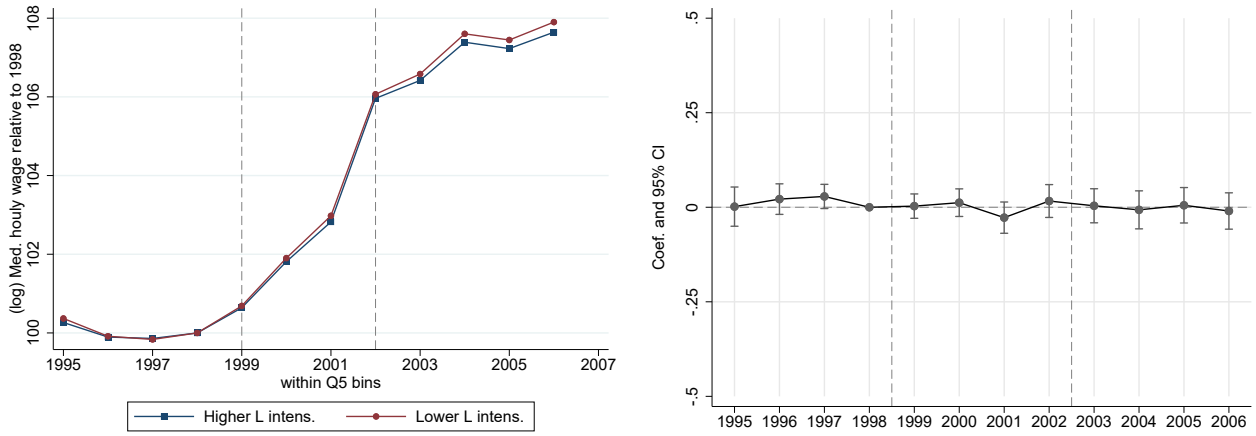
(a) Averages over Time **(b) Event Study Estimates**

Figure 2.5: 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 2.3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). 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 2.6a), I do not find a significant effect in the event study (see Figure 2.6b). The reform did not prompt firms to increase their salaries.

I also investigate whether hours worked per worker vary after the reform (see Figure 2.7). As labor becomes relatively cheaper, firms could select a group of employees and make them work additional hours. I do not find a significant effect. The coefficient in 2003 is marginally significant and negative. However, the pooled estimate is insignificant (see Appendix Table 2.A.8). The absence of wage or hours adjustment is not very surprising given the context of France; wages are known to be rigid, and hours worked are relatively well regulated. However, it is essential to note the absence of a response in hours worked per worker supports the identification strategy as it suggests that the labor share in 1998 does not predict the reduction in working time implemented between 2000 and 2002. This is, however, not sufficient to claim



(a) Averages over Time **(b) Event Study Estimates**

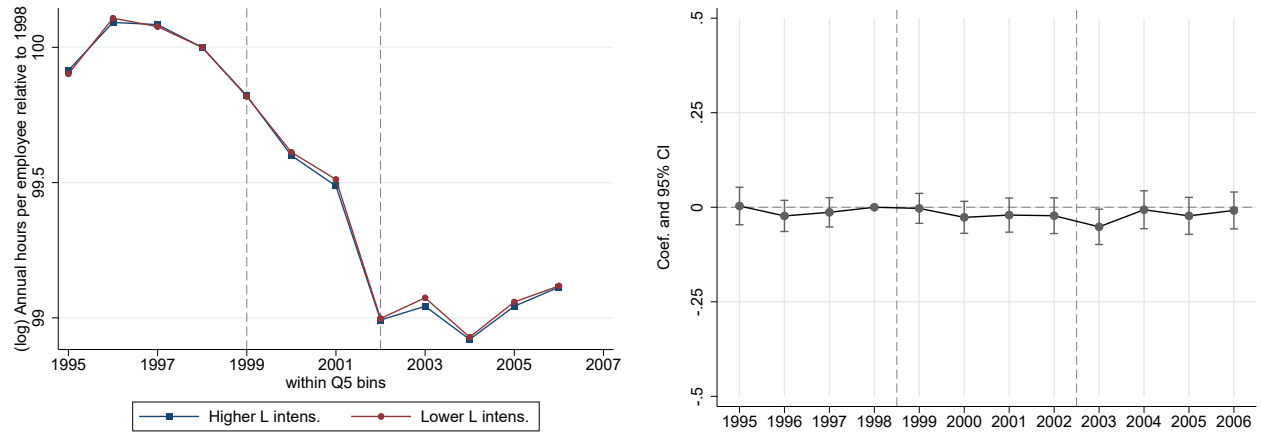
Figure 2.6: Effects on Median Hourly Wages

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 2.3. 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 (2.2). 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.

that this reform is not affecting my point estimates; I, therefore, carry out additional tests in Section 2.4.3.

Effects on capital. Although the tax reform targets only labor and capital remains taxed after the reform, firms can still use this tax windfall to invest. I, therefore, investigate the effect on the stock of capital defined by the sum of tangible assets and investment in tangible assets.[12] As one can see in Figure 2.8, the stock of capital of relatively more and relatively less labor-intensive firms evolve similarly in the pre-reform periods. This translates into coefficients close to 0, not significantly different from 0 in the period 1995-1998 in event study estimates. When the reform unfolds, the stock of capital significantly increases. Differences-in-differences estimates suggest that a one percentage point increase in exposure to the reform leads to an increase in capital of 0.3% (see Appendix Table 2.A.7). More precisely, for an additional percentage point in exposure to the reform, firms invest an additional 2,060 euros. Or, expressed in terms of tax reduction, for a one percent reduction in taxes paid, the investment increases by

[12]In Appendix Figure 2.B.7, I test the robustness of my results with alternative outcomes definitions.



(a) Averages over Time **(b) Event Study Estimates**

Figure 2.7: Effects on Annual Hours Worked per Employee

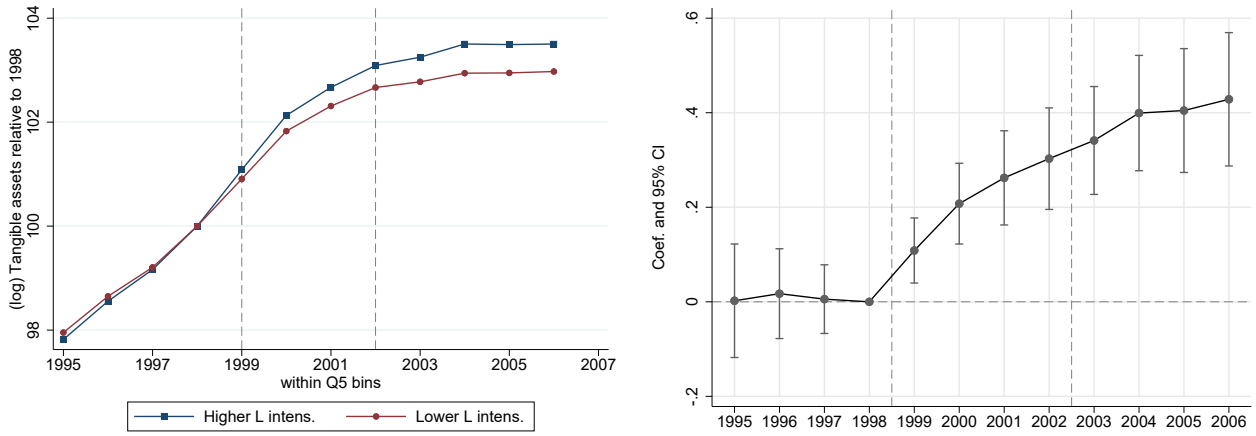
Note: Panel (a) plots the average (log) number of annual hours worked per employee overtime relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 2.3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). 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.

0.43%.[13] This effect is comparable to the investment responses measured by Saez et al. (2019), who find an elasticity close to 0.49.[14] My estimates are larger than that of Duan and Moon (2024b); 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).[15] However, their contexts are quite different as the policy targets capital directly and forces firms to invest to benefit from the tax reduction.

[13] I first compute the elasticity of capital to labor intensity based on the Differences-in-differences estimates, see in Appendix Table 2.A.9, and compute the elasticity of capital to the taxes paid (ξ_τ^K) defined by $\xi_\tau^K = \frac{\xi_K^{Lsh}}{\xi_\tau^{Lsh}}$: the ratio of the elasticity of capital to the labor intensity (ξ_K^{Lsh}) over the elasticity of taxes paid to the labor intensity (ξ_τ^{Lsh}).

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

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

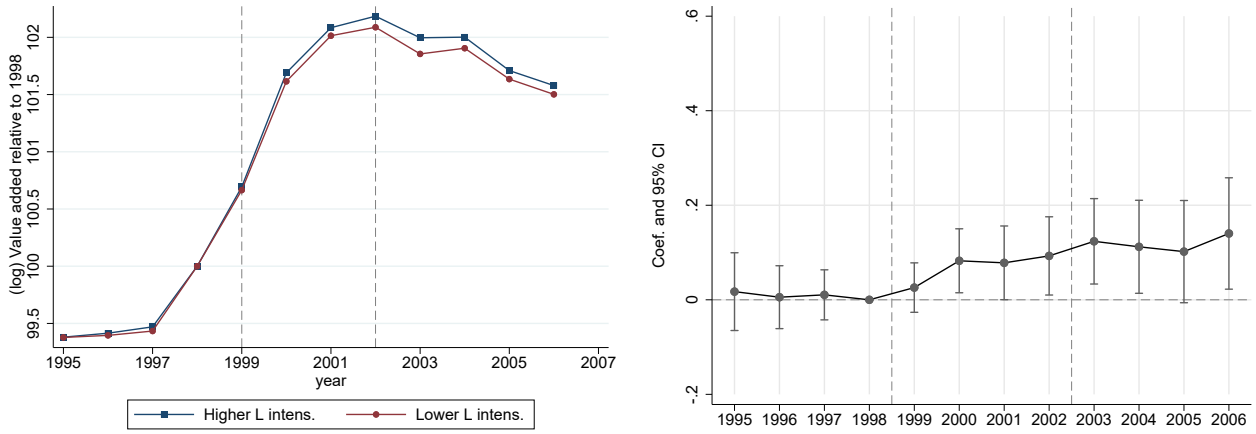


(a) Averages over Time **(b) Event Study Estimates**

Figure 2.8: 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 2.3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). The dependent variable is the log of tangible assets. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

Effects on productivity. I investigate whether the reform affects the productivity of firms. To do so, I estimate the effect of the reform on the log of value-added; see Figure 2.9b. I observe a flat pre-trend and a significant increase in the value-added produced in the post-reform period. Productivity increases significantly after the reform. However, differences-in-differences estimates suggest that a one percentage point increase in exposure to the reform leads to an increase in productivity per employee of 0.08% (see Appendix Table 2.A.8). Post-reform coefficients evolve similarly to the coefficients of the regression for the tangible assets; this suggests that the productivity boost is likely the result of the strong investments. I also scaled the effect on value-added, estimating the impact of the reform on the value added produced divided by the number of employees. Figure 2.B.8b in the Appendix displays the results and suggests an increase in labor productivity.



(a) Averages over Time **(b) Event Study Estimates**

Figure 2.9: 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 2.3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). 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.

2.4.2 Heterogeneity

Effects by quintiles. Table 2.2 reports point estimates of the Differences-in-Differences model interacted with the five quintiles of the labor share distribution in 1998. Focusing on the effect on taxes in Table 2.2 column (1), I show that all quintiles of the distribution benefited from a significant tax reduction. Effects are approximately constant across quintiles. In column (2), I report the effect on employment: the negative effect is driven by the lower part of the labor share distribution (Q1 and Q2), the firms relatively more capital intensive than the rest of the sample. The effect on tangible assets, on the other hand, is held across the overall distribution, except quintile 1, but still has a positive coefficient. The effect on the value-added is less clear, with point estimates only significant for quintiles 3 and 4. However, when scaling the values added produced by the number of workers, I obtain a positive and significant effect for most quintiles. This latter outcome is reported in Appendix Table 2.A.10 together with additional outcomes.

Table 2.2: Differences-in-Differences Estimates by Quintiles of Labor Intensity

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

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

Effects across industries. Table 2.3 reports point estimates of the Differences-in-Differences model interacted with the industry for the main industries; the other industries can be found in Appendix Table 2.A.11. Focusing on the effect on taxes in Table 2.3 column (1), I estimate that all industries benefited from a significant tax reduction. In column (2), I report the effect on employment. The construction and trade sectors have a negative effect. This suggests that apart from the labor intensity, the industry also matters for the response in terms of employment. The construction and trade sectors are not more represented in the lower quintiles of the labor share distribution (see Table 2.A.1); therefore, it seems to be another source of heterogeneity. In column (3), we note an increase in investment across all sectors, with a more extensive response for the Hospitality sector. The last column of the Table suggests that the productivity effect is driven by Manufacturers. However, when measuring the value added per worker, I estimate significant and positive effects across all industries. In Appendix Table 2.A.12 column (1), I find a negative effect that is small but statistically significant on the median hourly wage. This effect could result from a change in wages or the composition of the workforce, as firms with the largest reduction in median hourly wage also change employment the most.

Table 2.3: Differences-in-Differences Estimates by Industry

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

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

Effects by firms' size. Table 2.4 reports point estimates of the Differences-in-Differences model interacted with the firms' size in 1998; additional outcomes can be found in Appendix Table 2.A.13. Focusing on the effect on taxes in Table 2.4 column (1), I show that no matter the size of the firms, they all benefit from a tax reduction, although the reduction is more substantial for smaller firms. This is not surprising, given that smaller firms stopped paying taxes on labor earlier than larger ones. In column (2), I report the effect on employment. There, I observe that the negative effect is driven by the smaller and the largest firms. In column (3), I estimate an increase in investment across all firms. Smaller firms drive the results with an effect on tangible assets twice as large as the effect for the largest firms. As smaller firms tend to be more cash-constrained than larger ones, these differences could be explained by a difference in initial liquidity. This potential mechanism is further discussed in the next section. Last, in column (4), the results highlight an increase in overall productivity in all firms except the largest one. The labor productivity, however, increases for all firms (see Appendix Table 2.A.12).

Table 2.4: Differences-in-Differences Estimates by Size in 1998

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

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

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

2.4.3 Robustness

Bins. As underlined in the previous section, I verify that my point estimates are not the results of bin choices. In Appendix Figure 2.B.9, I report point estimates with different bins: bins using the four quartiles, the eight percentiles, the ten percentiles of the distribution, and no bins. It is important to note here that bins are required to bring together firms with pre-reform similar evolution, particularly in employment. Post-reform estimates with different bins and without bins are in the same ballpark. Only standard errors are affected; the smaller the bins, the larger the standard errors. This is not surprising as variation within smaller bins and the number of observations are mechanically reduced, inducing a loss in precision. Overall, these suggest that the choice of bins does not drive my results but helps only to bring more

comparable firms together.

Measure of labor share. In the main results, I use the labor share reported in 1998. The advantage is that this is as close as possible to the actual exposure to the reform. However, as this is a value at one point in time, it is more prone to mean reversion and potentially more sensitive. I, therefore, test the robustness of my results by changing the period I use to compute the labor share. In Appendix Figure 2.B.11, I report point estimates for all outcomes using the labor share calculated in 1997 and the average labor share in the pre-reform period (1995-1998). The point estimates of employment and taxes paid are hardly affected by a change in the computation of labor share (Appendix Figure 2.B.11a and Figure 2.B.11b, respectively). Regarding tangible assets, pre-reform period coefficients significantly differ from 0 for the two alternative measures. However, we still observe a clear trend break in the pre- vs. post-period, and the post-reform point estimates have similar values to the baseline estimations (Appendix Figure 2.B.11e).

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

et al. (2021) highlights that although the development had started in 1999, the development of broadband internet across France was slow until 2002; only 2% of the cities were covered in 2000. Third, municipality-by-year fixed effects control for a change in the tax rate and, therefore, support that the estimations are not the result of tax rate differences across firms.

Reduction in working time. In 2000, a mandatory reduction in working time was enforced, although it followed a voluntary transition period. From 1998 to 2000, firms were incentivized with subsidies to voluntarily reduce full-time working hours from 39 to 35 per week. In 2000, the change became mandatory for larger firms, and the subsidies ended. For smaller firms (with fewer than 20 employees), the switch to 35 hours became mandatory in 2002. The reduction in working time was uniformly enforced by law throughout France. It was applied differently according to industries and depending on negotiations with the various branch agreements. As this was a major reform in the French labor market, I carry out several tests to ensure that my point estimates are not confounded by the reduction in working hours. Two key points are worth highlighting. First, the reduction in working time reform could undermine my identification strategy if the labor share correlates with the timing of the reduction in working time. I do not observe differences in evolution between relatively low and high labor share firms within bins (see Figure 2.7a). Second, in the previous section, I found no significant effect on hours worked per worker in the baseline results (Figure 2.7b).[16] This is already reassuring as it suggests that the labor share is not a good proxy for the exposure to the reduction in working time.

To further address concerns that the reduction in working hours could be a confounding factor, I conduct several tests. I add various control variables that should, at least partially, account for the reduction in working hours in case my results were affected. I control for firms' size in 1998 (Appendix Figure 2.B.13), the hours worked in the previous year (expressed in hours per year or week), and the year of plausible transition.[17] The results remain consistent with the baseline across all outcomes (Appendix Figure 2.B.14). Additionally, I restrict the sample

[16] 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 2.A.8).

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

to firms with fewer than 20 employees, as these firms were required to switch to the 35-hour workweek in 2002. If the reduction in working hours impacted my baseline results, we would expect different results when focusing on smaller firms. However, as shown in Appendix Figure 2.B.15, the estimates remain consistent with the baseline. These robustness checks alleviate some concerns about the reduction in working hours as a potential confounder. Still, they do not definitively prove that the change in working hours is orthogonal to the 1999 reform. To my knowledge, there is no consensus on how the reduction in working time affected employment (Chemin and Wasmer, 2009).

Further robustness checks. I report additional sensitivity checks to ensure that my results are not driven by modeling assumptions. First, as Differences-in-differences models often provide biased standard errors (Bertrand et al., 2004), I verify the robustness of my results by clustering the standard errors at higher levels than the firm level (as suggested by Angrist and Pischke, 2009). In Appendix Figure 2.B.16, I show that the standard errors are hardly affected by a change in the clustering level (at the municipal or the labor market level). Second, I also change the sensitivity of the estimation to the window of the event study, and I find similar effects (Appendix Figure 2.B.17). In

2.5 Potential Mechanisms

Section 2.5.1 highlights that the investment effect is driven by firms that were initially cash-constrained. Section 2.5.2 details potential explanations for the negative effect on employment.

2.5.1 The Role of Credit Constraints

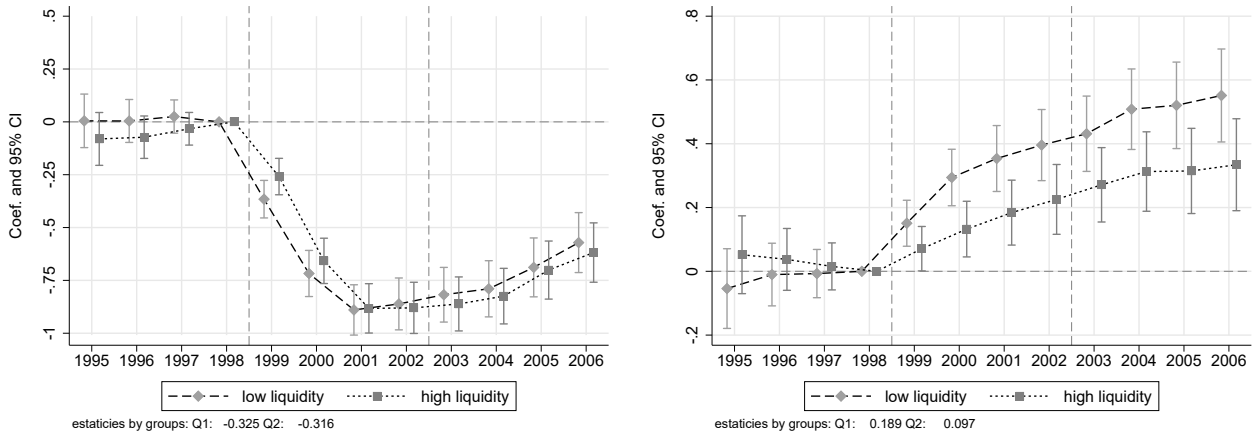
So far, I have demonstrated that following the change in the tax base in 1999, firms benefited from a significant tax windfall and used it to invest at the expense of employees substantially. This generates a significant productivity improvement. Given that the investment generated gains in productivity,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

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

Modigliani and Miller (1958) predict that if external financing is more costly than internal financing, cash injections should have a positive effect on capital expenditures.[18] 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; Benzarti and Harju, 2021b). 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.[19] I still compare firms within quintiles of labor share and further divide each quintile into constrained and unconstrained groups for comparison.

Figure 2.10 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 2.10a, we see that whether a firm is initially financially constrained hardly affects the tax reduction it faces. Both cash-constrained and unconstrained firms benefit from a significant and similar tax windfall. This mitigates concerns that the heterogeneous effects of financial constraints could result from a difference in the exposure and tax windfall between relatively high and less labor intensive, compared to unconstrained firms. I further report averages of labor shares by financially constrained status and show that differences in labor shares between relatively high and less labor-intensive are similar within quintiles (see Appendix Table 2.A.14). In Figure 2.10b, on the other hand, we observe that cash-constrained firms invest significantly more after the reform; the effects are twice as large for the cash-constrained firms. Therefore, these results are consistent with the credit constraint channel. This also translates into higher productivity gains for financially constrained firms (see Appendix Table 2.A.15).[20] However, I do not find significant differences in the effect on employment.

[18] External financing could be costly because of asymmetric information or incomplete contracting.
[19] I use the value of circulating assets divided by the total assets net of depreciation.
[20] Event study estimates for all outcomes can be found in Appendix Figure 2.B.18.



(a) Effects on Taxes Paid **(b) Effects on Tangible Assets**

Figure 2.10: Effects by Liquidity Constraint

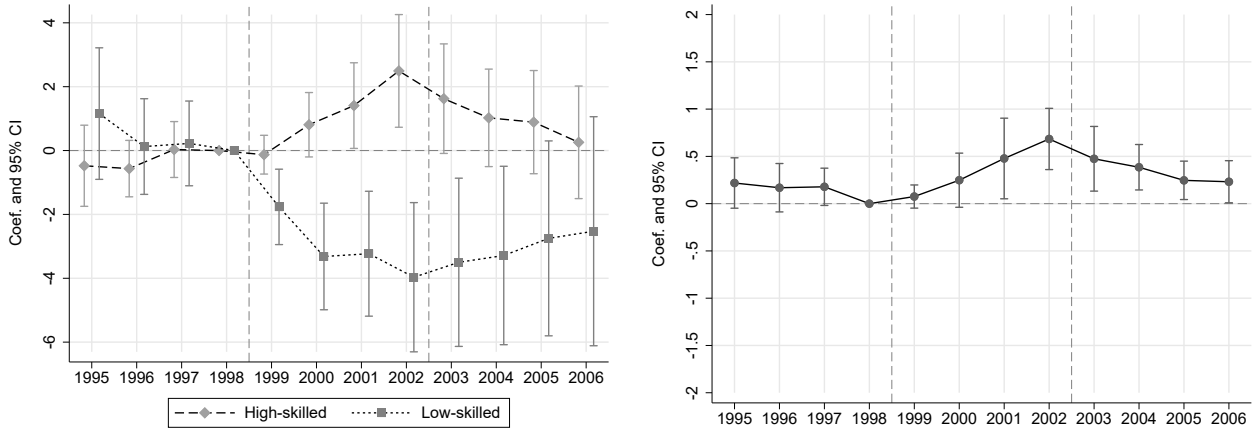
Note: Panel (a) and (b) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) fully interacted with a dummy indicating whether the liquidity of firms is below or above the sample's median. The dependent variable is the log of taxes paid in panel (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.

2.5.2 Explaining the Negative Effect on Employment

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

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

and low-skilled.[21] For these outcomes, I use the raw numbers rather than log transformations, as some firms may have no workers in one of these categories. Figure 2.11a shows that before the reform, the number of high- and low-skilled workers evolved similarly across firms. Post-reform, firms relatively more exposed to the reform decrease the number of low-skilled employees significantly. In contrast, the number of highly skilled workers increases only significantly around 2001, when the tax allowance becomes more substantial (see Table 2.1). Additionally, Figure 2.11b illustrates that the skill ratio – defined as the number of high-skilled workers relative to low-skilled workers at the firm level – increases post-reform, confirming that this substitution mechanism occurs within firms. I estimate that for a one percentage point increase in exposure to the reform, the number of high-skilled workers increases by 0.01, while that of low-skilled workers shrinks by about 0.03 (see Appendix Table 2.A.17). This roughly indicates that firms replace about three low-skilled with one high-skilled employee, explaining the overall decline in the total workforce.[22]



(a) Effects on Workers by Skill Groups **(b) Effects on the Skill Ratio**

Figure 2.11: Effects by Skill Level

Note: Panel (a) and (b) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). The dependent variable is the number of high-skilled and low-skilled workers, respectively, in panel (a), and the skill ratio in panel (b) is defined as the number of high-skilled workers relative to low-skilled workers at the firm level. I define high-skilled workers as those who earn more than two times the minimum hourly wages and low-skilled workers as those under (as detailed in Section 2.2.3). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

[21] I define high-skilled workers as those who earn more than two times the minimum hourly wages and low-skilled workers as those under (as detailed in Section 2.2.3).

[22] To obtain such numbers, I use the DiD estimates of Appendix Table 2.A.17, compute the related elasticities, and compute the absolute variation based on the pre-reform averages.

Overall, the evidence suggests that firms leverage the reform to improve their production technologies, increasing investment and shifting their labor force toward higher-skilled workers likely to complement the new technology. This aligns with recent findings on the effects of tax incentives targeting capital (Tuzel and Zhang, 2021; Duan and Moon, 2024a) and further highlights that tax reforms can accelerate skill-biased technological change, even when the tax reduction does not directly target capital. These results might contribute to our understanding of the mechanisms fostering skill-biased technological change (Acemoglu, 2011), a phenomenon that often exacerbates skill-based inequalities. Additional work is needed to investigate the composition of capital firms invested in.

Differences in tax sensitivity. A potential explanation for this difference in employment growth is the possibility of deducting the business tax payment from the corporate income tax depending on firms' profitability. Profitable firms pay corporate tax and can, therefore, deduct the tax payment related to the local business tax from the corporate tax base. The larger the business taxes paid, the lower the corporate tax payment. On the other hand, unprofitable firms do not pay corporate taxes – as these taxes are based on profits – and fully bear the burden of the local business tax. In a sense, firms not paying corporate taxes should be more sensitive to changes in business taxes, as they will fully incur the cost of it. To show this mechanism, I derive a simple model in Appendix 2.E. This exercise tells us that when facing a tax on labor, firms will reduce labor and capital. However, the more the firm can deduct the wage bill tax from the corporate tax, the less sensitive the firm will be to the tax on the wage bill. This is because the deduction in the corporate tax partially offsets the cost of the wage bill tax. Transferring the conclusions of this model to my empirical setting is straightforward and generates the following predictions: removing the wage bill from the business tax base should increase employment and capital, and, interestingly, firms that can deduct the business tax from their corporate tax should react less to the reform as the change in their production cost will be smaller.

Going back to the data, I plot in Figure 2.12 the average number of employees over time, distinguishing two groups: the profitable firms paying the corporate tax in 1998 and those that

do not make profits and, therefore, do not pay corporate taxes. While firms have relatively similar evolutions in the pre-reform period, we note that following the reform, the employment growth of firms that are not profitable is larger than that of those that are profitable.[23] This suggests that the interaction between the business tax liabilities and its deductibility in the corporate tax base is a plausible mechanism explaining the growth difference in employment between relatively high and low labor-intensive firms.

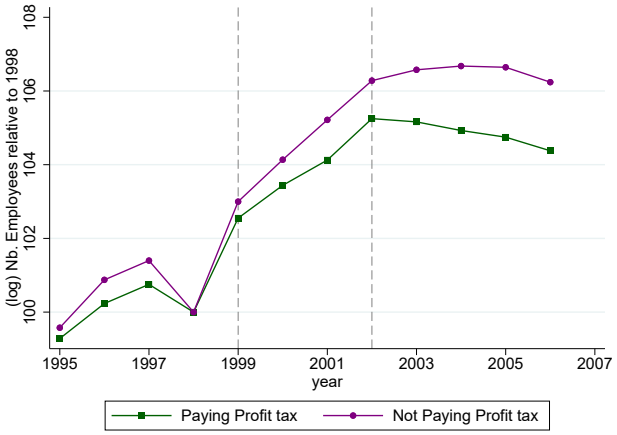


Figure 2.12: Employment Effect by Corporate Tax Liabilities – Averages over time

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

The heterogeneity analysis has shown that the negative employment effects are driven by (i) capital-intensive firms in the first two quintiles of the labor intensity distribution (Table 2.2), (ii) firms in the construction and trade sectors (Table 2.3), and (iii) firms with fewer than ten or more than 50 employees (Table 2.4). Appendix Table 2.A.16 shows that firms not paying corporate tax in 1998 are more likely to belong to these groups, as they are overrepresented in the construction, trade, and hospitality sectors, have fewer than ten employees, and fall within the first two quartiles of the labor share distribution. Additionally, these firms are concentrated in the lower range of the within-bin labor share distribution. This could explain the strong negative effects observed for these groups and supports the model's insight that firms unable to deduct corporate taxes are more responsive to the reform.

[23] Averages by quintiles are displayed in Appendix Figure 2.B.19, we note that the differences in pre-trends come mainly from the 5th quintiles; firms have otherwise similar paths in the pre-reform period. The first and second quintiles particularly drive the post-reform effects.

2.6 Conclusion

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

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

2.A Additional Tables

Table 2.A.1: Characteristics of Firms in 1998 across Quintiles

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

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

Table 2.A.2: Additional Characteristics of Firms in 1998 across Quintiles

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

Note: This table reports 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 2.A.3: Characteristics of the Workforce in 1998 across Quintiles

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

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

Table 2.A.4: Descriptive Statistics of the Different Measures of the Exposure to the Reform

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

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

Table 2.A.5: Contribution of the Different Measures of the Exposure to the Reform to the Variation in the Taxes Paid

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

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

Table 2.A.6: Characteristics of Firms in the Sample of Estimation compared to the Tails of the Labor Share Distribution

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

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

Table 2.A.7: Differences-in-Differences Estimates

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

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

Table 2.A.8: Differences-in-Differences Estimates - additional outcomes

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

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

Table 2.A.9: Elasticity to the Labor Intensity

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

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

Table 2.A.10: Differences-in-Differences estimates by Quintiles of Labor Intensity

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

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

Table 2.A.11: Differences-in-Differences Estimates by Industry

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

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

Table 2.A.12: Differences-in-Differences Estimates by Industry

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

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

Table 2.A.13: Differences-in-Differences Estimates by Size in 1998

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

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

Table 2.A.14: Labor share by Financial Constraint

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

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

Table 2.A.15: 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.

Table 2.A.16: Characteristics of Firms by Profitability in 1998

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

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

Table 2.A.17: Differences-in-Differences Estimates by Skill Groups

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

Note: This table reports the estimated coefficients of a difference-in-differences model. The standard errors are displayed in parentheses and are clustered at the firm level. Coefficients measure the effect of an increase in Labor share measured in 1998 by 1. The dependent variable is the number of low-skilled workers (1), the number of low-skilled workers (2), and the ratio of high-skilled workers to low-skilled (3). Low-skilled workers are defined as workers earning less than two times the minimum wage and high-skilled more than two times (see Section 2.2.3 for more detail). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

2.B Additional Figures

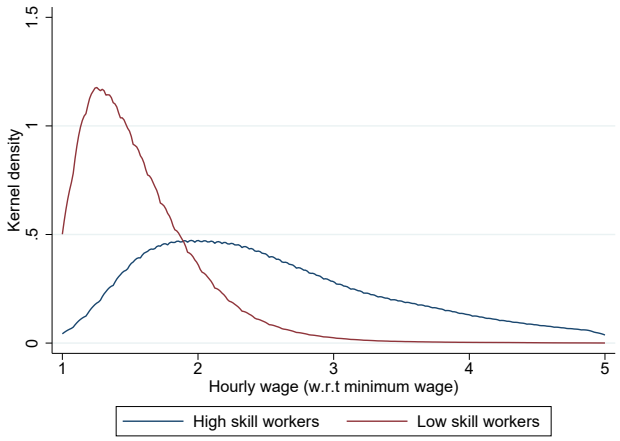
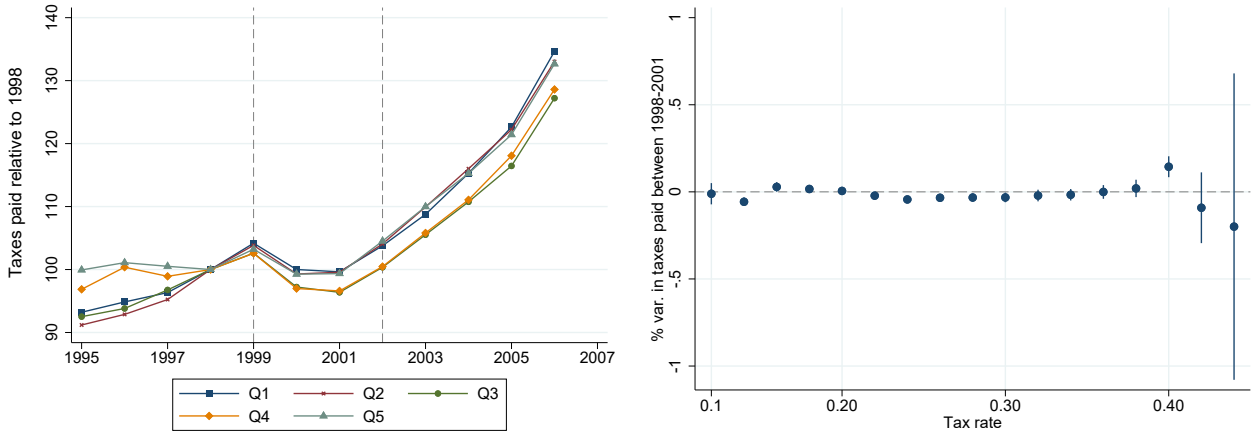


Figure 2.B.1: Distribution of Wages by Occupations

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

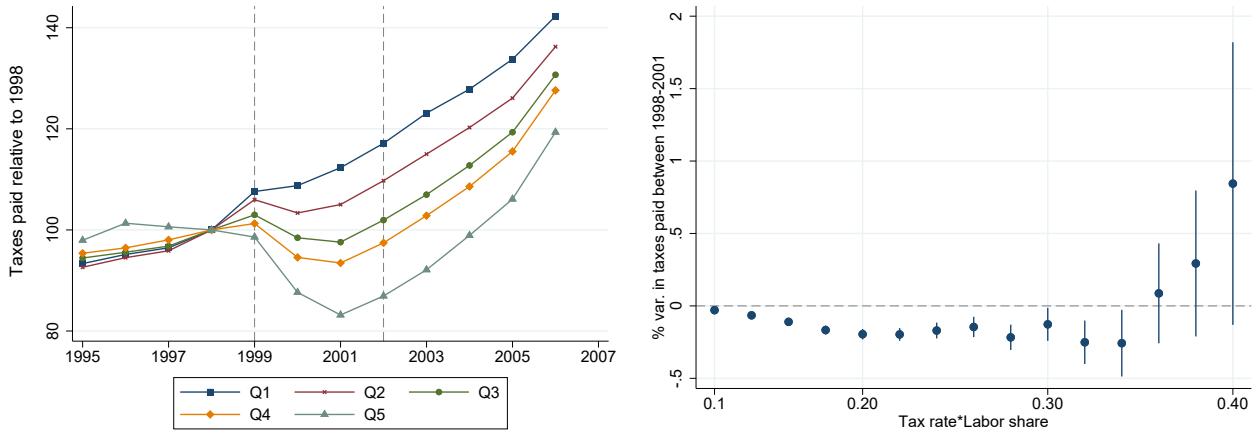


(a) Evolution of Taxes Paid by Quintiles of the Tax Rate in 98

(b) Percentage Change in Taxes Paid between 1999 and 2001 by Tax Rate

Figure 2.B.2: Correlations between the Tax Rate and the Taxes Paid

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



(a) Evolution of Taxes Paid by Quintiles of the Tax Rate×Labor share

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

Figure 2.B.3: Correlations between the Tax rate×Labor share and the Taxes Paid

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

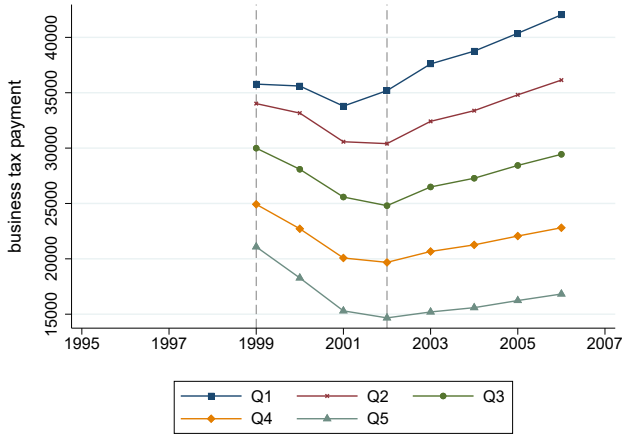


Figure 2.B.4: Percentage change in Taxes Paid between 1995 and 1998 by 2 pp bins

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

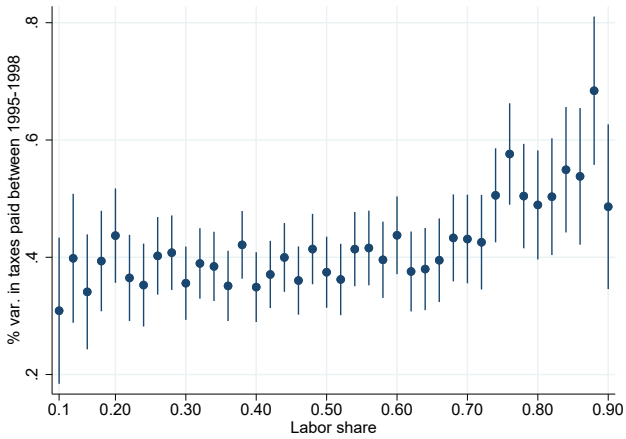


Figure 2.B.5: Evolution of the Business Tax Payments by Quintiles of Labor Share Distribution in 1998

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

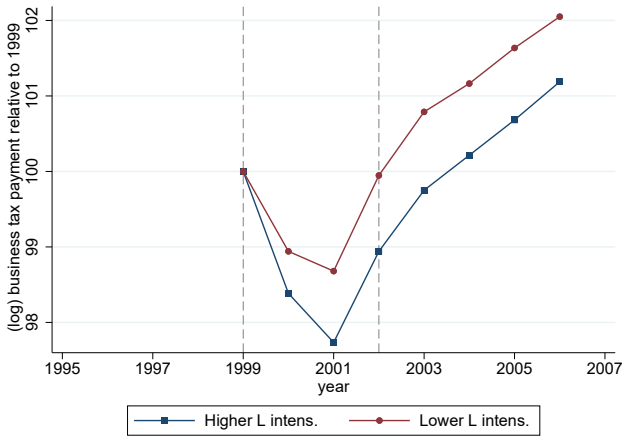


Figure 2.B.6: Averages of the Business Tax Payments over Time

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

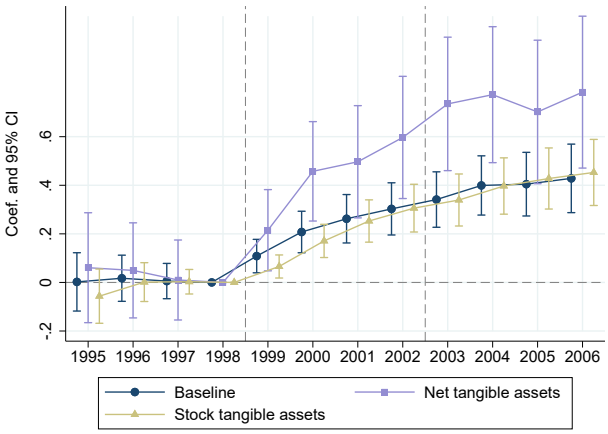
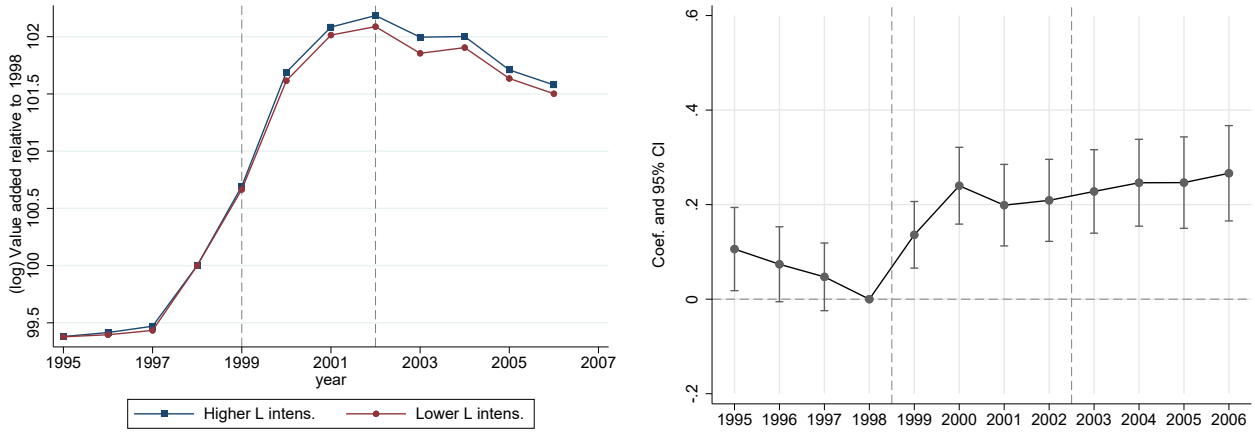


Figure 2.B.7: Effects on Capital - alternative definitions

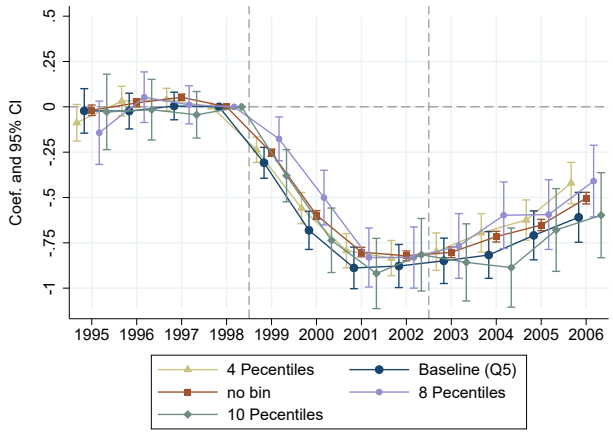
Note: This figure plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). The dependent variables are the log of stock of tangible assets plus investment (labeled as "Baseline"), the log of stock of tangible assets and investment minus depreciation (labeled as "Net tangible asset"), and the log of stock of tangible asset (labeled as "Stock of tangible asset"). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



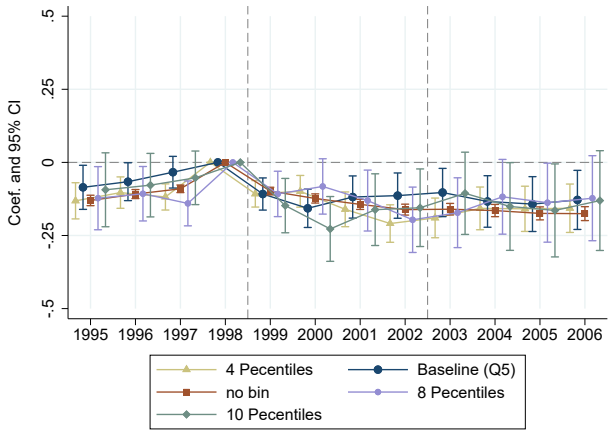
(a) Averages over Time **(b) Event Study Estimates**

Figure 2.B.8: Effects on Productivity per Employee

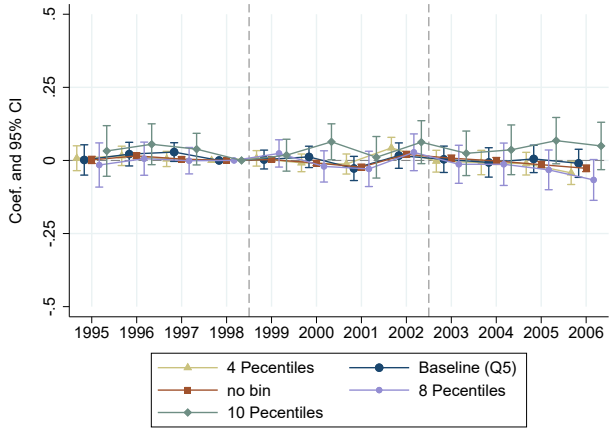
Note: Panel (a) plots the average (log) Value added divided by the number of employees over time relative to 1998. I consider two groups of firms according to whether their 1998 labor intensity is above or below the within-bin median. Bins are defined as detailed in Section 2.3. Panel (b) plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2). The dependent variable is the log of (log)Value added divided by the number of employees. The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



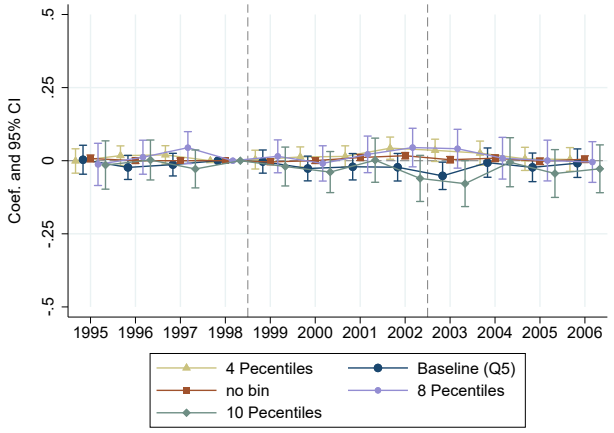
(a) Effect on Taxes paid



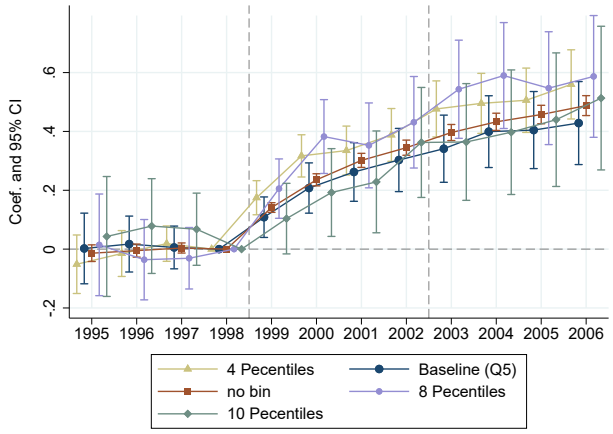
(b) Effect on Employment



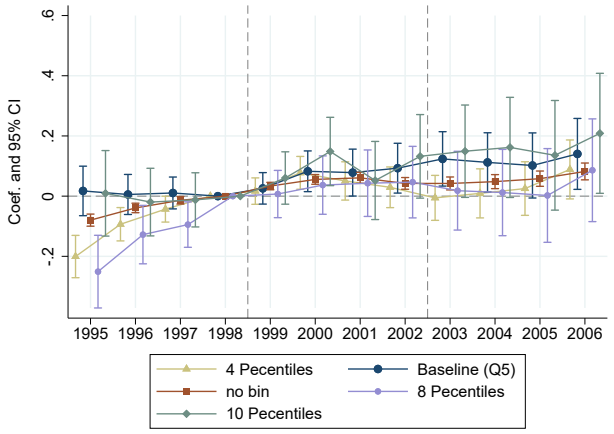
(c) Effect on Median hourly wages



(d) Effect on Annual hours worked per employee



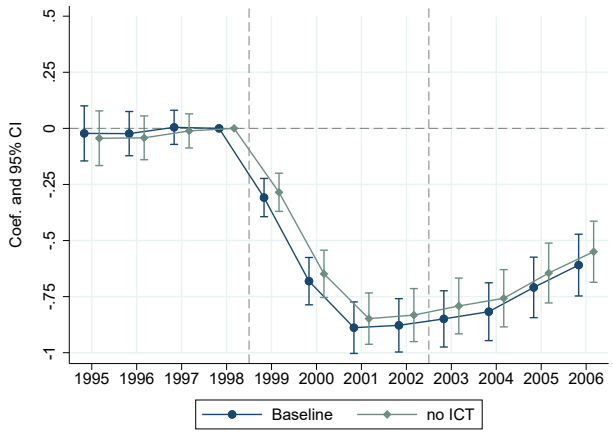
(e) Effects on Capital



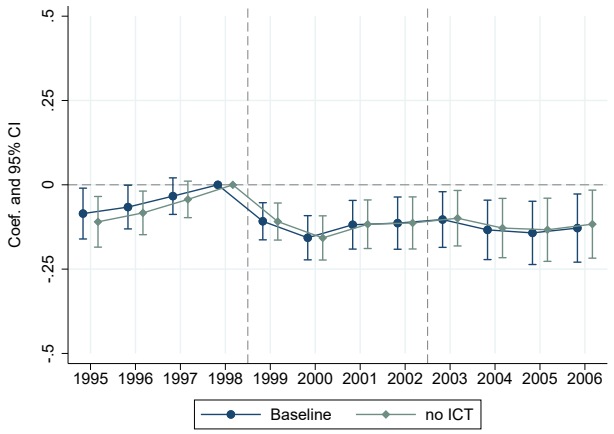
(f) Effects on Productivity

Figure 2.B.9: Robustness: change in bins.

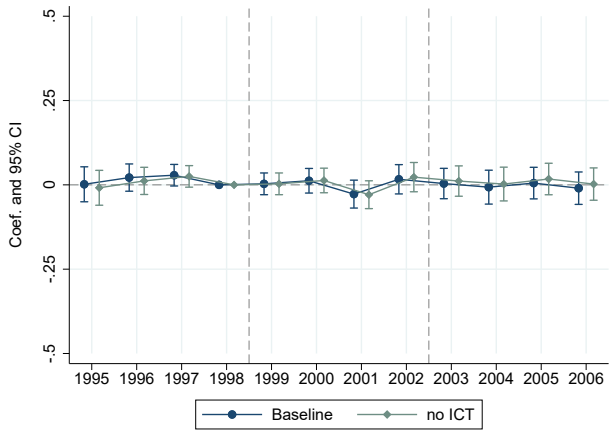
Note: The plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) with different definitions of bins: the four quartiles, the eight percentiles, the ten percentiles of the distribution and no bins. The dependent variable is the log of taxes paid in 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.



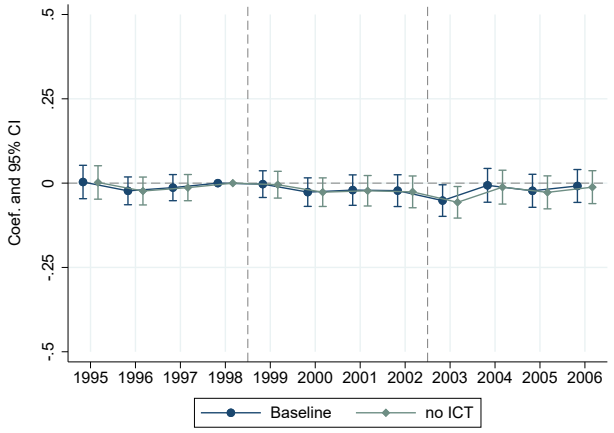
(a) Effect on Taxes paid



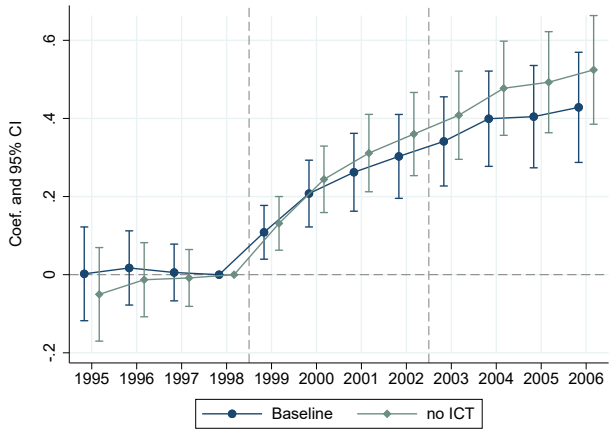
(b) Effect on Employment



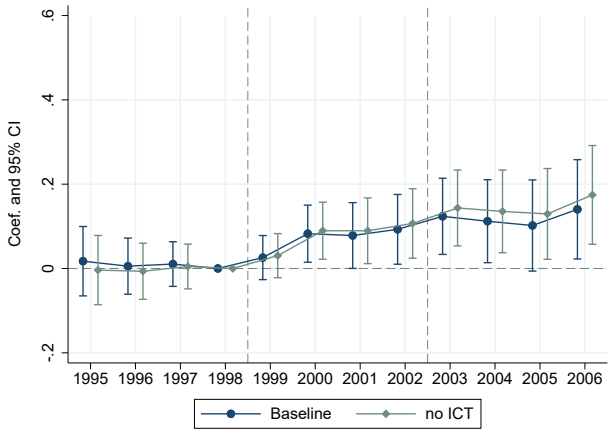
(c) Effect on Median hourly wages



(d) Effect on Annual hours worked per employee



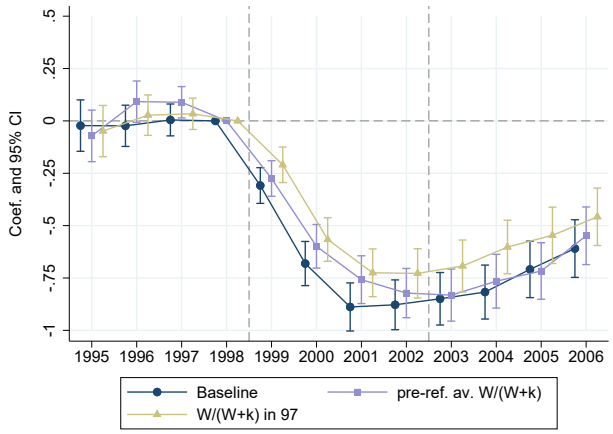
(e) Effects on Capital



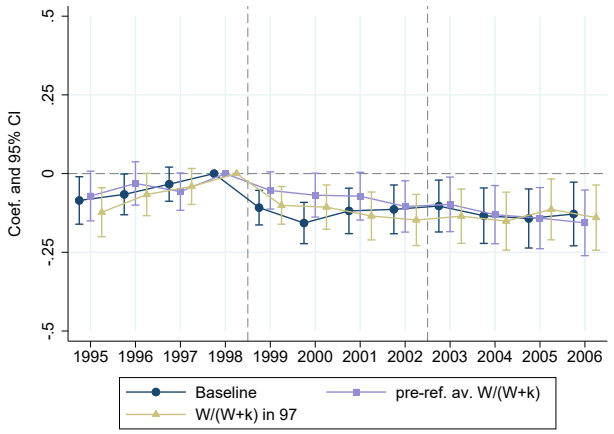
(f) Effects on Productivity

Figure 2.B.10: Robustness: excluding the telecommunication and information sector.

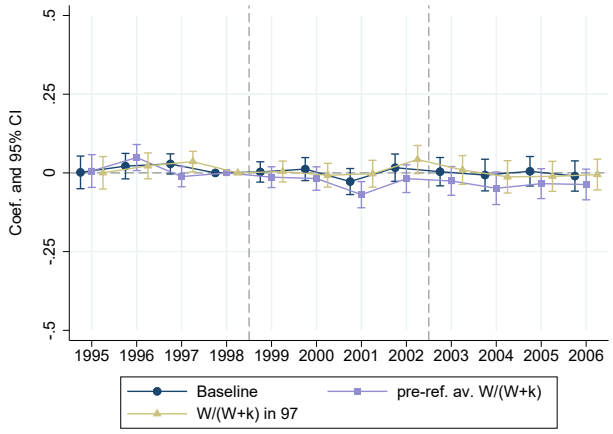
Note: The plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) with different definitions of bins: the four quartiles, the eight percentiles, the ten percentiles of the distribution and no bins. The dependent variable is the log of taxes paid in 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.



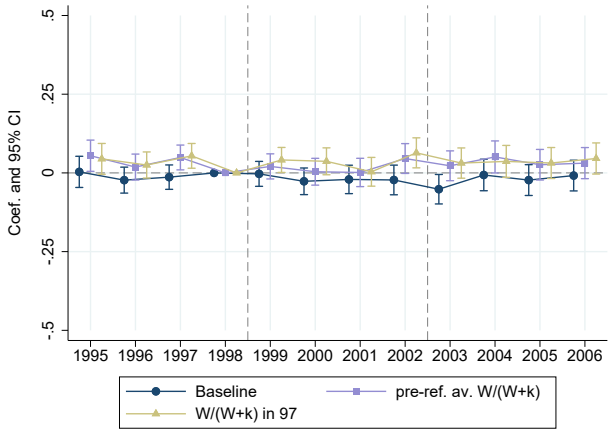
(a) Effect on Taxes paid



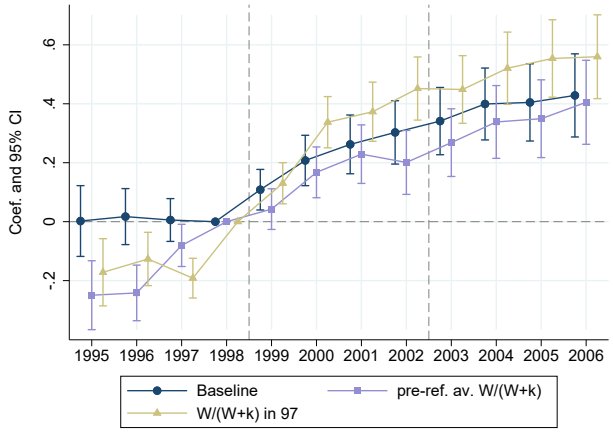
(b) Effect on Employment



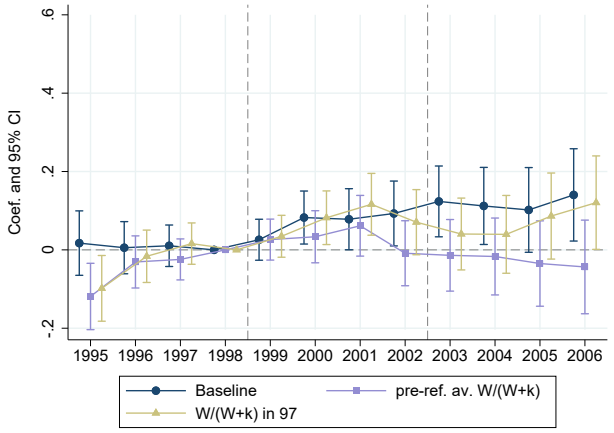
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



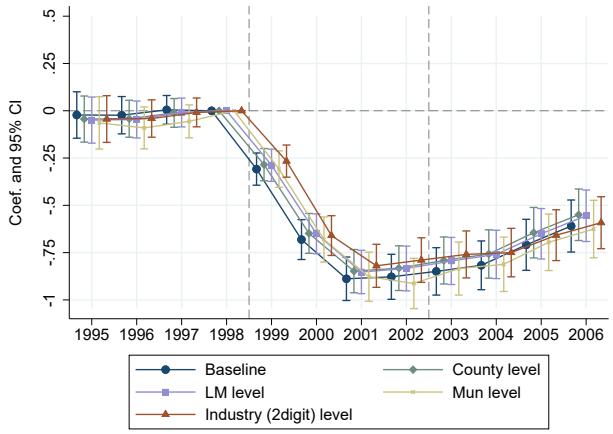
(e) Effects on Capital



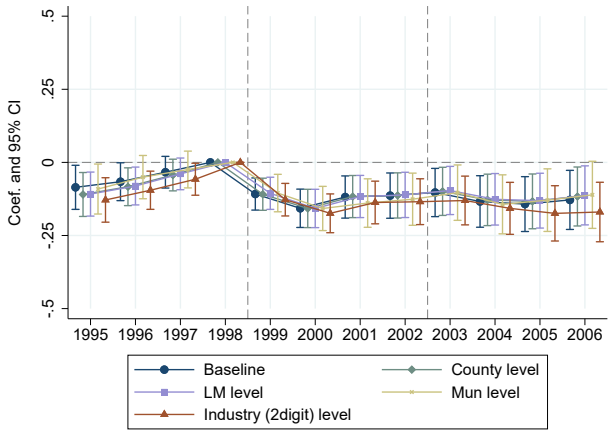
(f) Effect on Productivity

Figure 2.B.11: Robustness: change in the measure of labor share.

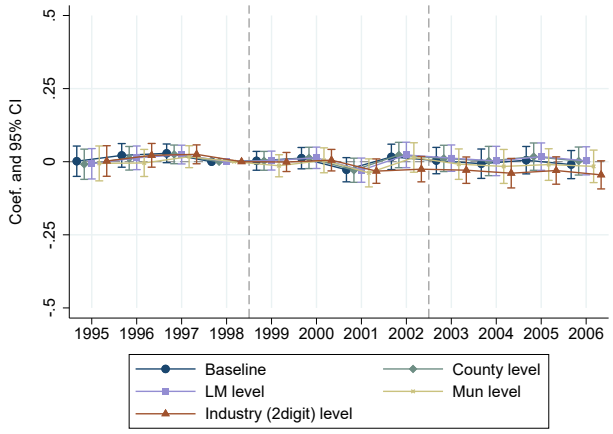
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) with different measures of labor share: the labor share calculated in 1997 – labeled "W/(W+K) in 97", and the average labor share in the pre-reform period (1995-1998) – 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.



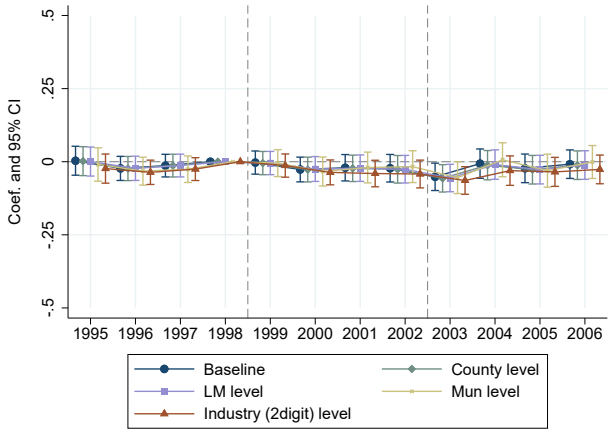
(a) Effect on Taxes paid



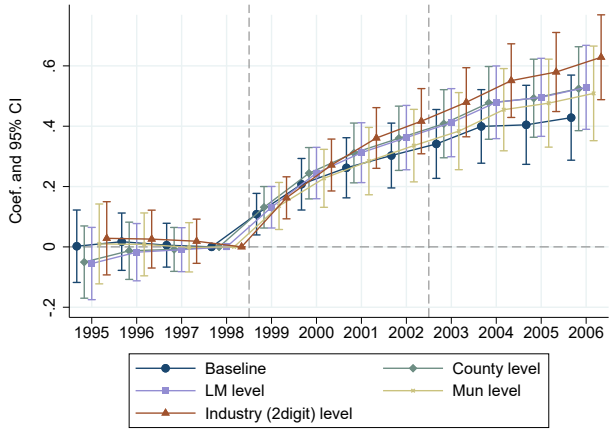
(b) Effect on Employment



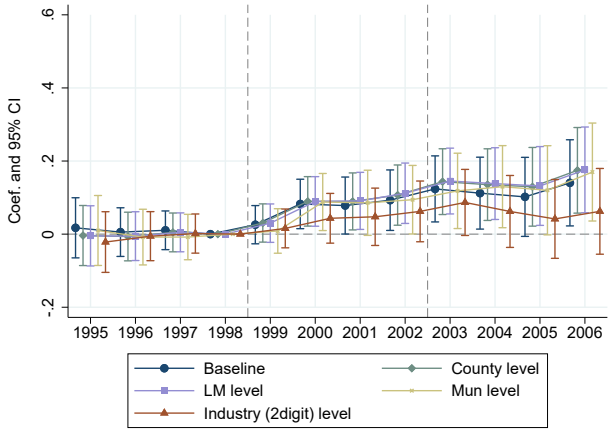
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



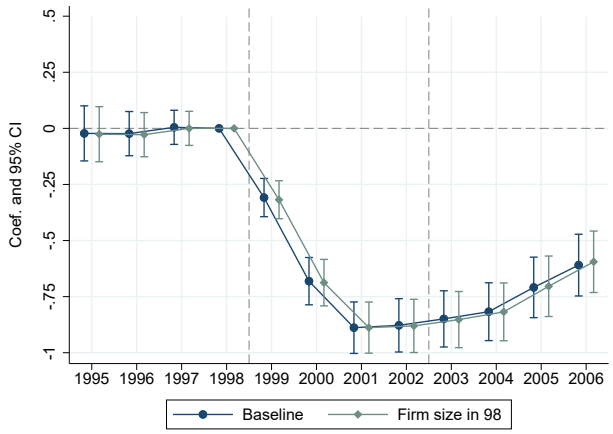
(e) Effects on Capital



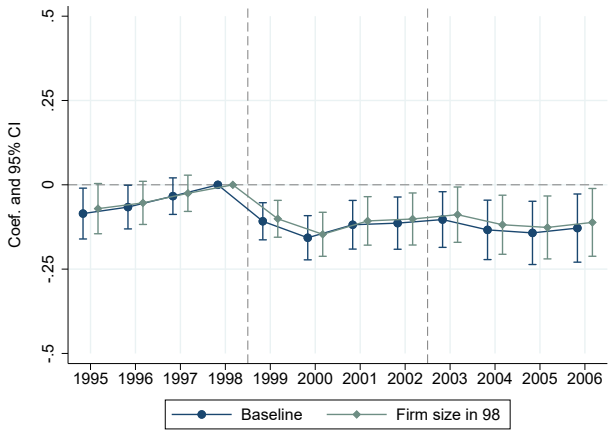
(f) Effect on Productivity

Figure 2.B.12: Robustness: controlling for local shocks.

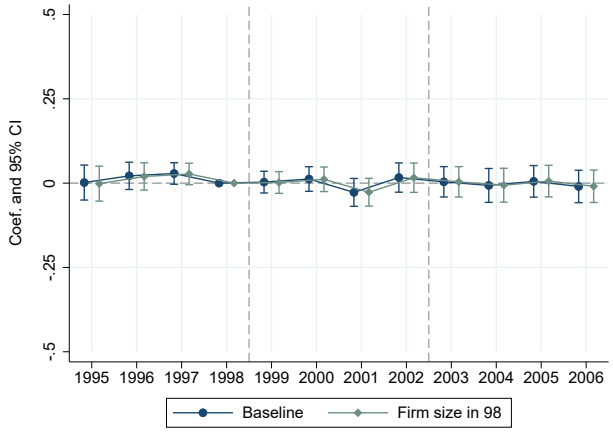
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) and alternative models controlling for the county, labor market (LM), and industry shocks. The dependent variable is the log of taxes paid in Panel (a), the log of 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.



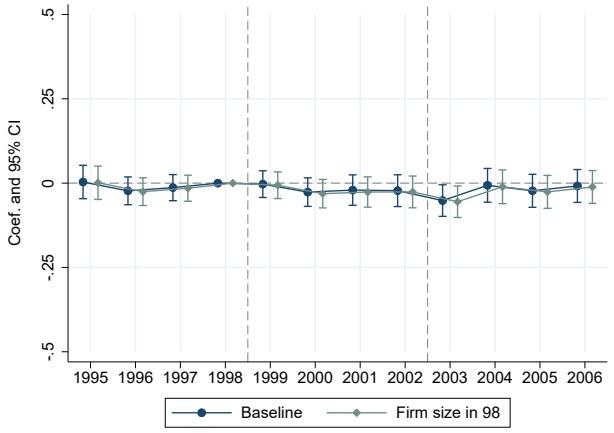
(a) Effect on Taxes paid



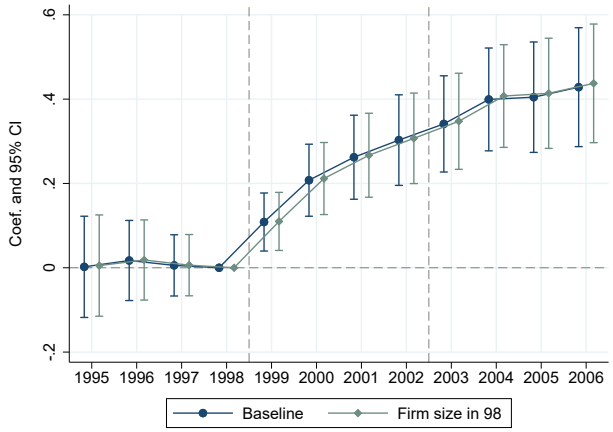
(b) Effect on Employment



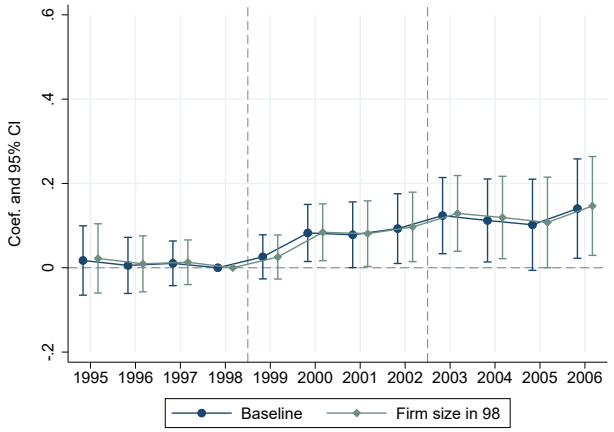
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



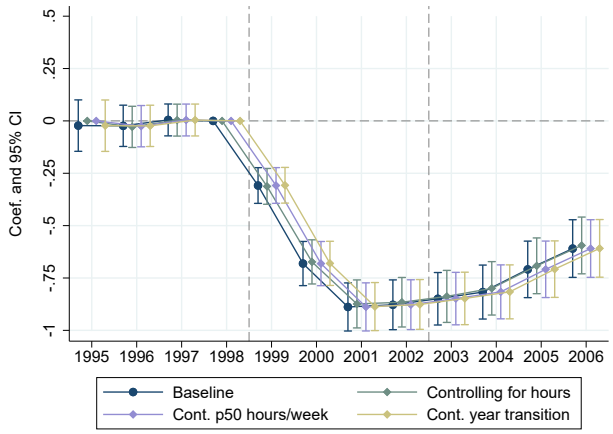
(e) Effects on Capital



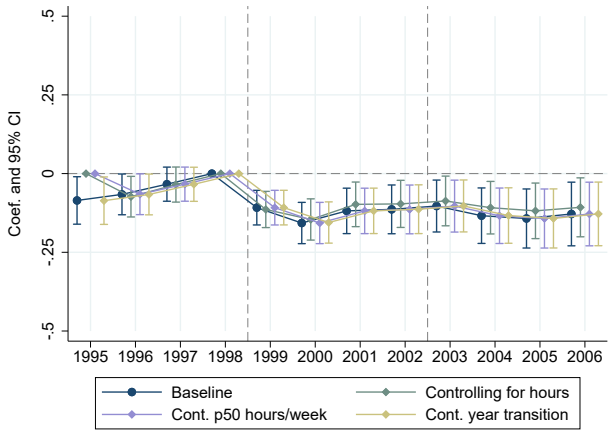
(f) Effects on Productivity

Figure 2.B.13: Robustness: controlling for firms' size.

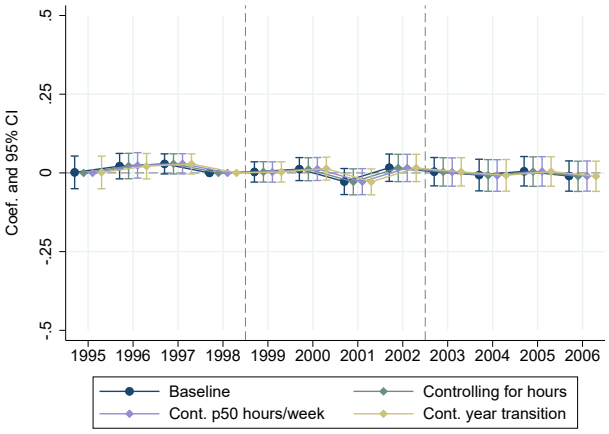
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) and an alternative model controlling for the firm size using seven categories: less than 10, 10-19, 20-49, 50 to 249, 250 to 499, 500 to 999, and more than 999 employees. 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.



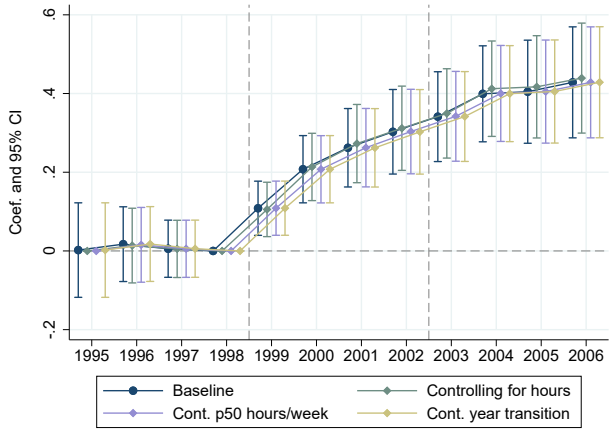
(a) Effect on Taxes paid



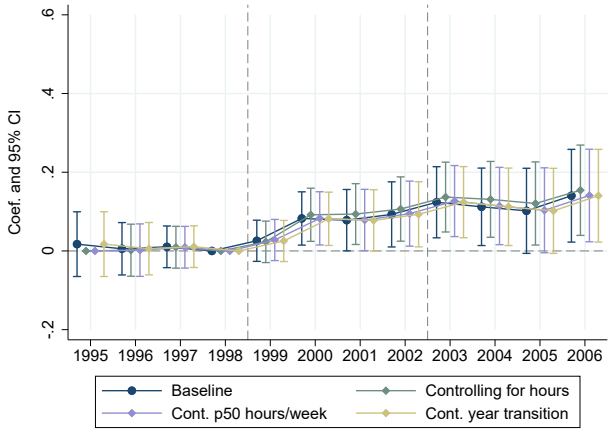
(b) Effect on Employment



(c) Effect on Median hourly wage



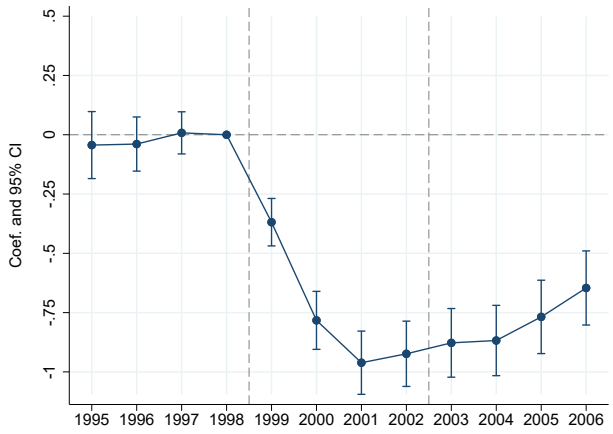
(d) Effects on Capital



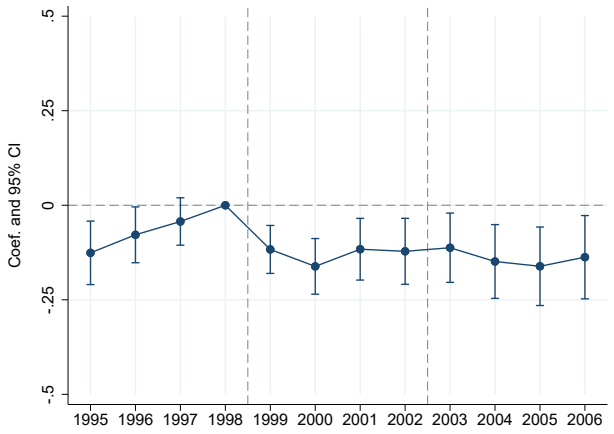
(e) Effect on Productivity

Figure 2.B.14: Robustness: controlling for the reduction in working time.

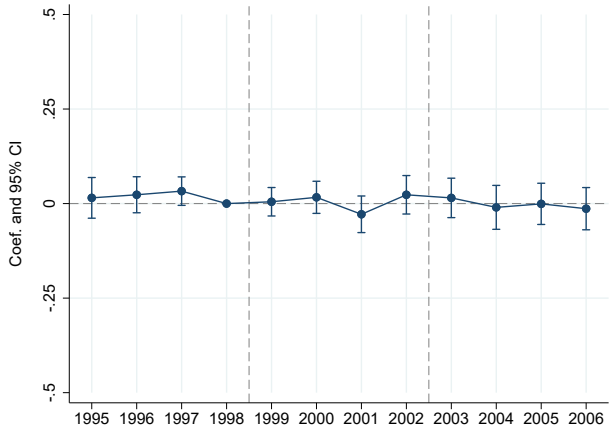
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) and alternative models controlling for hours worked per worker in the previous year, the median hours worked per week per worker in the previous year, and the approximate 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.



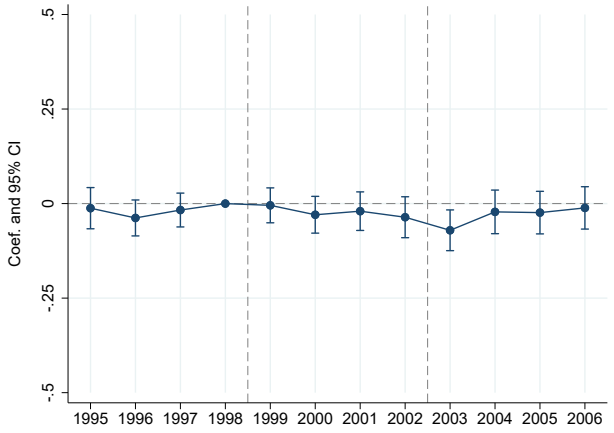
(a) Effect on Taxes paid



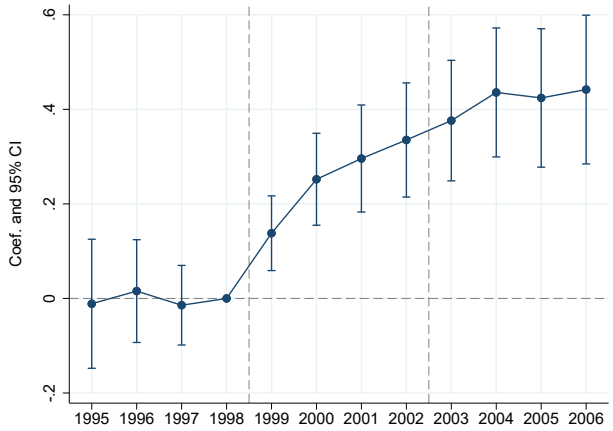
(b) Effect on Employment



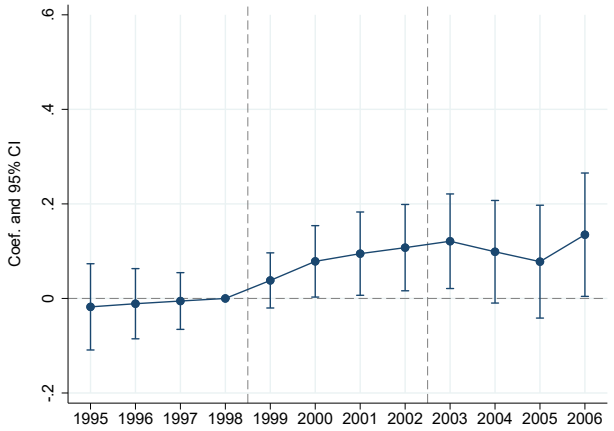
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



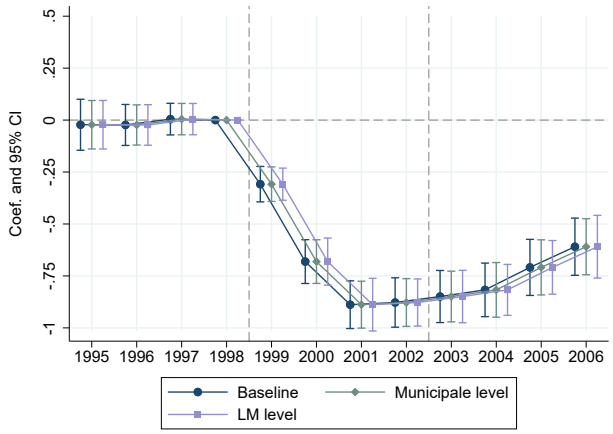
(e) Effects on Capital



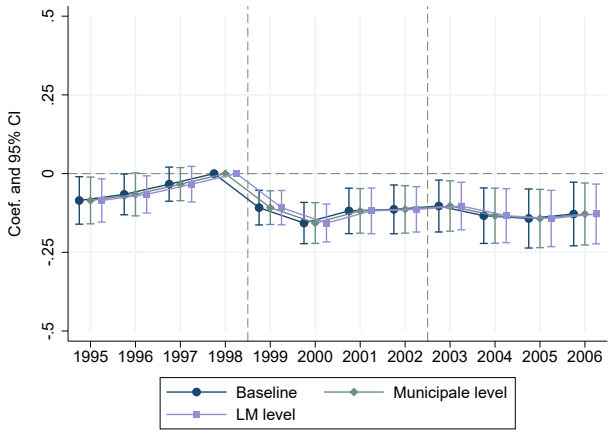
(f) Effect on Productivity

Figure 2.B.15: Robustness: firms with less than 20 employees

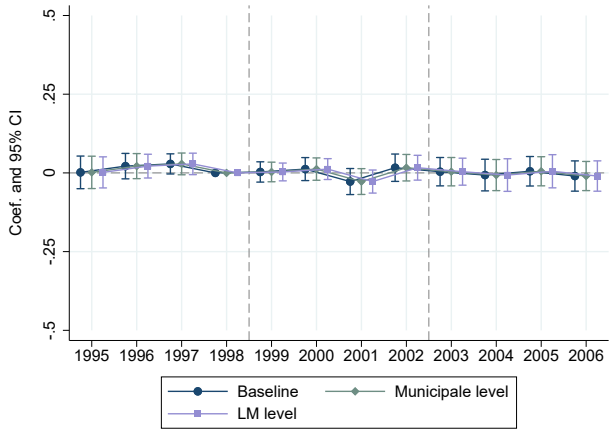
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) restricted to the sample of firms with less than 20 employees in 1998. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees 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.



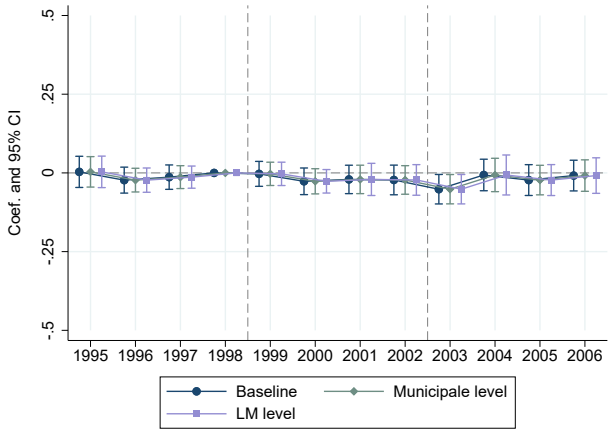
(a) Effect on Taxes paid



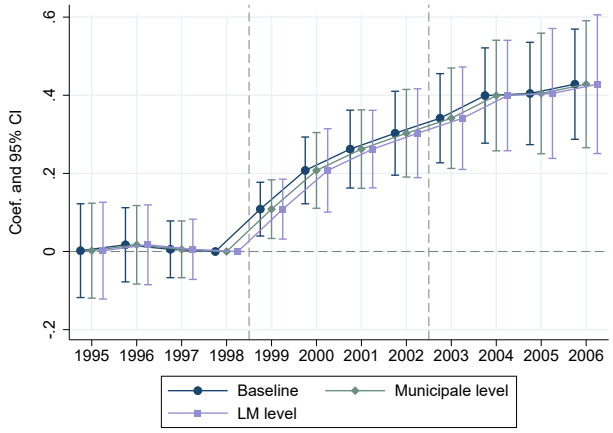
(b) Effect on Employment



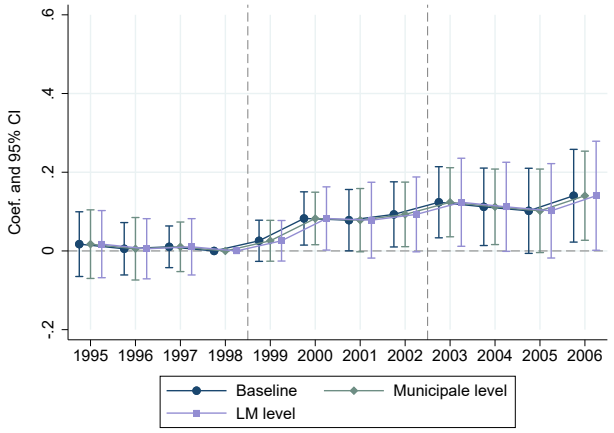
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



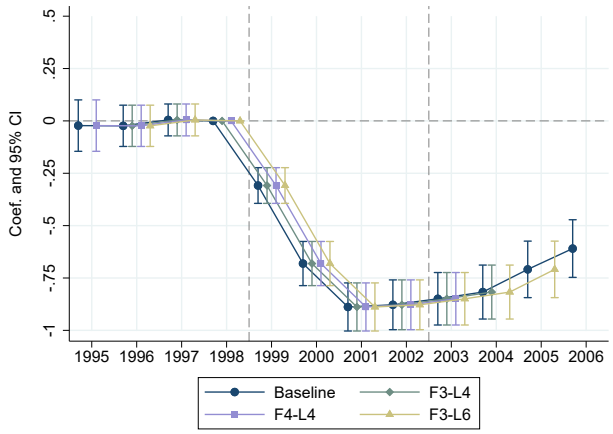
(e) Effects on Capital



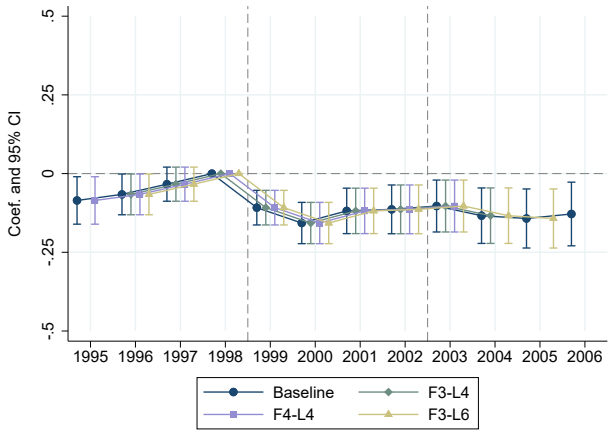
(f) Effect on Productivity

Figure 2.B.16: Robustness: change in the clustering level.

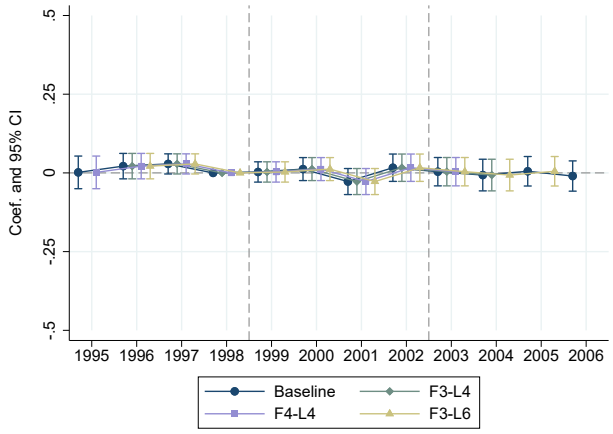
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) and alternative models with standard errors clustered at different levels: the municipality level (n≃36,000) or the local labor market (n≃400). The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.



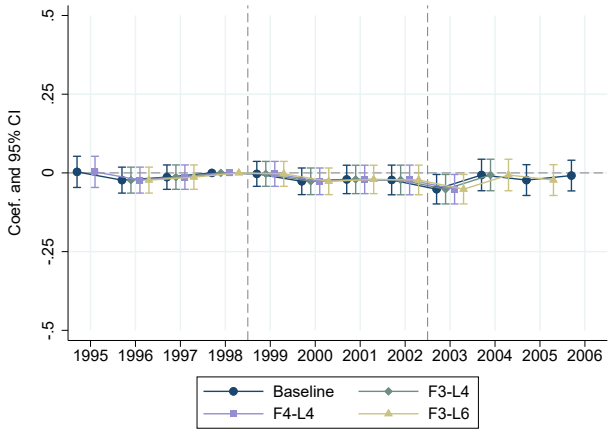
(a) Effect on Taxes paid



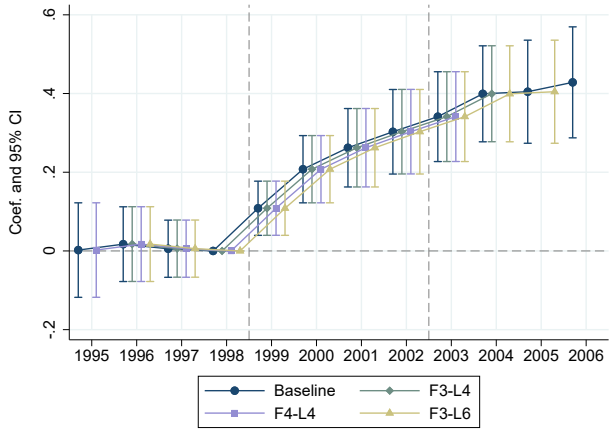
(b) Effect on Employment



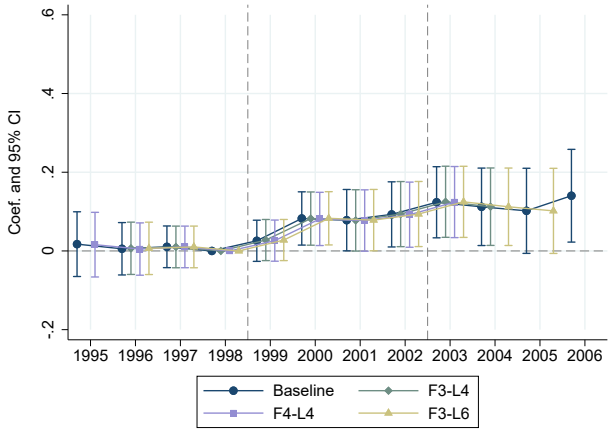
(c) Effect on Median hourly wage



(d) Effect on Hours worked per employee



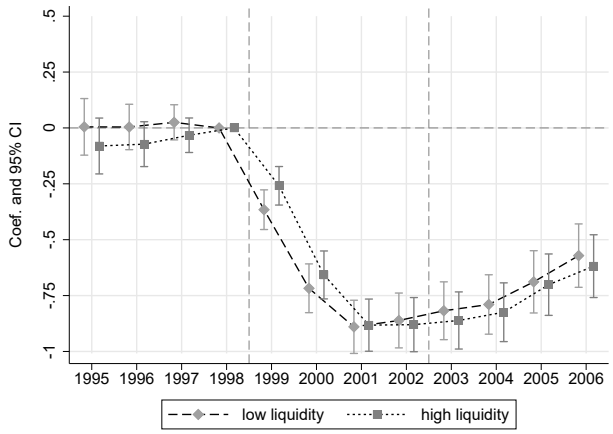
(e) Effects on Capital



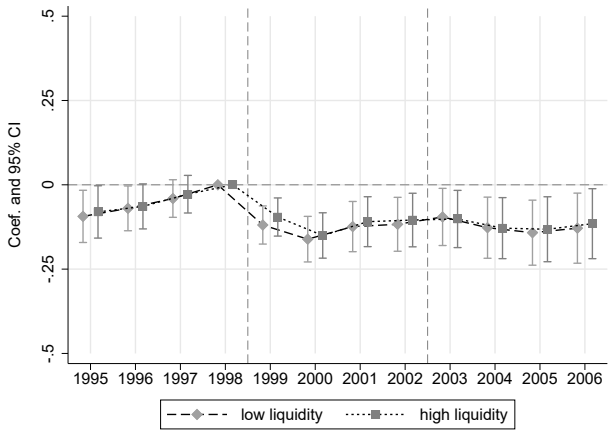
(f) Effects on Productivity

Figure 2.B.17: Robustness: change in the window of estimation.

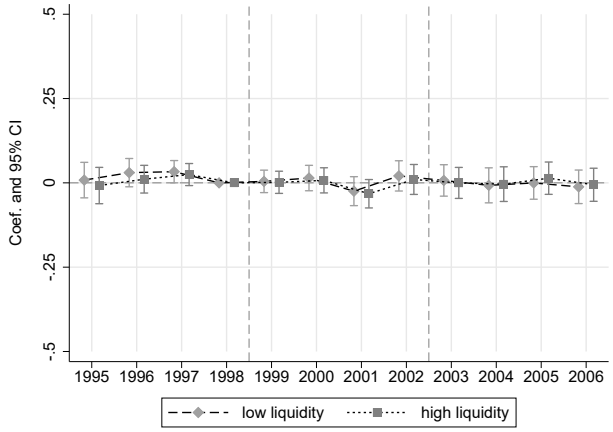
Note: Panel (a), (b) and (c) plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) and alternative models with different numbers of leads and lags: three leads and four lags (F3-L4); four leads and four lags (F4-L4); three leads and six lags (F3-L6). 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.



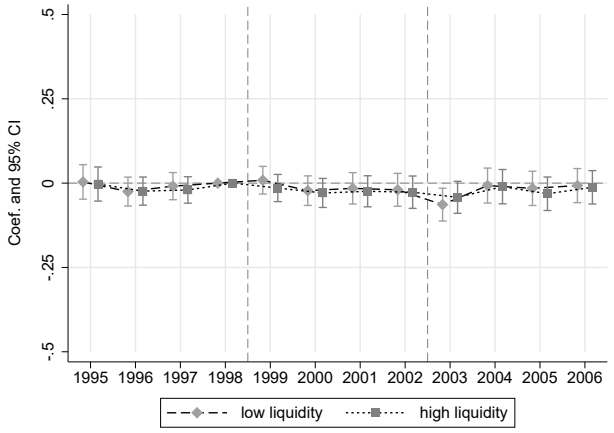
(a) Effect on Taxes paid



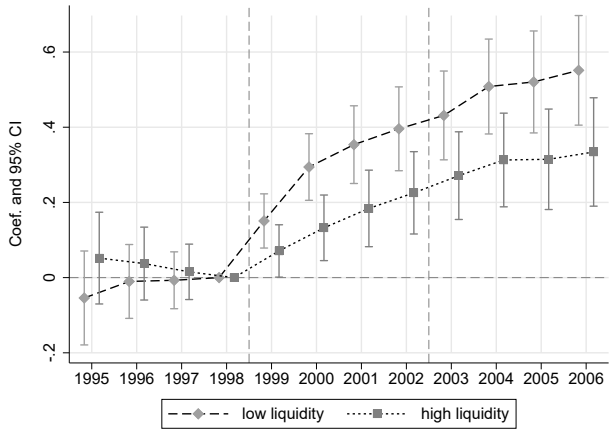
(b) Effect on Employment



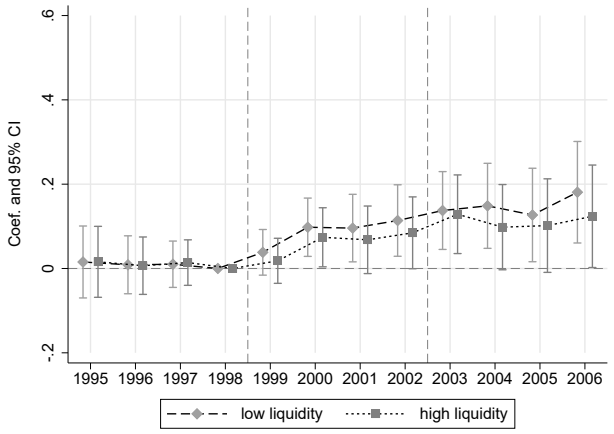
(c) Effect on Median hourly wages



(d) Effect on Annual hours worked per employee



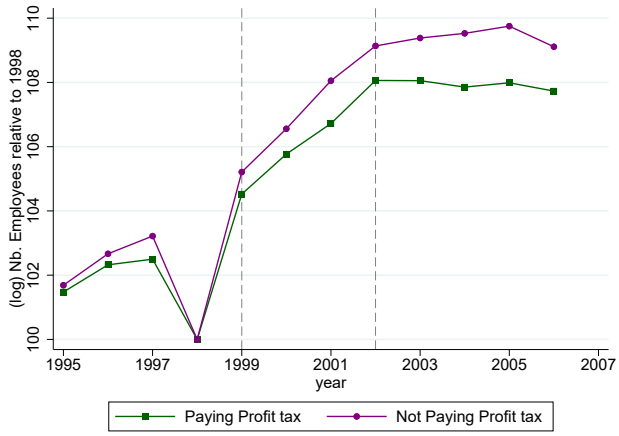
(e) Effects on Capital



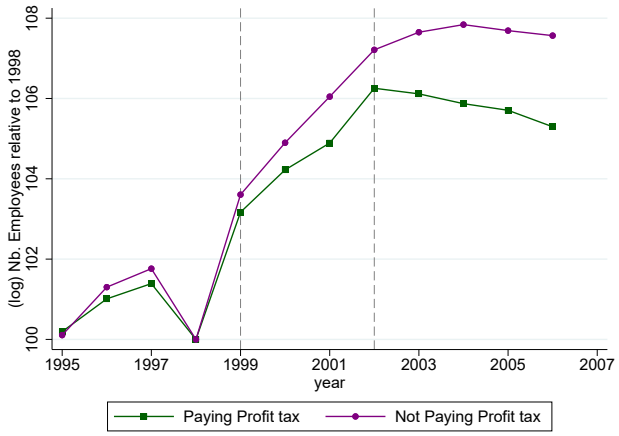
(f) Effects on Productivity

Figure 2.B.18: Effects by Liquidity Constraint

Note: The plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (2.2) fully interacted with a dummy indicating whether the liquidity of firms is below or above the sample's median. The dependent variable is the log of taxes paid in Panel (a), the log of the number of employees in Panel (b), the log of the median hourly wage in Panel (c), the log of the annual hours worked per employee in Panel (d), the log of tangible assets in Panel (e), and the log of value-added/number of employees in Panel (f). The first vertical line highlights the implementation of the reform. The second vertical line highlights the end of the phasing-out period.

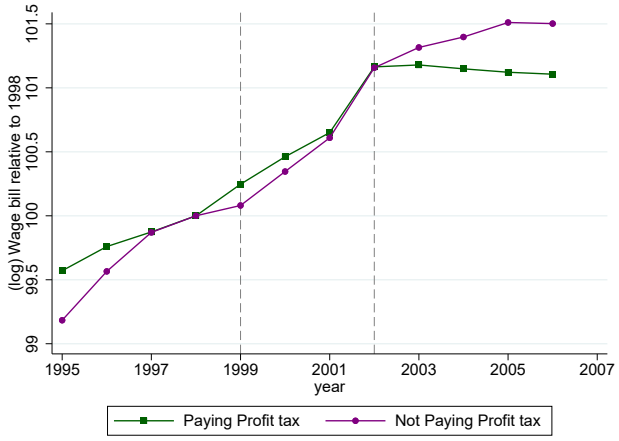


(a) First Quintile



(b) Second Quintile

(c) Third Quintile



(d) Fourth Quintile

(e) Fifth Quintile

Figure 2.B.19: Average Nb. 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.

2.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.

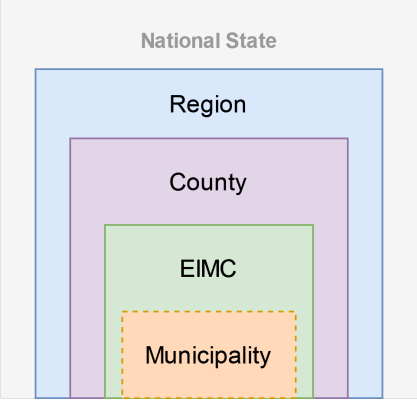


Figure 2.C.1: Administrative Divisions

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 2.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*), with an average of 1,400 inhabitants, and half of these municipalities count fewer than 400 inhabitants (see Table 2.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.[24]

[24]They also share the cost of local public goods such as public transport or waste management. See Tricaud (2025) 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 transferred 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 continue to define their tax rates, and the EIMC determines a rate that adds up to those of the municipalities. See Breuillé et al. (2018) for more details.

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 2.C.2 shows the average business tax rates for the different tiers of local government. It is worth noting that the bulk of the tax rate is determined at the municipal or EIMC level.

Table 2.C.1: Average Municipality

	Mean	SD	Min.	Max.	p25	p50	p75
Population	1,657.6	12,212.14	0	2,215,197	181	396	981
EIMC *(share of Mun.)*	0.83	0.38	0.00	1.00	1.00	1.00	1.00
Business tax rate	23.05	6.53	0	91.40	18.54	22.37	26.83
Property tax rate on dev. land	29.81	8.12	7.40	86.80	24.10	29.20	34.60
Property tax rate on undev. land	85.31	41.52	11.50	507.50	59.80	74.50	99.30
Observations							400,279

Note: This table shows the mean, Standard deviation (SD), minimum and maximum values, as well as the 25th (p25), 50th (p50), and 75th (p75) percentiles of non-merged municipalities over the period 2000-2010. The tax rates summarized here are the cumulative rates, summing municipal/EIMC, county, and regional rates. The rates are reported in percentages.

Table 2.C.2: Local Business tax Rates by Administrative Division

	Mean	SD	Min.	Max.	p25	p50	p75	N
Municipality/EIMC	13.15	6.21	0	80.53	8.93	12.22	16.39	36,389
+ *County*	8.27	2.40	0	16.2	6.9	7.8	9.30	96
+ *Region*	2.62	0.71	0	4.3	2.20	2.50	3.10	22
= **Business tax**	23.05	6.53	0	91.4	18.54	22.37	26.83	36,389

Note: This table shows the mean, Standard deviation (SD), minimum and maximum values, as well as the 25th (p25), 50th (p50), and 75th (p75) percentiles of the local business tax rates at the different administrative divisions (Municipality, County, and Regions) over the period 2000-2010. The rates are reported in percentages.

2.D Construction of the Panel of Firms

Linked employer-employee data contains key elements that enable me to construct a panel of firms with all the information required. I use the *SIREN* firm identifier and the year to merge the labor force data to the Balance sheet data.[25] In the linked employer-employee data, I also have the municipality where the plant is located. I first harmonize this identifier to

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

the geography of 2010.[26] 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.

2.E Conceptual Framework

In this section, I use a simple microeconomic model of labor demand to show under which circumstances a tax on labor affects firms' employment and investment decisions. Let there be a representative firm that produces with a production function Cobb-Douglas $q = F(K, L) = K^{\alpha}L^{\beta}$ with factors capital K and labor L and $0 < \alpha, \beta < 1$ and $\alpha + \beta \leq 1$ such that firms make profits. Firms also face a tax on the wage bill τ_p and a corporate profit tax τ_c where a share $\rho \in [0; 1]$ of the capital costs and a share $\theta \in [0; 1]$ of the tax on labor can be deducted from the corporate tax base.[27]. Firms' after-tax profit (Π) can be decomposed in the following way:

$$\Pi = \underbrace{K^{\alpha}L^{\beta} - wL - rK}_{\text{Gross profit}} \overbrace{-\tau_p wL}^{\text{Tax on the wage bill}} \underbrace{-\tau_c\Big[K^{\alpha}L^{\beta} - wL - \rho K - \overbrace{\theta\tau_p wL}^{\text{deductible part of the tax on the wage bill}}\Big]}_{\text{Coporate tax liabilities}} \tag{2.3}$$

Collecting terms provides the following equation:

$$\Pi = K^{\alpha}L^{\beta}(1 - \tau_c) - [1 + \tau_p - \tau_c(1 + \theta\tau_p)]wL - (1 - \rho\tau_c)K \tag{2.4}$$

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:

[26] 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.

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

$$\frac{\partial \Pi}{\partial L} = \beta K^{\alpha} L^{\beta-1}(1-\tau_c) - [1+\tau_p - \tau_c(1+\theta\tau_p)]w = 0 \tag{2.5}$$

$$\frac{\partial \Pi}{\partial K} = \alpha K^{\alpha-1} L^{\beta}(1-\tau_c) - (1-\rho\tau_c)r = 0 \tag{2.6}$$

The ratio of the two conditions gives:

$$\frac{\beta}{\alpha}\frac{K}{L} = \frac{[1+\tau_p - \tau_c(1+\theta\tau_p)]w}{(1-\rho\tau_c)r} \tag{2.7}$$

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

$$L = \left(\frac{\alpha}{(1-\rho\tau_c)r}\right)^{\frac{\alpha}{1-\alpha-\beta}} \cdot \left(\frac{\beta}{w} \cdot \frac{1}{1+\tau_p - \tau_c(1+\theta\tau_p)}\right)^{\frac{1-\alpha}{1-\alpha-\beta}} \tag{2.8}$$

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

$$K = \left(\frac{\alpha}{(1-\rho\tau_c)r}\right)^{\frac{1-\beta}{1-\alpha-\beta}} \cdot \left(\frac{\beta}{w} \cdot \frac{1}{1+\tau_p - \tau_c(1+\theta\tau_p)}\right)^{\frac{\beta}{1-\alpha-\beta}} \tag{2.9}$$

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:

$$\frac{\partial L}{\partial \tau_p} = A \cdot \frac{1-\alpha}{1-\alpha-\beta} \cdot (-(1-z\tau_c)) \cdot \left(\frac{1}{1+\tau_p - \tau_c(1+\theta\tau_p)}\right)^{\frac{1-\alpha}{1-\alpha-\beta}+1} \tag{2.10}$$

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

$$\frac{\partial L}{\partial \tau_p \partial z} = A \cdot \frac{1-\alpha}{1-\alpha-\beta} \cdot \tau_c \cdot \left(\frac{1}{1+\tau_p - \tau_c(1+\theta\tau_p)}\right)^{\frac{1-\alpha}{1-\alpha-\beta}+1} \tag{2.11}$$

$$\times \left[1 - (1-z\tau_c)\cdot(\tau_p)\cdot\left(\frac{1-\alpha}{1-\alpha-\beta}+1\right)\cdot\frac{1}{1+\tau_p - \tau_c(1+\theta\tau_p)}\right] \tag{2.12}$$

This expression is positive if and only if the second line of the equation (2.12) is positive, which is equivalent to:

$$\theta > \frac{1}{\tau_c} + \frac{1}{\tau_p}\frac{1-\alpha-\beta}{1-\alpha}(1-\frac{1}{\tau_c}) \tag{2.13}$$

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.

Chapter 3

Business Taxes, Capital and Labor

This chapter is based on Gharbi et al. (2022).

3.1 Introduction

How do business taxes affect firms' production? This has been one of the fundamental questions in the academic and public debate. Proponents of higher business taxes argue that business income is often reclassified as labor income, which should be taxed for fairness reasons. They also point that firms should contribute to public finances. Opponents, meanwhile, argue that increasing the tax burden on firms can hinder economic growth by discouraging investment and, ultimately, reducing employment.

This concern has underpinned numerous policy reforms aimed at lowering the business tax burdens, such as the US Tax Cuts and Jobs Act (TCJA). Empirical assessments have proven challenging and yielded a wide range of estimated elasticities from -.25 to -1.6 (see Zwick and Mahon, 2017, for a quick overview). As a result, much of the recent evidence comes from targeted tax deductions that apply to specific industries or asset types (e.g., Zwick and Mahon, 2017; Ohrn, 2018; Garrett et al., 2020). However, the extent to which the effects of these targeted policies can be generalized to broader business tax reforms remains unclear.

This paper provides systematic empirical evidence on how capital stock and employment respond to business taxation on capital. We use France as our laboratory, where a local business tax, known as the *Professional Tax*, has been levied since the mid-1970s. We focus on the period

from 2002 to 2010, during which the tax was primarily based on capital. This period enables us to leverage local tax variations across municipalities while maintaining a constant tax base.

A few studies have examined the effects of business tax increases (Fuest et al., 2018; Giroud and Rauh, 2019; Link et al., 2024; Lichter et al., 2025), but they fall short of fully assessing their impact, as they do not link investment and employment responses within firms across all industries. Yet such a connection is crucial, as capital and labor responses to tax changes are often intertwined. Rich micro-level data are therefore essential to better understand the mechanisms behind firm behavior and for uncovering potential heterogeneity in how firms respond to changes in business taxation.

In this paper, we use administrative-linked employer-employee microdata from the Social Security record. These data encompass the entire universe of French private sector establishments and provide detailed information about their employees, including wages, occupations, and the number of hours worked. Importantly, we can observe the capital stock of establishments, distinguishing long-duration from short-duration assets. We link firm data to municipal data on local taxes using unique plant identifiers, enabling us to track plants over time and space. By taking advantage of the multiple and staggered changes in business tax, we estimate the causal impact of taxes on labor and capital in standard event study models and confirm our main results are robust to potential heterogeneous treatment effects.

The baseline results indicate that firms' tax payments increase significantly after a tax hike – with an estimated elasticity of -2.26 with respect to the net-of-tax rate. This response is consistent with the mechanical effect of higher statutory tax rates. However, we find no evidence that firms adjust their capital stock in response to the tax increase. This null effect persists across different types of assets, including both movable and immovable properties, despite the tax being levied almost entirely on the stock of capital. These findings suggest that firms do not engage in capital disinvestment or reallocation in the short to medium run, possibly reflecting the irreversibility or rigidity of capital adjustments.

Similarly, we find no significant effect of the tax increase on labor-related outcomes. Employment levels remain stable, and although there is a small decline in the median hourly wage, the overall wage bill remains unaffected. Moreover, hours worked do not increase, which could

have otherwise offset a constant wage bill. Taken together, these findings indicate that the tax increase does not trigger compensatory adjustments in the firm's workforce, either through changes in the number of employees, wage levels, or working hours.

To assess whether these null effects mask heterogeneous responses across firms, we sort firms by capital intensity and firm size. Capital-intensive firms – defined by a low ratio of wage bill to tangible assets – are, by construction, more exposed to the tax. However, even within this group, we find no significant adjustment in either capital or labor inputs. Similarly, splitting the sample by firm size or revenue levels reveals no differential effects, despite the relatively larger burden that fixed business taxes impose on smaller firms. These results suggest that the absence of behavioral responses to local business tax increases is robust across key dimensions of firm heterogeneity.

We demonstrate that these results are robust. We obtain similar estimates after including a comprehensive set of flexible, non-parametric local controls at various levels of aggregation. It suggests that potentially relevant omitted variables, such as local shocks, are not driving the results. We demonstrate that the point estimates are also robust to the level of clustering, indicating that standard errors are not artificially inflated. The results do not depend on the window of the event study. More importantly, by applying the estimators provided by de Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024), we demonstrate that potential heterogeneous treatment effects across cohorts do not bias our estimations.

Related literature This paper contributes to several strands of the public finance literature. First, this paper adds to the literature focusing on the effect of local taxes on firms' decisions. Since Hall and Jorgenson (1967) and King and Fullerton (1984), we know that business taxation impacts firms, especially by affecting the cost of capital. Timing, levels, and compositions of investments are significantly impacted by fiscal policies[1]. Suárez Serrato and Zidar (2016) and Fuest et al. (2018) show that around 50% of the corporate tax burden is borne by workers via

[1]Thanks to improvement in data availability – especially administrative data, recent articles have assessed the effects of business taxes on additional outcomes: Cummins et al. (1996) on firm investment, Mukherjee et al. (2017) on research and development, Duranton et al. (2011) on economic growth.

wage adjustments, using different methodologies (e.g., a spatial equilibrium model or a purely empirical approach). Recent evidence suggests the effect is symmetric, following a tax cut, wages increase, at least for manufacturers in Québec (Duan and Moon, 2024a). We also know that corporate tax changes affect employment (Ljungqvist and Smolyansky, 2014; Giroud and Rauh, 2019; Kennedy et al., 2022), but also investment (Mukherjee et al., 2017; Link et al., 2024; Lichter et al., 2025). In the US context, Giroud and Rauh (2019) further highlights that reallocation across lower-tax states explains half of the reduction in local employment. However, most of these studies focus on business taxes based solely on profit, which may not provide insight into the impact of taxes directly tied to the cost of capital used in production. Even when different fiscal policies reduce the user cost of capital by the same margin, they can have very different effects on investment by introducing distinct frictions into the firms' decision-making process (Chen et al., 2023).

As previously highlighted, another strand of the literature has leveraged targeted tax incentives for identification purposes. These incentives directly reduce the cost of fixed capital investments. This strand of the literature has been growing over the last few years. However, there is still limited evidence examining the effect of tax incentives on labor and capital outcomes within the same firms and across various industries. Most existing studies linking labor and capital outcomes within firms have focused exclusively on manufacturing (Lerche, 2022; Duan and Moon, 2024b). The only exception is probably Harju et al. (2022) – in the context of corporate taxation, but it is limited to small firms. Other papers, typically focusing on capital, have consistently shown that investment is highly sensitive to taxes (Zwick and Mahon, 2017; Ohrn, 2018; Maffini et al., 2019; Chen et al., 2023). Despite targeting specific capital and industries, these tax incentives can have spillover effects on the local labor market (Curtis et al., 2022; Lerche, 2022). Additionally, studies have found that these incentives can influence firms' workforce composition (Gaggl and Wright, 2017; Tuzel and Zhang, 2021; Duan and Moon, 2024b). Given these potential interactions, it is crucial to examine how firms respond to tax incentives by linking labor and capital outcomes across all industries.

Second, by assessing comprehensively the effects of a business tax that is substantially based on the stock of capital – properties and machinery, we contribute to the literature estimating

the incidence of local property taxes. While most recent estimates underline the negative effects of property taxes on firms, older empirical papers hardly confirm this conclusion[2]. Reasons for the insignificant effect could be the lack of microdata and weak identification strategies fraught with endogeneity. Using pairwise spatial differentiation and instrumental variables, Belotti et al. (2016) and Ramboer (2019) find negative effects on firms growth. Ramboer (2019) and Rathelot and Sillard (2008) underline negative effects on firms' entries. Due to limited data coverage, these papers cannot give a full picture of the consequences of local property tax variations. Namely, they do not have granular data and do not cover every sector. Enami et al. (2023) evaluates the effects on a wider panel of firms, but they focus on the state of Ohio in the United States. By combining linked employer-employee data covering the universe of French establishments with the establishments' tax statements, we precisely assess the effects of capital taxes on employment and investment in tangible assets across all private sectors, whereas previous papers could not.

The remainder of the paper is organized as follows. Section 3.2 presents the institutional setting and the data. Section 3.3 details the empirical approach. Section 3.4 shows the impact of local business taxation on employment, investment, and wages and details the mechanisms at stake. Section 5 concludes.

3.2 Institutional Setting and Data

Section 3.2.1 first describes the institutional context. Section 3.2.3 details the data sources and the construction of the panel of estimation.

3.2.1 The Business Tax in France until 2010

Since 1975, a local business tax (henceforth BT), known as the *Professional Tax*, has been levied on establishments with for-profit activities in France. The tax was initially based on fixed tangible assets, properties, and the wage bill. Even though this base has evolved, it has

[2]Carlton (1983), Bartik (1985), Papke (1991), and Dye et al. (2001)

always been defined at the national level.[3] The tax base of each jurisdiction depends solely on the inputs located within its boundaries; there are no apportionment rules. In this paper, we focus on the period 2003-2010, when the tax is based on capital, including buildings, machinery, and equipment.

More precisely, firms are taxed based on the capital used in their production process, regardless of ownership. The taxable value of fixed assets is determined by their rental value – defined as 16% of the purchase cost – or by the actual rent paid by the establishment. For properties, the rental value is set at 9% of the value recorded on the balance sheet. Several tax reductions are available. Firms automatically receive a 16% abatement on the entire tax base. Additionally, they may claim a further reduction on tangible assets, which decreases progressively with net revenue. Starting in 2004, a tax relief measure was introduced that exempts new investments in tangible assets from taxation during the first year. In 2007, this relief was modified to become gradually phased out over three years. As this tax reduction on investment applies to all firms, it should not pose a threat to identification. More details on the computation of the tax base can be found in Appendix 3.C.2.

While the tax base is the same for any business located in France, the tax rate depends on the location of the plant. Three sub-national administrative divisions determine the final rate that plants face. These jurisdictions divide the national territory into different tiers of local government. Each division has fiscal authority, setting the tax rates levied within its jurisdiction. As can be seen in Figure 3.2.1, 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*), with an average of 1,400 inhabitants, and half of these municipalities count fewer than 400 inhabitants (see Appendix Table 3.A.1).

[3] A reform in 1999 abolished labor from being part of the tax base. The phasing out of the waste bill from the tax base was progressive between 1999 and 2002, see Chapter 2 for more details. Another reform in 2010 changed the business tax once more. The BT is replaced by a contribution solely based on property value and a constant value-added portion.

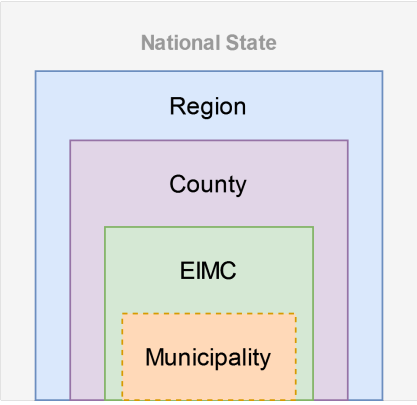


Figure 3.2.1: Administrative Divisions

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.

In addition, municipalities may voluntarily engage in inter-municipal cooperation, which is strongly encouraged by the national government through financial incentives. This cooperation allows municipalities to share the costs of local public goods and coordinate their fiscal policy. In particular, for the business tax, this introduces an additional local tax rate, which may either supplement or replace the rate set by individual municipalities.[4] Such arrangements result in the formation of a new administrative structure known as Establishments for Inter-Municipal Cooperation (henceforth EIMC).[5] Each municipality may belong to only one EIMC at a time.

Overall, the business tax rate faced by firms is the sum of the tax rates fixed by the municipality, the EIMC, the county, and the region. As a result, the rate faced by firms depends on their location. Appendix Table 3.A.3 shows the average business tax rates for the different tiers of local government. Regions and counties typically have lower tax rates and tend to change them less frequently. Therefore, we take advantage of variations in the tax rate set at the municipal and/or EIMC level, if applicable.[6] We control for variations coming from counties and regions, using county-by-year fixed effects.

[4]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 continue to define their own tax rates, and the EIMC determines a rate that adds up to those of the municipalities. See Section 3.C.1 for more details, and for specificities of EIMCs Breuillé et al. (2018) and Tricaud (2025).

[5]In French, *Etablissement Public de Coopération Inter-Communale, EPCI*)

[6]More precisely, we take advantage of variations in the tax rate set at the municipality level if the municipality does not belong to an EIMC; in the sum of the municipal and inter-municipal tax rates when municipalities belong to an EIMC with additive tax regime and in the unique rate when the EIMC has a single tax regime. See Appendix Table 3.C.1

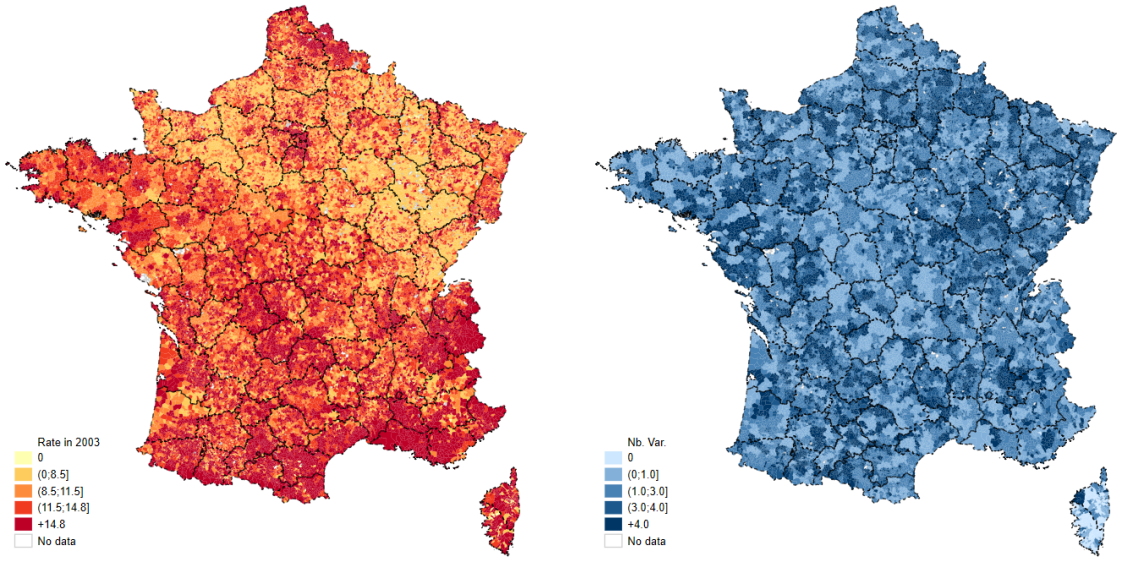
In 2010, a major reform fundamentally changed the design of the business tax. The previous system was replaced with a new contribution based solely on the value of properties and a fixed share of the value added by each establishment. Fixed tangible assets were no longer subject to taxation, and counties and regions ceased collecting the business tax altogether. As a result, our analysis focuses on the period from 2003 to 2008, which allows us to isolate the effects of tax rate changes while maintaining a consistent tax base. Additionally, during this timeframe, most municipalities joined an Establishment for Inter-Municipal Cooperation (EIMC), creating a relatively uniform institutional environment. By 2005, 88% of municipalities had joined an EIMC; in our estimation sample, 80% were already part of an EIMC at the beginning of the period of study, in 2003.

3.2.2 Municipal Data and Tax Rate Variation

We find local tax rates in the exhaustive administrative panel of French municipalities (*Recensement des Eléments d'Imposition* – REI). Observations are reported at the municipal level annually since 1993. This panel displays tax rates for all administrative divisions, including municipalities, EIMCs, counties, and regional levels, as well as the number of tax returns, tax base, and amount of collected taxes for each tier. Each municipality has a unique identifier. We harmonize it to that of 2010, fixing the geographical border to this period. Due to mergers, some municipal borders changed before 2010. As we cannot assign an exact tax rate to affected jurisdictions, we exclude all municipalities that underwent such mutations, which represent approximately 150 municipalities in the period of interest. In addition, we focus our study on the mainland territory, as the economy in overseas territories is substantially different from that of mainland France. Overall, we focus on 36,389 non-merged municipalities.

To estimate an event study with three leads and four lags over the period 2003-2008, we leverage variation in tax rates over the period 2000-2010. Over this period, on average, approximately 65.5% of municipalities change their tax rate each year (see Appendix Table 3.A.5). This is most of the time, a tax increase with a median (average) of +0.4 (+2.31) percentage points – departing from an average tax rate of 13.15%, resulting in a median tax increase of 3% (Appendix Table 3.A.4). As can be seen on the maps in Figures 3.2.2a and 3.2.2b, there

are substantial cross-sectional and temporal variations in BT rates during the period of interest (2000-2010). Most municipalities adjusted their tax rates at least once during the period, and no area appears to be more affected than others. In particular, we do not observe any clustering pattern along regions and county borders (highlighted in black).



(a) The Business Tax Rates in 2003 **(b) Number of Tax Increases**

Figure 3.2.2: Cross-Sectional and Time Variations in Business Tax Rates

Note: Panel (a) shows the tax rates in place in 2005 and Panel (b) the number of times the municipal/inter-municipal tax rate has been changed over the period 2000-2010. The black dotted lines highlight the county borders.

3.2.3 Firms Data

We retrieve information related to firms from two sources: the Linked Employer-Employee Data and the Business Tax Statements. The Linked Employer-Employee Data are based on the French annual declaration of social data at the job position level (known as *Déclaration Annuelle de Données Sociales* au niveau poste - DADS Postes). It is a mandatory procedure that all employers must fulfill. This dataset is an exhaustive panel covering the period from 1995 to 2022. This enables us to have information related to the labor force: gross and net wages, hours worked, occupation, and working period. We aggregate the panel at the establishment-year level thanks to a unique establishment identifier. We define the main workforce as the number of workers present in the establishment at the end of the year.[7] We then derive the

[7]We focus on workers with some attachment to the labor market ("posted 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

median hourly gross wage of employees, the related wage bill, and average hours worked per employee. We can also sort workers into different groups: according to their occupation or their wage. Specifically, we obtain the number of blue-collar and low-skilled workers at the plant level.[8] In this source, we also have information on the location of the establishment and its industry, enabling us to merge this panel with the panel of municipalities and identify the tax rate faced by firms.

In addition, we have access to the annual business tax statements for all establishments from 1999 to 2010. This database details the amount declared for each item of the tax base, as well as the final tax payment. In particular, we proxy the stock of capital – properties, machinery, and equipment – using the taxable value of each element. To evaluate the change in stock of movables (machinery and equipment) or properties, we use the taxable value reported in this dataset. We define the ratio of movable (resp. properties) as the ratio of the taxable value of movable (resp. properties) in period t over the sum of the taxable value of movable and properties reported in 2003.

3.2.4 Sample of Analysis and Descriptive Statistics

We construct a six-year panel of establishments using linked employer-employee data from 2003 to 2008. This period allows us to analyze the effects of a business tax change within a window spanning three years before and four years after the tax hike, which requires tax data from 2000 to 2010. By focusing on this timeframe, we avoid capturing the effects of the Great Recession and national tax base reforms (as underlined in Section 3.2.1).

To estimate the economic impact of business tax increases on the private sector in mainland France, we aim to track tax variations using a consistent sample of establishments. First, we restrict the dataset to municipalities that neither merged nor reduced their business tax rates over the study period (this represents a bit more than 12% of the municipalities, see Section

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).

[8]The occupation definition is the socio-professional classification (PCS) of the French National Statistics Office (*Institut National de la Statistique et des Études Économiques* - INSEE) at a 1-digit level. This classification comprises four main groups among active workers: executives and higher intellectual professions (Class 3), intermediary professions (Class 4), employees (Class 5), and blue-collar workers (Class 6). We pool classes 3 and 4 under the category of high-skilled workers and group classes 5 and 6 together as low-skilled workers, following the approach of Carbonnier et al. (2022) and Caliendo et al. (2015).

3.2.2). Second, we include only establishments that were continuously active from 2003 to 2008. This ensures that observed changes can be attributed to variations in tax rates, while also excluding potential confounding effects from the Great Recession. In addition, to measure meaningful variations in terms of employment and wages, we further restrict the sample to establishments with at least three employees in every period. We also ensure that firms use capital in their production and are therefore liable.

This final sample comprises 610,470 observations, covering 101,745 establishments (belonging to 89,192 different firms) located in 11,032 municipalities across mainland France.[9] The median tax increase is 0.39 percentage points, departing from an average tax rate of 12.83%, resulting in a change of about 3%. On average, municipalities increased the tax rate about 7 times between 2000 and 2010, and half of them at least 8 times (see Appendix Table 3.A.6 for detailed variations each year). Figure 3.2.3 displays the distribution of the tax rate increases across municipalities in the sample. We note one huge spike on the left-hand side, as most tax increases are less than 2 percentage points. We observe a second "bump" of around a 10 percentage point increase; this is due to the 2010 reform, which only allowed municipalities to levy the business tax, thereby further increasing the rate. This large variation is not a concern, as it mainly affects pre-trends. Robustness checks further show that our point estimates remain unchanged when the window of estimation is smaller, therefore excluding the variations induced by this reform.

[9] In 2003, this sample accounts for 20.7% of the French workforce, 7.8% of plants, and 17.4% of the local business tax revenue. Municipalities included in the sample encompass about 38% of the French population on the mainland.

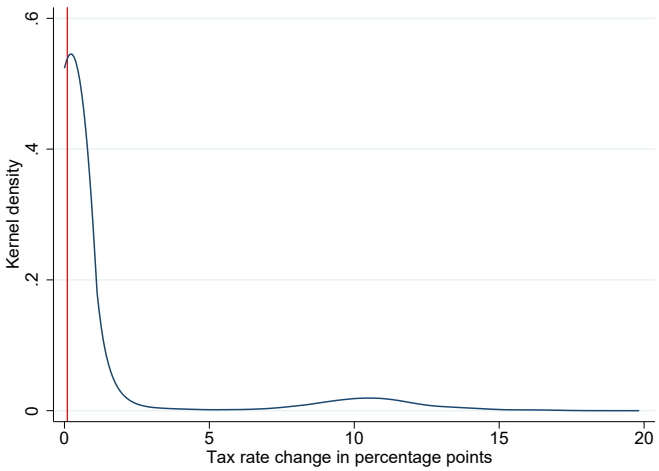


Figure 3.2.3: Distribution business tax increases

Note: This figure displays the kernel distribution of tax hikes over the period 2000-2011, used in the estimations. The vertical red line highlights the median (0.39).

Appendix Table 3.A.7 reports descriptive statistics related to the firms. We note that the average firm in the sample of analysis has about 23 employees. 35% of the firms are in the Trade sector, 22% in the Manufacturing sector, and 18% in the Construction sector. 72% of the establishments are single-plant firms. Most of the workforce is low-skilled and earns more than the minimum wage. Firms own movable and immovable properties of a taxable value of about 70,000 and 13,000 euros on average and pay a local business tax of about 21,000 euros.

3.3 Empirical Strategy

This section presents our empirical strategy, leveraging staggered local tax increases and using an event study design, similar to Fuest et al. (2018) and Lichter et al. (2025). Section 3.3.1 specifies the model of estimation. Section 3.3.2 discusses the assumption required to identify the causal effect of tax increases on firms' outcomes.

3.3.1 Model Specification

To causally estimate the effects of the business tax on labor and capital, we take advantage of the multiple and staggered changes of the cumulated tax rate defined by the municipalities and/or EIMC when applicable (see Section 3.2.1). First, we base our analysis on an event

study approach, which enables us to check for the absence of a pre-trend and estimate the medium-term effects of a tax increase. This approach allows us to use all tax increases within municipalities available in our sample and treat them as independent events. More precisely, we use a classical two-way fixed effect event study model with binned endpoints (following Schmidheiny and Siegloch, 2023), assuming that pre- and post-treatment effects are constant beyond the endpoints. Formally, the model is given by:

$$\log(y_{i,t}) = \sum_{\ell=-3}^{4} \beta_\ell I^\ell_{m(i),t} + \delta_i + \zeta_{d(i),t} + \xi_{z(i),t} + \varepsilon_{it} \tag{3.1}$$

where $I^\ell_{m(i),t}$ is the treatment variable capturing leads and lags of changes in the tax rate ($\tau_{m(i),t-\ell}$) where the firm is located. More precisely, $I^\ell_{m(i),t}$ is defined as follows:

$$I^\ell_{m,t} = \begin{cases} \sum_{s=t+2}^{2010} \mathbb{1}[\Delta\tau_{m(i),s} > 0] \cdot \Delta\tau_{m(i),s} & \text{if} \quad l = -3 \\ \mathbb{1}[\Delta\tau_{m(i),t-\ell} > 0] \cdot \Delta\tau_{m(i),t-\ell} & \text{if} \quad -3 < l < +4 \\ \sum_{s=2000}^{t-3} \mathbb{1}[\Delta\tau_{m(i),s} > 0] \cdot \Delta\tau_{m(i),s} & \text{if} \quad l = +4 \end{cases} \tag{3.2}$$

The event window contains three periods before and four periods after the tax hike in our baseline estimate. We control for establishment fixed effects (δ_i). We do not need to control for municipal fixed effects as establishment identifiers are tied to the location of the plant.[10] We also control for local shocks using local-labor-market-by-year ($\xi_{z(i),t}$) and county-by-year fixed effects ($\zeta_{d(i),t}$). The county-by-year fixed effects allow us to control for potential changes in tax rates at the county or regional level. These sets of fixed effects limit our identification to variation at the local labor market level. $\varepsilon_{i,t}$ denotes the error term. We cluster standard errors at the municipality level, accounting for potential correlations across plants within municipalities. This model has the advantage of accounting for the multiple tax changes per municipality over the period.

When investigating potential heterogeneity with respect to firms' characteristics, we fully interact the model (3.1) with the characteristic of interest. As some characteristics, such as the size of the firm, can be endogenous to tax reforms, we use the initial value of such attributes in

[10] Firm identifiers are unique and made of a *siren* (first nine figures) and a *nic* (last five figures). Every time the establishment relocates, the *nic* changes. Therefore, the municipality and establishment FE are collinear.

2003. To estimate the average magnitude of the effect of business tax increases on the outcomes of interest, we use the following generalized Differences-in-Differences model:

$$\ln(y_{e(m),t}) = \delta \ln(1 - \tau_{m(i),t}) + \delta_i + \zeta_{d(i),t} + \xi_{z(i),t} + \varepsilon_{it} \tag{3.3}$$

δ estimates the percentage change in the outcome of interest due to a one percent decrease in the net of tax.

3.3.2 Identification

To interpret estimates causally, we have to assume that after absorbing labor market and county time-varying shocks, tax changes are not systematically correlated with remaining trends in local factors that also affect firms' outcomes. The key identification assumption in this differences-in-differences type of regression is that in the absence of a tax increase, firms would have evolved in a similar manner. While this assumption cannot be directly tested, we assess its plausibility using the pre-reform period. Reassuringly, we cannot reject the presence of differential trends in most specifications.

However, de Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Sun and Abraham (2021), and Borusyak et al. (2024) have highlighted that this kind of event study model (Eq. 3.1) – two-way fixed effects models – can provide biased estimates when the treatment is staggered. Given that tax hikes occur at different points in time in our setting, we use different estimators addressing this potential issue to test for potential heterogeneous treatment effects across cohorts. The different estimators report similar point estimates to our baseline, see Section 3.5.1.

Furthermore, the recent work of de Chaisemartin and d'Haultfoeuille (2020) and Callaway et al. (2024) has highlighted that differences-in-differences estimates with continuous treatment effects could be biased. In our setting, we have continuous treatments as the tax hikes firms face differ across time and municipalities. This implies that we need to rely on the continuous dose assumption. This assumption implies that the treatment effect is linear in nature. Increasing the tax by 0.1 has the same effect regardless of whether the initial rate is 10% or 15%. However, if this assumption does not hold in our setting, our point estimates may be biased.

3.4 Results

Section 3.4.1 investigates firms' responses in terms of employment, wages, and investment. Section 3.4.2 then explores potential heterogeneity in the sample.

3.4.1 Baseline Effects

Increase in tax payment. In Figure 3.4.1, we plot the coefficients β_l resulting from the estimation of the model (3.1). We normalize the coefficients and set β_{-1} to 0. Evaluating the pre-period coefficients (F3-F1), we note that the pre-trend is slightly increasing, and the coefficient in period F3 is significant. However, we observe a clear break in trend between the pre-reform and post-reform periods. The event study suggests that following a one percentage point tax increase, the tax payment increases by approximately 2%, resulting in an elasticity to the net of tax of -2.26, as shown in Appendix Table 3.A.8. It is important to note that the tax paid only encompasses the business tax.

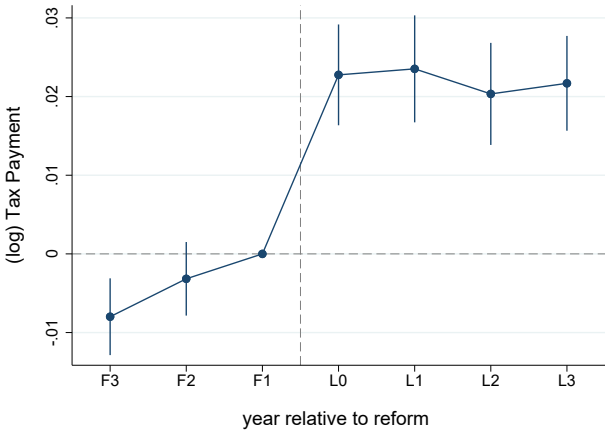


Figure 3.4.1: Effect on Tax Payment

Note: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (3.1). The dependent variable is the log of tax payment. The vertical line highlights the implementation of the reform.

No effect on capital. Figure 3.4.2 reports the effect of a business tax hike on the stock of capital. We note the absence of a significant pre-trend. Following the reform, we do not detect any adjustments in terms of capital. We verify that this effect does not hide heterogeneity across different types of assets by investigating the effect on movable and immovable properties

separately, see Figures 3.B.2a and 3.B.2b. We also do not detect any adjustment for any of those assets. Therefore, despite the fact that the tax base is almost entirely dependent on the stock of capital, firms do not adjust their capital to counterbalance the increase in the tax rate. Given the irreversibility of capital investment, we investigate whether firms adjust employment and wages instead, which may offer greater flexibility.

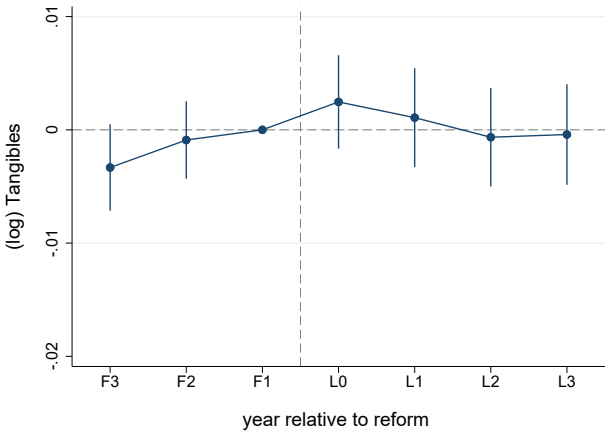
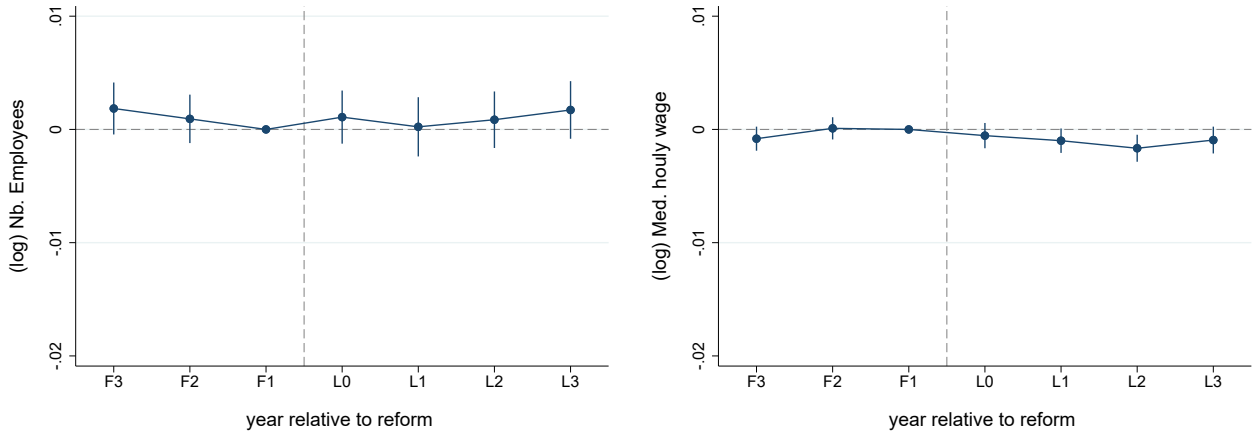


Figure 3.4.2: Effect on the Stock of Capital

Note: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (3.1). The dependent variable is the log of tangible assets. The vertical line highlights the implementation of the reform.

No effect on the workforce. Figure 3.4.3a displays the baseline estimates of the effect of business tax hikes on employment, and Figure 3.4.3b on the median hourly wage. On both figures, we observe the absence of a significant trend in the pre-reform period. Figure 3.4.3a shows that following a business tax increase, there is no employment adjustment. We observe a slight reduction in the median hourly wage; the coefficients for periods L1 and L3 are marginally significant, while that for period L2 is significantly different from 0. However, this slight reduction does not pass the differences-in-differences specification, see in Appendix 3.A Table 3.A.8. In addition, we do not find any effect on the wage bill (see in Appendix 3.B Figure 3.B.1a). This absence of change in the wage bill could also be compensated for by an increase in hours worked. However, Figure 3.B.1b in Appendix 3.B shows that if anything, the number of hours worked has reduced. These findings, taken together, suggest that the estimated effect on median hourly wages is minimal and may be due to noise. Therefore, the workforce does not appear to be significantly affected by local business tax hikes.



(a) Effect on Employment **(b) Effect on Median Hourly Wage**

Figure 3.4.3: Effect on the Workforce

Note: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (3.1). The dependent variable in Panel (a) is the log number of employees, and in Panel (b), the log of the median hourly wage. The vertical line highlights the implementation of the reform.

Despite a significant increase in tax payment, firms do not adjust wages, employment, or capital investment that is directly subject to the local business tax. These results are rather at odds with the literature. In the next section, we verify that these baseline effects do not hide heterogeneous effects across firms' characteristics.

3.4.2 Absence of Heterogeneity

As more capital-intensive firms are more affected by a tax variation – their tax base is more substantial – we investigate the effect of tax hikes on firms' outcomes by capital intensity. We measure the capital intensity of firms using the ratio of the gross wage bill to the stock of tangible assets, measured in 2003. We then categorize firms as capital-intensive or labor-intensive based on the median value of this ratio. We detect differences in tax payment, which is mechanical, but do not make adjustments in capital or labor across the different groups. Estimates are reported in Appendix Figure 3.B.3. We note that this time the effect on the wage bill is significantly negative for capital-intensive firms. However, this does not remain significant when estimating elasticities (see Table 3.A.9 in Appendix 3.A).

The tax burden of the local business tax is also larger for smaller firms, as this accounts for a larger share of their revenue. Therefore, we investigate the effect of the tax hikes by firm size.

We define a firm's size by the number of employees in 2003 or the turnover reported in that year. We do not detect significant heterogeneity along this characteristic (see in Appendix 3.A Tables 3.A.10 and 3.A.11).

Furthermore, multi-establishment firms with more resources and shifting opportunities could react more strongly than single-establishment firms. To explore this potential heterogeneity, we categorize establishments into single-plant or multi-plant firms and interact the baseline model of estimation with a dummy variable indicating whether an establishment belongs to a multi-plant firm. However, we do not detect any significant differences between the two groups (see Appendix Table 3.A.12). We also report point estimates by industries but do not find any heterogeneity (see Appendix Table 3.A.13).

3.5 Robustness and Discussion

Section 3.5.1 tests the robustness of these null effects. Section 3.5.2 discusses the results and suggests plausible explanations.

3.5.1 Robustness

We run a set of robustness checks. Our baseline specification controls for all time-variant characteristics at the local labor market level by interacting local market dummies with year fixed effects (LM $\times$ year). It enables us to discard any time-variant local characteristics that simultaneously affect business tax increases and plant economic activity. To test the robustness of our results, we estimate the same event study model with two different specifications: one that controls for all time-variant characteristics at the regional level and another that controls for industry-specific trends at the 2-digit industry level. As shown in Appendix 3.B Figure 3.B.4, our main results remain robust to these new specifications.

As Differences-in-differences models often provide biased standard errors (Bertrand et al., 2004), we further assess the robustness of our results by changing the level of clustering of standard errors (as suggested by Angrist and Pischke, 2009). In our baseline estimation, we cluster standard errors at the municipality level. We test two other specifications: one at the

county level and another at the local labor market level. Appendix Figure 3.B.5 confirms that our point estimates are not affected.

We also check the robustness of our results with respect to the event study window, varying the number of leads and lags. Reducing the number of leads also enables us to confirm that our baseline results are not affected by the inclusion of 2010, year of a major reform of the business tax (detailled in Section 3.2.4) In Appendix 3.B, Figure 3.B.6 displays the results when estimating the model with different windows. We observe that pre-trends remain flat and non-significant, and we still do not detect any significant adjustments in firms' inputs.

Our identification relies on multiple tax reforms and a traditional event-study approach – a two-way fixed-effects model. As these may provide biased estimates in the case of heterogeneous treatment effects across cohorts, we use the estimators provided by de Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024). To properly implement such estimators, we also restrict the baseline sample to establishments in municipalities that experienced at most one increase over the period. This methodology requires us to limit the study window to 2003-2007 and use only three leads and two lags to ensure sufficient observations in both the treatment and control groups. In Appendix 3.B, Figure 3.B.7 displays the resulting estimates with the baseline model adjusted for the window of the event as well as the estimates resulting from the estimators of de Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024). Point estimates obtained with the different estimators are all within the same ballpark and provide similar conclusions: the tax hike effectively generates an increase in tax payment but does not result in any input adjustments. It is, therefore, unlikely that our estimates are driven by heterogeneous treatment effects across cohorts.

3.5.2 Discussion

In contradiction with existing literature. Our results are rather at odds with the existing literature. Recent evidence suggests that following a tax hike, wages, employment, and capital investment typically decrease (e.g. Suárez Serrato and Zidar, 2016; Fuest et al., 2018; Giroud and Rauh, 2019; Link et al., 2024). One could argue that the business tax used in the literature

is usually based on profit rather than on capital, as is the case in our setting. However, Simula and Trannoy (2009) shows that the French local business tax also affects the user cost of capital and, if anything, is more distortive than a classical business tax based on profit. Evidence based on non-residential property – potentially closer to our setting – also reports a negative effect on employment (Duranton et al., 2011). It is, however, possible that the business tax we study generates different frictions in the firms' decision-making process and therefore has a different effect, as suggested by Chen et al. (2023).

Multiplicity and Salience of the tax changes. We noted that municipalities frequently adjust their local tax rates in Section 3.2.2. Within the estimation sample, 50% of municipalities increased their tax rate at least eight times between 2000 and 2010, implying that tax hikes occurred more often than every other year. These repeated adjustments result in almost systematic changes in taxation, suggesting that tax policy is a regularly used tool at the municipal level. Notably, most of these increases are relatively modest in size, with the majority being below 0.20 percentage points, as shown in Appendix Table 3.A.7. This pattern suggests a form of incremental tax policy, where municipalities fine-tune their tax rates rather than overhaul them. This is potentially due to the harmonization phase that follows when municipalities join an EIMC (see Section 3.C.1 for more details).

From the perspective of firms, such a pattern may lead to the expectation that tax changes are both frequent and moderate, potentially reducing uncertainty around local fiscal policy. As a result, firms may view these tax adjustments as part of the normal policy environment and may be less likely to drastically alter investment or location decisions in response to any single tax change. However, when leveraging only the largest tax increase, we also do not detect further adjustment, see Appendix Figure 3.B.8. Anecdotal evidence also suggests that businesses are not systematically notified of changes in tax rates. Instead, they typically become aware of such changes when they receive their tax notices. Although local media may report on voted rates, these often diverge from the effective tax rates due to the gradual convergence of rates across municipalities that choose to coordinate and join an EIMC (see Section 3.C.1 for further details). In some cases, firms may subsequently be informed of these changes by

their accounting or legal advisors.

In addition, the French local business tax has often been strongly criticized by Prime Ministers or Presidents, generating uncertainty about its future and the necessity for adjustments in response to tax rate changes.[11] The strong criticisms have been translated into several national – more substantial – reforms attempting to reduce the burden of the tax: in 1999, the phasing out of the wage bill (see Chapter 2 for more details); the introduction of the investment exoneration (detailed in Section 3.2.1) and changes in the cap of tax payment introduced in 2007 (see Section 3.C.3 for more detail).

3.6 Conclusion

Our analysis provides robust evidence that local business tax increases in France, despite significantly raising firms' tax liabilities, do not lead to measurable adjustments in either capital or labor inputs. Firms do not reduce their capital stock, reallocate assets, or alter investment behavior, even though the tax is levied almost entirely on capital. The lumpiness of investment could explain this absence of adjustment, as the window of study is relatively limited, and capital might take time to adjust. On the labor side, we similarly find no adjustment either – employment levels, wages, and hours worked remain stable, suggesting a limited role for labor-related margins in absorbing the increased tax burden.

These findings hold across a range of firm characteristics, including capital intensity, size, and revenue, indicating that the absence of responses is not driven by firm heterogeneity. The results suggest the rigidity of capital and labor adjustments in the short to medium term, likely due to factors such as capital irreversibility, fixed adjustment costs, or institutional constraints.

From a policy perspective, our findings suggest that local business taxes, while effective in raising revenue, may not distort firm input decisions as much as standard models might predict, at least in the short term. This could have important implications for the design and evaluation of subnational tax policy, particularly in contexts where concerns about capital flight or employment losses are central to the policy debate.

[11] President Francois Mitterand – said in 1983 that "This [...] tax is terribly unfair, inefficient, [...] it works against employment, it works against exports". Nicolas Sarkozy underlined in 2009 that "For the last 35 years, we have known that the [...] tax that dampens investments is an economic aberration".

3.A Additional Tables

Table 3.A.1: Average Municipality

	Mean	SD	Min.	Max.	p25	p50	p75
Population	1,657.6	12,212.14	0	2,215,197	181	396	981
EIMC *(share of Mun.)*	0.83	0.38	0.00	1.00	1.00	1.00	1.00
Business tax rate	23.05	6.53	0	91.40	18.54	22.37	26.83
Property tax rate on dev. land	29.81	8.12	7.40	86.80	24.10	29.20	34.60
Property tax rate on undev. land	85.31	41.52	11.50	507.50	59.80	74.50	99.30
Observations							400,279

Note: This table shows the mean, Standard deviation (SD), minimum and maximum values, as well as the 25th (p25), 50th (p50), and 75th (p75) percentiles of non-merged municipalities over the period 2000-2010. The tax rates summarized here are the cumulative rates, summing municipal/EIMC, county, and regional rates. The rates are reported in percentages.

Table 3.A.2: Average Municipality in the Sample of Estimation

	Mean	SD	Min.	Max.	p25	p50	p75
Nb. Employees	218.94	1019.33	3.00	54004.00	10.00	30.00	116.00
Nb. Establishments	9.22	43.83	1.00	2793.00	1.00	2.00	7.00
Population	2099.97	10491.26	26.00	860363.00	399.00	784.00	1655.00
EIMC *(share of Mun.)*	0.80	0.40	0.00	1.00	1.00	1.00	1.00
Business tax rate	22.61	5.98	6.60	91.40	18.32	21.91	26.10
Property tax rate on dev. land	29.46	8.10	7.40	86.80	23.70	28.80	34.30
Property tax rate on undev. land	84.42	39.52	11.50	507.50	60.10	74.50	97.10
Observations							66,192

Note: This table shows the mean, Standard deviation (SD), minimum and maximum values, as well as the 25th (p25), 50th (p50), and 75th (p75) percentiles of municipalities in the sample of estimations over the period 2003-2006. The tax rates summarized here are the cumulative rates, summing municipal/EIMC, county, and regional rates. The rates are reported in percentages.

Table 3.A.3: Local Business tax Rates by Administrative Division

	Mean	SD	Min.	Max.	p25	p50	p75	N
Municipality/EIMC	13.15	6.21	0	80.53	8.93	12.22	16.39	36,389
+ *County*	8.27	2.40	0	16.2	6.9	7.8	9.30	96
+ *Region*	2.62	0.71	0	4.3	2.20	2.50	3.10	22
= **Business tax**	23.05	6.53	0	91.4	18.54	22.37	26.83	36,389

Note: This table shows the mean, Standard deviation (SD), minimum and maximum values, as well as the 25th (p25), 50th (p50), and 75th (p75) percentiles of the local business tax rates at the different administrative divisions (Municipality, County, and Regions) over the period 2000-2010. The rates are reported in percentages.

Table 3.A.4: Average Variations Used for Identification

	Variation | increase			Av.Rate	Share of Mun.
	Av. Var.	Median Var.	Std.Dev		with $\Delta \geq 0$
Business Tax	0.46	0.30	0.55	15.09 %	69.68%

Note: This table reports the average and the median tax increase used in the difference-in-differences and event study regression. The average tax rate in the baseline sample is 15.09% and 69.98% of the municipalities in the baseline sample increased their tax rate.

Table 3.A.5: Average Tax Variation in BT across Non-merged Municipalities

	All Variations				Increases				Decreases			
	Share	Average	Median	Nb. Mun.	Share	Average	Median	Nb. Mun.	Share	Average	Median	Nb. Mun.
Average	65.5	1.75	.26	400,279	53.3	2.31	.4	400,279	12.2	-.7	-.33	400,279
2000	39.3	.07	.1	14,287	27.8	.64	.21	10,121	11.4	-1.31	-.58	4,166
2001	46.9	.2	.2	17,080	34.8	.67	.3	12,680	12.1	-1.16	-.5	4,400
2002	71.2	.86	.4	25,911	59.5	1.18	.59	21,666	11.7	-.78	-.36	4,245
2003	68.2	.32	.2	24,828	53.6	.59	.31	19,521	14.6	-.67	-.33	5,307
2004	68.8	.28	.2	25,043	54.9	.5	.3	19,993	13.9	-.58	-.31	5,050
2005	71.8	.25	.2	26,121	57.3	.45	.28	20,868	14.4	-.54	-.29	5,253
2006	68.9	.24	.2	25,078	54.5	.45	.27	19,835	14.4	-.52	-.29	5,243
2007	62.6	.17	.14	22,780	47.2	.39	.23	17,165	15.4	-.51	-.29	5,615
2008	60.1	.18	.15	21,858	46.3	.39	.23	16,844	13.8	-.52	-.29	5,014
2009	64.2	.3	.2	23,377	52.8	.47	.3	19,196	11.5	-.48	-.28	4,181
2010	98.9	10.81	10.74	36,006	98	10.94	10.76	35,656	1	-2.8	-2.23	350

Note: This table shows the frequency, sign, and size of the local business tax changes – cumulated up to the EIMC level, among non-merged municipalities. The share is reported in percentage terms, and the average and median changes in the business tax rate are reported in percentage points. The first row is the average over the period 2000-2010.

Table 3.A.6: Average Tax Increases in the Sample of Estimation

	Share	Average	Median	Nb. Mun.
Average	65.3	1.92	.39	121,352
2000	35	.55	.2	3,857
2001	46	.51	.3	5,076
2002	71.8	.83	.45	7,923
2003	70.2	.52	.34	7,745
2004	70.6	.48	.32	7,786
2005	72.9	.44	.3	8,037
2006	68.4	.43	.3	7,544
2007	60.7	.37	.26	6,693
2008	57.9	.36	.26	6,393
2009	64.9	.45	.32	7,155
2010	100	10.71	10.6	11,031

Note: This table shows the frequency, sign, and size of the local business tax increases – cumulated up to the EIMC level, used in the estimations. The share is reported as a percentage, and the average and median changes in the business tax rate are reported in percentage points. The first row is the average over the period 2000-2010.

Table 3.A.7: Characteristics of Establishments in the Sample of Estimation

	(1) **Mean**	(2) **SD**	(3) **p50**
Nb. of employees	23.74	99.68	8
Median hourly gross wage	13.52	120.97	12.31
Gross wage bill	660,239.51	3.80e+06	185,147.56
Property	13,655.04	21,388.17	4,597.61
Movable	69,877.70	98,025.02	26,874.26
Movable ratio	1.91	46.96	0.87
Property ratio	0.35	6.21	0.18
Tax payment	21,477.19	30,359.76	8,352.50
Sales	4,881.67	6,384.60	1,467.27
Local BT rate (in %)	15.09	4.97	15.05
Change in BT rate (in pp)	0.46	0.30	0.55
Nb. tax hikes	7.39	2.93	8
Types of workers *(Share of workers)*			
MW earners	0.11	0.17	0.01
High Skilled	0.27	0.27	0.20
Low Skilled	0.71	0.27	0.78
Worker	0.44	0.34	0.46
Manager	0.09	0.15	0
Intermediate	0.18	0.20	0.13
Clerk	0.27	0.28	0.17
Industries *(Share of plants)*			
Manufacturers	0.22	0.42	0
Construction	0.18	0.38	0
Trade	0.35	0.48	0
Transport	0.06	0.24	0
Hospitality	0.08	0.27	0
Information & Comm.	0.02	0.12	0
Real Estate	0.02	0.12	0
Specialized Activities	0.05	0.21	0
Administrative Services	0.03	0.17	0
Firm size *(Share of plants)*			
Single-plant	0.72	0.45	1
[3;9] Employees	0.56	0.50	1
[10;19] Employees	0.21	0.41	0
[20;49] Employees	0.14	0.35	0
50+ Employees	0.08	0.27	0
Observations			610,470

Note: This table reports the mean in column (1), standard deviations in column (2), and the median in column (3) of firms' characteristics in the sample of estimation. All monetary values are reported in 2010 euros. The local BT rate is the cumulative rate up to the EIMC level, which is used in the regressions. The types of workers, industries, and firms' sizes are reported as shares.

Table 3.A.8: Differences-in-Differences Estimates

	(1)	(2)	(3)
	(log) Tax Payment	(log) Nb. Employees	(log) Med. houly wage
$\ln(1-\tau_{m(i),t})$	-2.264***	0.022	0.055
	(0.191)	(0.082)	(0.035)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Obs. 610,128	610,196	610,196	
R2	0.947	0.965	0.869
	(1)	(2)	(3)
	(log) Av. hourly gross wage	(log) Gross wage bill	(log) Av. annual hours per employee
$\ln(1-\tau_{m(i),t})$	0.064	0.136	0.050
	(0.041)	(0.105)	(0.038)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.907	0.969	0.711
	(1)	(2)	(3)
	(log) Tangibles	Movables ratio	Property ratio
$\ln(1-\tau_{m(i),t})$	-0.162	-11.453	0.010
	(0.144)	(12.272)	(1.695)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.946	0.397	0.394

Note: This table reports the point estimates obtained with the generalized difference-in-differences model (Eq. 3.3). The standard errors are displayed in parentheses and are clustered at the municipality level. Coefficients measure the effect of a reduction in 1% in the net of tax. The dependent variable is the log of tax payment (1), the log of the number of employees (2), the log of the median hourly wage (3), the log of the average hourly gross wage (4), the log of the gross wage bill (5), the log of the number of hours worked per employee (6), the log of tangible assets (7), the movable ratio (8) and the property ratio (9). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 3.A.9: Differences-in-Differences Estimates by Capital Intensity

	(1)	(2)	(3)
	(log) Tax Payment	(log) Nb. Employees	(log) Med. houly wage
Capital Intensive	-1.737***	0.075	0.016
	(0.197)	(0.131)	(0.044)
Labor Intensive	-2.407***	-0.034	0.080*
	(0.254)	(0.114)	(0.048)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Capital intens.xFE	✓	✓	✓
Obs.	610,128	610,196	610,196
R2	0.949	0.965	0.869
equality of coefficients	.019	.668	.335
	(4)	(5)	(6)
	(log) Av. hourly gross wage	(log) Gross wage bill	(log) Av. annual hours per employee
Capital Intensive	0.070*	0.176	0.032
	(0.042)	(0.125)	(0.056)
Labor Intensive	0.040	0.057	0.052
	(0.052)	(0.123)	(0.056)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Capital intens.xFE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.907	0.969	0.711
equality of coefficients	.544	.360	.804
	(7)	(8)	(9)
	(log) Tangibles	Movables ratio	Property ratio
Capital Intensive	0.355**	-7.709	-0.718
	(0.149)	(7.104)	(1.005)
Labor Intensive	-0.239	-10.883	1.080
	(0.210)	(18.377)	(2.513)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Capital intens.xFE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.948	0.397	0.394
equality of coefficients	.003	.816	.329

Note: This table reports the point estimates obtained with the generalized difference-in-differences model (Eq. 3.3) fully interacted with a dummy variable indicating whether a plant is capital intensive. The standard errors are displayed in parentheses and are clustered at the municipality level. Coefficients measure the effect of a reduction of 1% in the net of tax. The line "equality of coefficients" reports the p-value of the test evaluating whether the difference between the coefficients of the two groups is statistically significant. The dependent variable is the log of tax payment (1), the log of the number of employees (2), the log of the median hourly wage (3), the log of the average hourly gross wage (4), the log of the gross wage bill (5), the log of the number of hours worked per employee (6), the log of tangible assets (7), the movable ratio (8) and the property ratio (9). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 3.A.10: Differences-in-Differences Estimates by Firm Size

	(1)	(2)	(3)
	(log) Tax Payment	(log) Nb. Employeess	(log) Med. houly wage
3 to 9 Employees	-2.499***	-0.007	0.090*
	(0.235)	(0.102)	(0.044)
10 to 19 Employees	-2.447***	-0.100	0.032
	(0.256)	(0.116)	(0.056)
20 to 49 Employees	-2.191***	-0.187	-0.020
	(0.454)	(0.144)	(0.073)
50+ Employees	-1.491***	0.052	-0.013
	(0.455)	(0.221)	(0.070)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Firm SizexYear FE	✓	✓	✓
Obs.	610,128	610,196	610,196
R2	0.947	0.966	0.869
	(4)	(5)	(6)
	(log) Av. hourly gross wage	(log) Gross wage bill	(log) Av. annual hours per Employees
3 to 9 Employees	0.104**	0.118	0.021
	(0.047)	(0.127)	(0.051)
10 to 19 Employees	-0.030	-0.017	0.113
	(0.061)	(0.151)	(0.069)
20 to 49 Employees	0.016	-0.159	0.011
	(0.084)	(0.154)	(0.067)
50+ Employees	0.000	0.214	0.162**
	(0.071)	(0.210)	(0.070)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Firm SizexYear FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.907	0.969	0.711
	(7)	(8)	(9)
	(log) Tangibles	Movables ratio	Property ratio
3 to 9 Employees	-0.322**	-5.489	1.068
	(0.166)	(12.350)	(1.786)
10 to 19 Employees	-0.256	-45.797	-3.965
	(0.210)	(29.629)	(3.857)
20 to 49 Employees	-0.014	13.626	0.422
	(0.241)	(14.165)	(2.123)
50+ Employees	-0.112	-12.336	2.631
	(0.285)	(11.057)	(1.841)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Firm SizexYear FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.946	0.397	0.394

Note: This table reports the point estimates obtained with the generalized difference-in-differences model (Eq. 3.3) fully interacted with firms' size. The standard errors are displayed in parentheses and are clustered at the municipality level. Coefficients measure the effect of a reduction of 1% in the net of tax. The dependent variable is the log of tax payment (1), the log of the number of employees (2), the log of the median hourly wage (3), the log of the average hourly gross wage (4), the log of the gross wage bill (5), the log of the number of hours worked per employee (6), the log of tangible assets (7), the movable ratio (8) and the property ratio (9). ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 3.A.11: Differences-in-Differences estimates by Firm Sales

	(1)	(2)	(3)
	(log) Tax Payment	(log) Nb. Employees	(log) Med. houly wage
Q1 Sales	-2.058***	0.137	-0.002
	(0.324)	(0.124)	(0.055)
Q2 Sales	-2.779***	-0.055	0.019
	(0.277)	(0.118)	(0.052)
Q3 Sales	-2.617***	-0.141	0.063
	(0.253)	(0.107)	(0.052)
Q4 Sales	-2.196***	0.164	0.062
	(0.233)	(0.130)	(0.050)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Q SalesxYear FE	✓	✓	✓
Obs.	601,847	601,914	601,914
R2	0.947	0.965	0.869
	(4)	(5)	(6)
	(log) Av. hourly gross wage	(log) Gross wage bill	(log) Av. annual hours per employee
Q1 Sales	-0.004	0.218	0.085
	(0.059)	(0.171)	(0.074)
Q2 Sales	0.021	-0.001	0.034
	(0.056)	(0.153)	(0.069)
Q3 Sales	0.066	-0.032	0.043
	(0.054)	(0.130)	(0.047)
Q4 Sales	0.056	0.181	-0.039
	(0.054)	(0.143)	(0.051)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Q SalesxYear FE	✓	✓	✓
Obs.	601,914	601,914	601,914
R2	0.907	0.969	0.711
	(7)	(8)	(9)
	(log) Tangibles	Movables ratio	Property ratio
Q1 Sales	-0.203	5.260	0.549
	(0.250)	(19.435)	(1.207)
Q2 Sales	-0.471*	-16.179*	0.472
	(0.243)	(9.873)	(1.302)
Q3 Sales	-0.316	-33.857	-0.813
	(0.203)	(15.077)	(4.390)
Q4 Sales	-0.197	-4.230	0.116
	(0.210)	(10.054)	(1.341)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Q SalesxYear FE	✓	✓	✓
Obs.	601,914	601,914	601,914
R2	0.945	0.396	0.392

Note: This table reports the point estimates obtained with the generalized difference-in-differences model (Eq. 3.3) fully interacted with the firms' sales quartile rank in 2003. The standard errors are displayed in parentheses and are clustered at the municipality level. Coefficients measure the effect of a reduction of 1% in the net of tax. The dependent variable is the log of tax payment (1), the log of the number of employees (2), the log of the median hourly wage (3), the log of the average hourly gross wage (4), the log of the gross wage bill (5), the log of the number of hours worked per employee (6), the log of tangible assets (7), the movable ratio (8) and the property ratio (9). Firms are sorted by size based on their sales quartile rank in 2003. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 3.A.12: Differences-in-Differences Estimates by Single-plant

	(1)	(2)	(3)
	(log) Tax Payment	(log) Nb. Employees	(log) Med. houly wage
Multi-plant	-2.442***	0.039	0.102**
	(0.237)	(0.129)	(0.049)
Single-plant	-2.270***	-0.008	0.030
	(0.196)	(0.082)	(0.038)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Single-plant x FE	✓	✓	✓
Obs.	610,128	610,196	610,196
R2	0.947	0.965	0.869
equality coef.	.3842	.7898	.1395
	(4)	(5)	(6)
	(log) Av. hourly gross wage	(log) Gross wage bill	(log) Av. annual hours per employee
Multi-plant	0.095*	0.154	0.020
	(0.058)	(0.1485	(0.051)
Single-plant	0.042	0.104	0.053
	(0.042)	(0.108)	(0.042)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Single-plant x FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.907	0.969	0.711
equality coef.	.3131	.6901	.5214
	(7)	(8)	(9)
	(log) Tangibles	Movables ratio	Property ratio
Multi-plant	-0.259	-2.711	1.960
	(0.229)	(9.306)	(2.944)
Single-plant	-0.210	-14.854	-0.596
	(0.155)	(14.386)	(1.651)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
Single-plant x FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.946	0.397	0.394
equality coef.	.8414	.2712	.3247

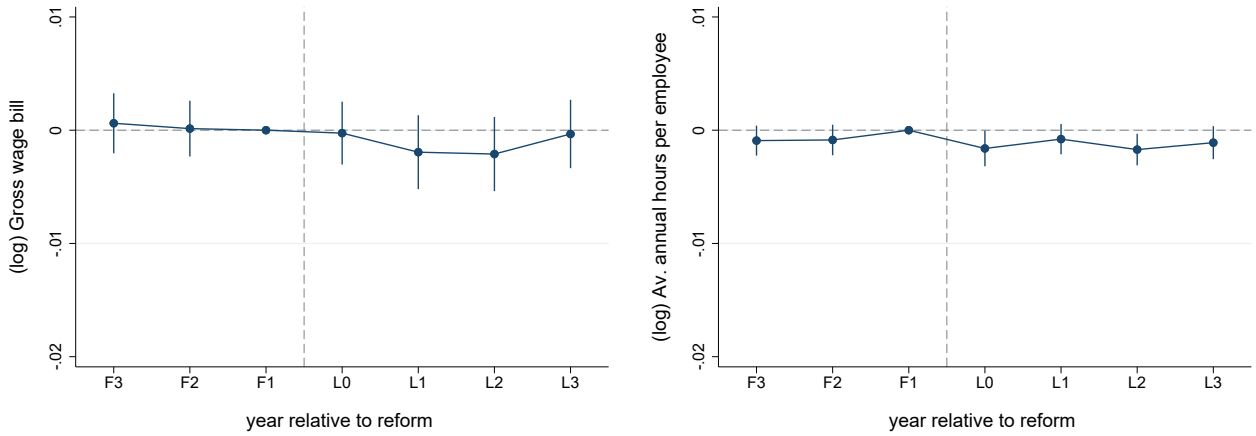
Note: This table reports the point estimates obtained with the generalized difference-in-differences model (Eq. 3.3) fully interacted with a dummy indicating whether the plant is a single-plant firm. The standard errors are displayed in parentheses and are clustered at the municipality level. Coefficients measure the effect of a reduction of 1% in the net of tax. The line "equality coef." reports the p-value of the test evaluating whether the difference between the coefficients of the two groups is statistically significant. The dependent variable is the log of tax payment (1), the log of the number of employees (2), the log of the median hourly wage (3), the log of the average hourly gross wage (4), the log of the gross wage bill (5), the log of the number of hours worked per employee (6), the log of tangible assets (7), the movable ratio (8) and the property ratio (9). Firms are sorted by size based on their sales quartile rank in 2003. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 3.A.13: Differences-in-Differences Estimates by Industries

	(1)	(2)	(3)
	(log) Tax Payment	(log) Nb. Employees	(log) Med. houly wage
Manufacturing	-2.026***	0.031	0.048
	(0.226)	(0.122)	(0.046)
Construction	-2.204***	0.152	-0.007
	(0.263)	(0.119)	(0.060)
Trade	-2.166***	0.051	0.100**
	(0.244)	(0.094)	(0.044)
Hospitality	-2.626***	0.048	-0.083
	(0.323)	(0.213)	(0.073)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
1-digit Industryx FE	✓	✓	✓
Obs.	610,128	610,196	610,196
R2	0.947	0.966	0.869
	(4)	(5)	(6)
	(log) Av. hourly gross wage	(log) Gross wage bill	(log) Av. annual hours per employee
Manufacturing	0.051	0.132	0.049
	(0.049)	(0.127)	(0.055)
Construction	0.054	0.206	0.000
	(0.064)	(0.138)	(0.058)
Trade	0.060	0.164	0.053
	(0.052)	(0.126)	(0.050)
Hospitality	0.045	0.110	0.017
	(0.066)	(0.223)	(0.113)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
1-digit Industryx FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.907	0.969	0.711
	(7)	(8)	(9)
	(log) Tangibles	Movables ratio	Property ratio
Manufacturing	-0.066	-5.585	2.303
	(0.194)	(11.614)	(1.823)
Construction	-0.374	-76.643	-2.756
	(0.322)	(67.918)	(3.426)
Trade	-0.049	-9.996	-0.879
	(0.177)	(9.112)	(1.493)
Hospitality	-0.341	3.952	1.318
	(0.274)	(11.677)	(1.629)
Firm FE	✓	✓	✓
CountyxYear FE	✓	✓	✓
LMxYear FE	✓	✓	✓
1-digit Industryx FE	✓	✓	✓
Obs.	610,196	610,196	610,196
R2	0.946	0.397	0.394

Note: This table reports the point estimates obtained with the generalized difference-in-differences model (Eq. 3.3) fully interacted with a categorical variable reporting 1-digit industry classification. We report only the point estimates for the main industries. The standard errors are displayed in parentheses and are clustered at the municipality level. Coefficients measure the effect of a reduction of 1% in the net of tax. The dependent variable is the log of tax payment (1), the log of the number of employees (2), the log of the median hourly wage (3), the log of the average hourly gross wage (4), the log of the gross wage bill (5), the log of the number of hours worked per employee (6), the log of tangible assets (7), the movable ratio (8) and the property ratio (9). Firms are sorted by size based on their sales quartile rank in 2003. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

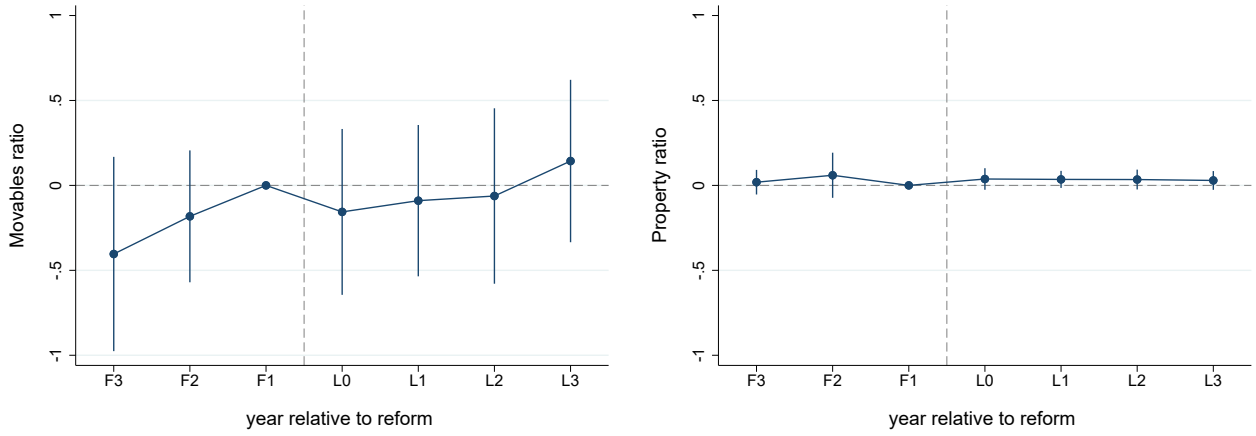
3.B Additional Figures



(a) Effect on the Wage Bill **(b) Effect on Hours Worked per Worker**

Figure 3.B.1: Effect on the Workforce – additional outcomes

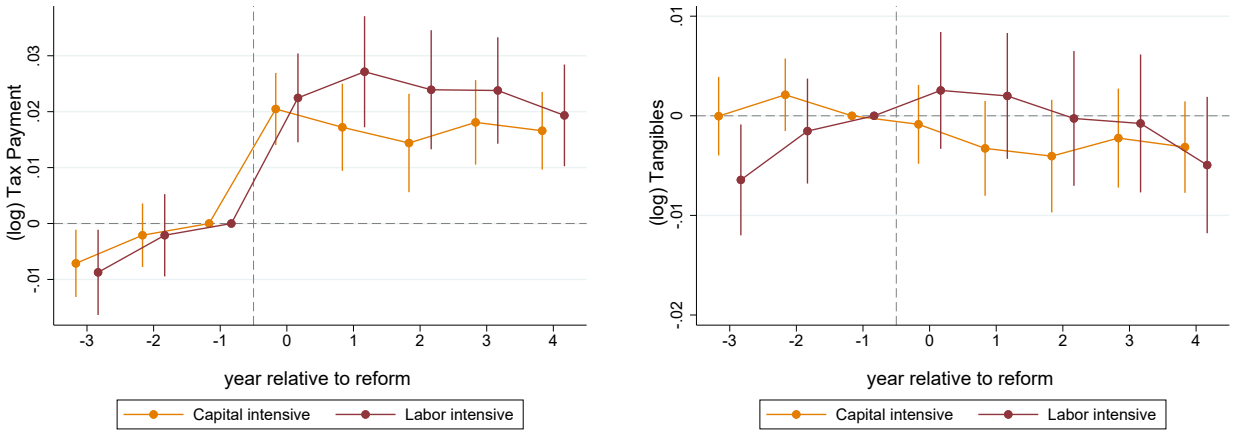
Note: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (3.1). The dependent variable in Panel (a) is the log of the wage bill, and in Panel (b), the log of annual hours worked per worker. The vertical line highlights the implementation of the reform.



(a) Effect on Movables **(b) Effect on Properties**

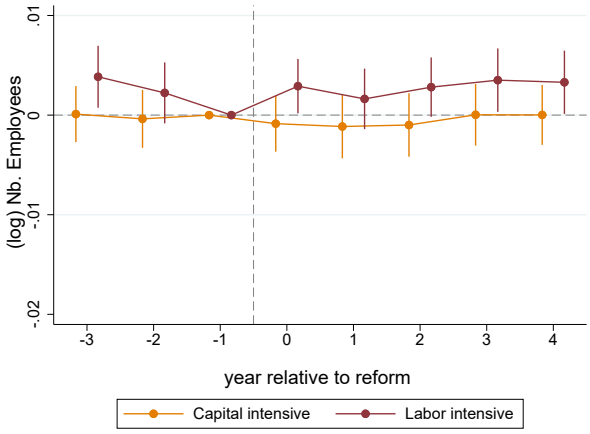
Figure 3.B.2: Effect on Capital by Type

Note: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (3.1). The dependent variable in Panel (a) is the ratio of the current stock of movables to the reported stock of capital in 2003. In Panel (b), the outcome is the ratio of the current stock of properties to the reported stock of capital in 2003. The vertical line highlights the implementation of the reform.

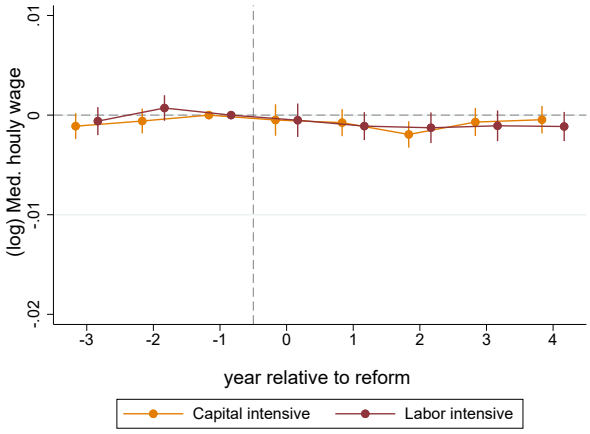


(a) Effect on Tax Payment

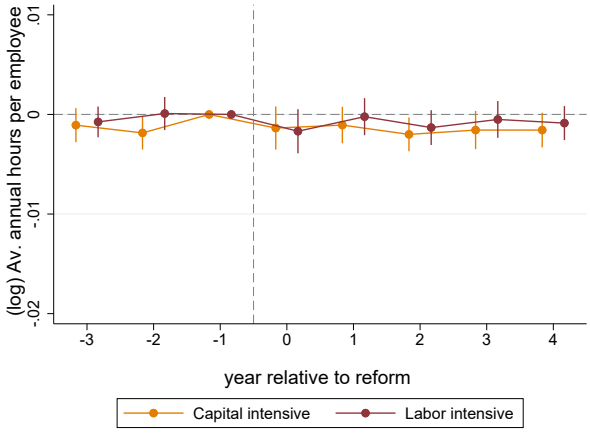
(b) Effect on Capital



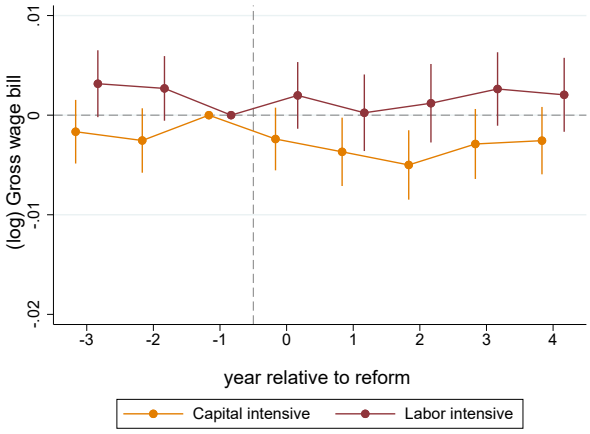
(c) Effect on Employment



(d) Effect on Median hourly wages



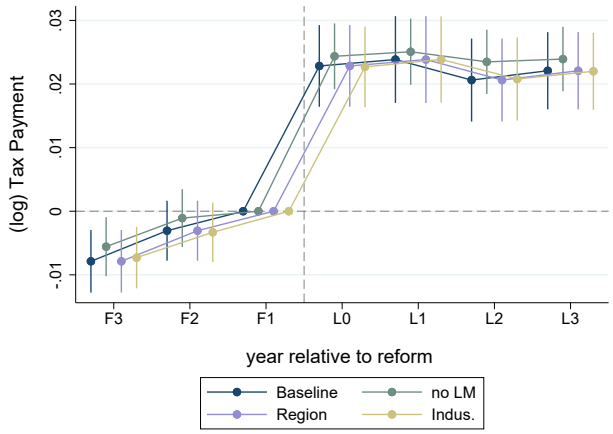
(e) Effect on Annual Hours Worked per employee



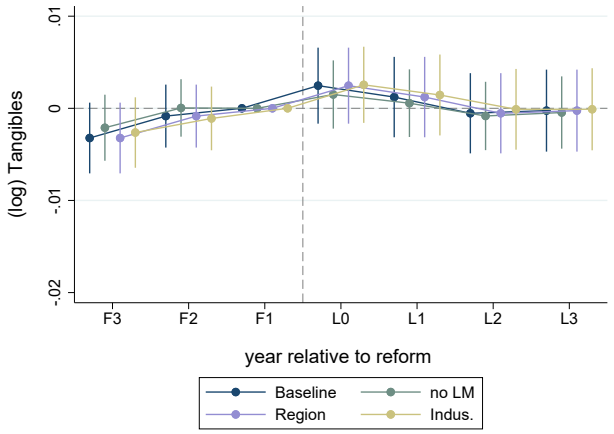
(f) Effects on the Wage bill

Figure 3.B.3: Heterogeneity: Effects by Capital Intensity.

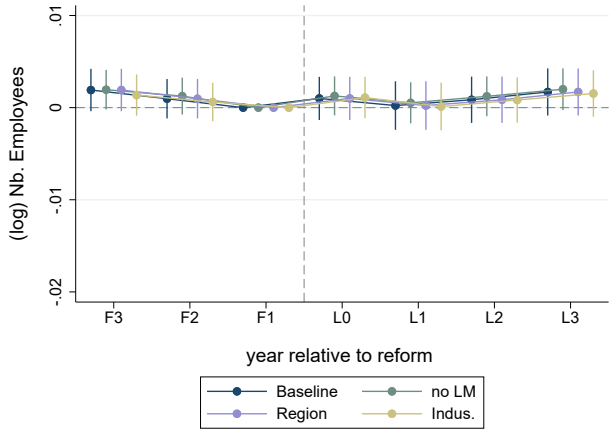
Note: The plot the event study estimates (β_l) and corresponding 95 percent confidence bands of the specification (3.1) fully interacted with a dummies variable indicating whether the ratio of the gross wage bill over the stock capital is below the sample median in 2003 – called Capital intensive and Labor Intensive otherwise. The dependent variable is the log of tax payment in Panel (a), the log of tangible assets in Panel (b), the log of the number of employees in Panel (c) the log of the median hourly wage in Panel (d), the log of the annual hours worked per employee in Panel (e), and the log of the gross wage bill in Panel (f). The vertical line highlights the implementation of the reform.



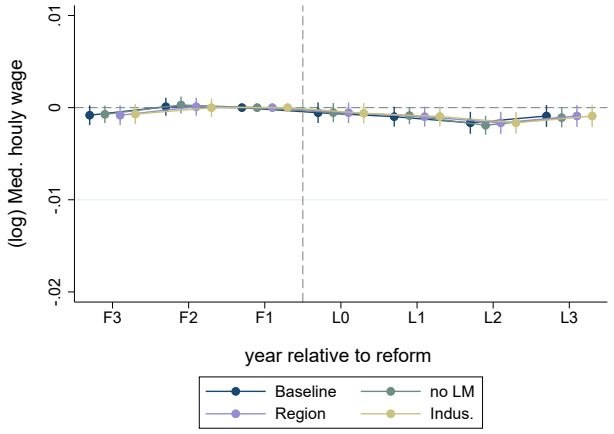
(a) Effect on Tax Payment



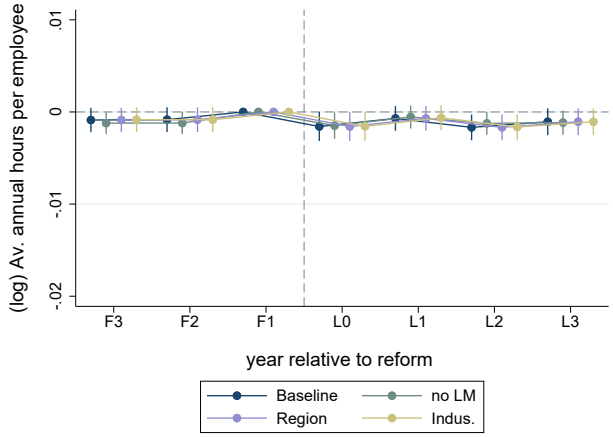
(b) Effect on Capital



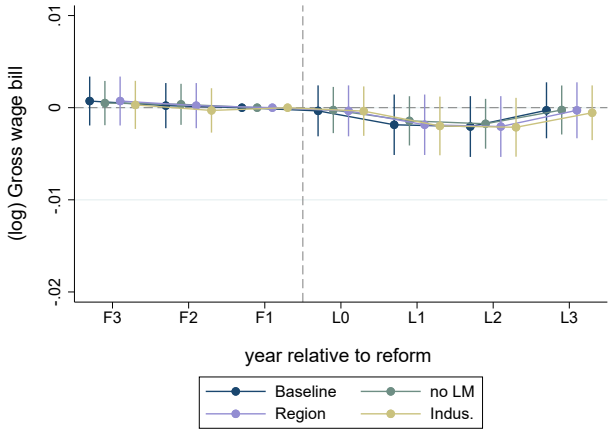
(c) Effect on Employment



(d) Effect on Median hourly wages



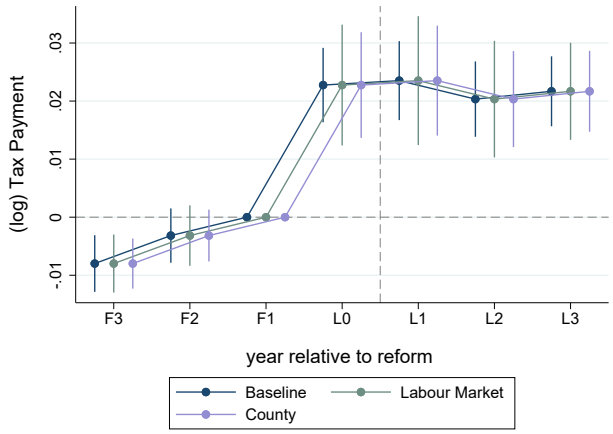
(e) Effect on Annual Hours Worked per employee



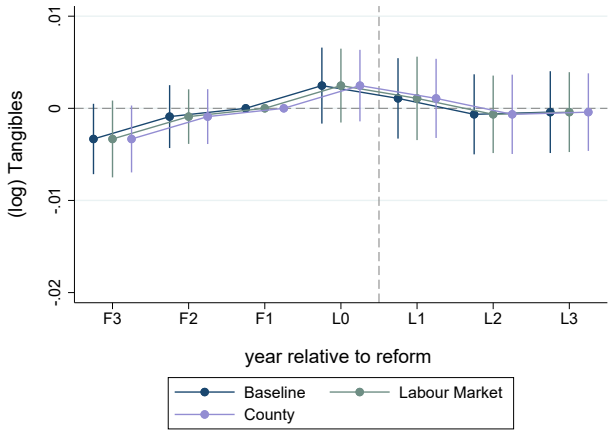
(f) Effects on the Wage bill

Figure 3.B.4: Robustness: Controlling for Shocks.

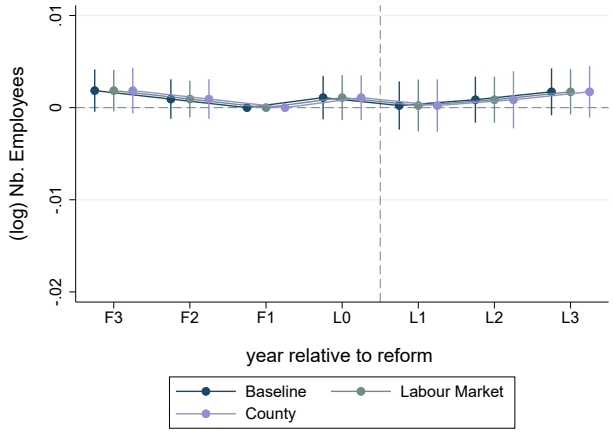
Note: The plot shows the event study estimates (β_l) and corresponding 95 percent confidence bands of the specification (3.1) and alternative models with different sets of fixed effects: removing the local labor market-by-year FEs; adding regional-by-year FEs; adding 2-digit industry-by-year FEs. The dependent variable is the log of tax payment in Panel (a), the log of tangible assets in Panel (b), the log of the number of employees in Panel (c) the log of the median hourly wage in Panel (d), the log of the annual hours worked per employee in Panel (e), and the log of the gross wage bill in Panel (f). The vertical line highlights the implementation of the reform.



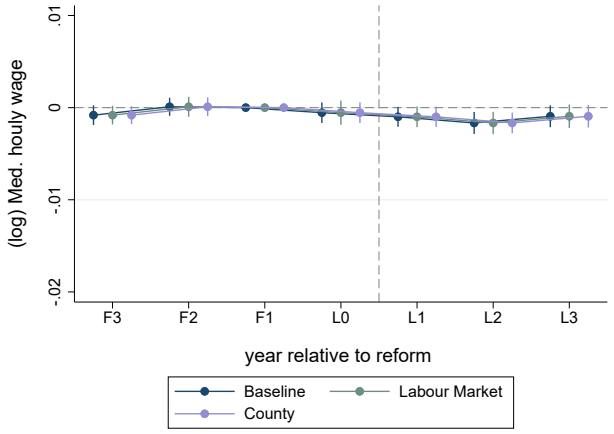
(a) Effect on Tax Payment



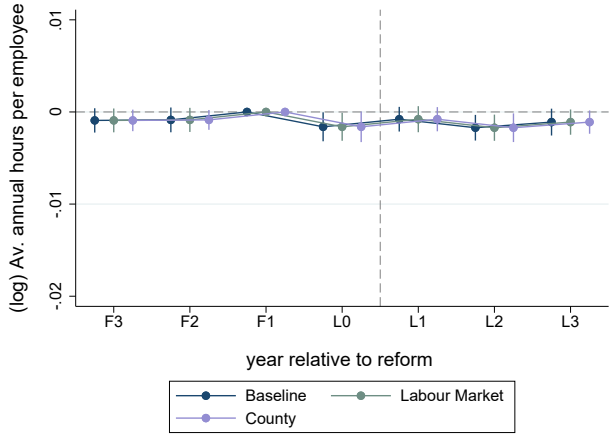
(b) Effect on Capital



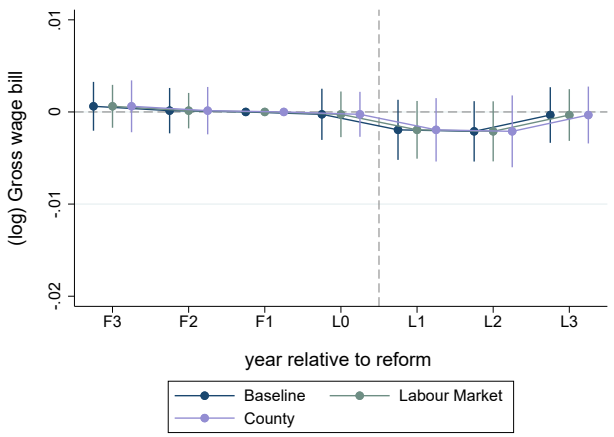
(c) Effect on Employment



(d) Effect on Median Hourly Wages



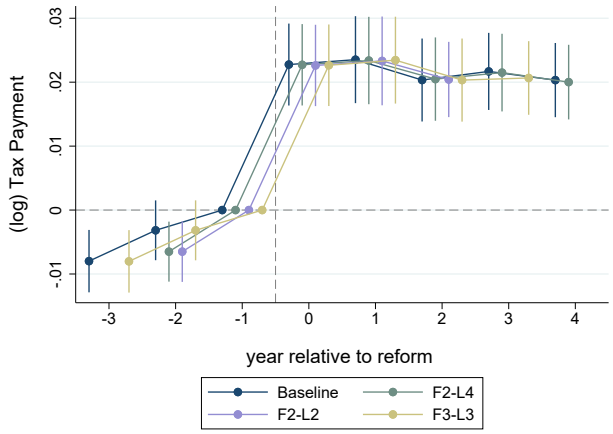
(e) Effect on Annual Hours Worked



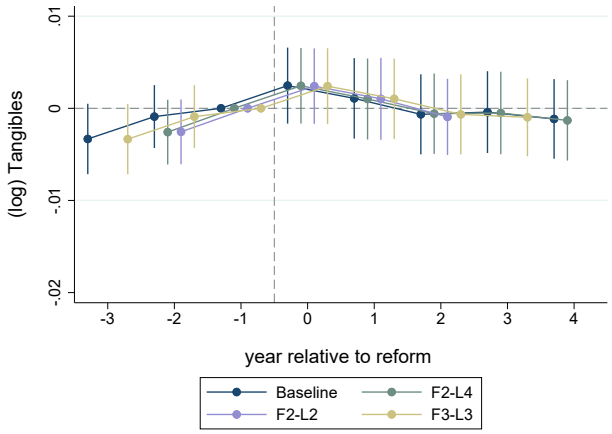
(f) Effects on the Wage bill

Figure 3.B.5: Robustness: Change in Clustering Level.

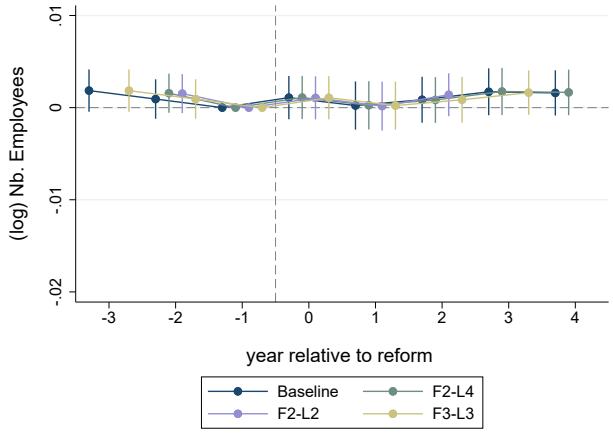
Note: The plot the event study estimates (β_l) and corresponding 95 percent confidence bands of the specification (3.1) and alternative models with standard errors clustered at different levels: the county (n=96) or the local labor market (n≈400). The dependent variable is the log of tax payment in Panel (a), the log of tangible assets in Panel (b), the log of the number of employees in Panel (c) the log of the median hourly wage in Panel (d), the log of the annual hours worked per employee in Panel (e), and the log of the gross wage bill in Panel (f). The vertical line highlights the implementation of the reform.



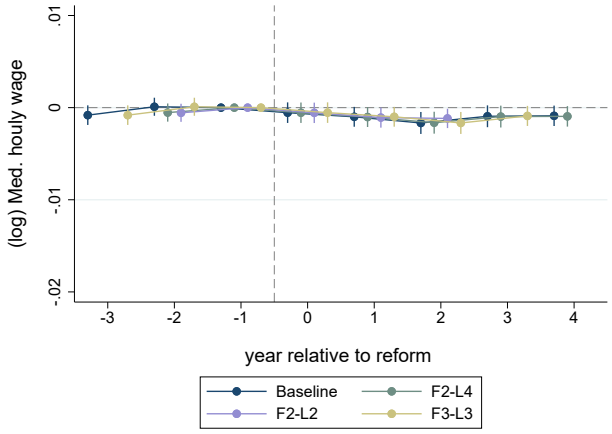
(a) Effect on Tax Payment



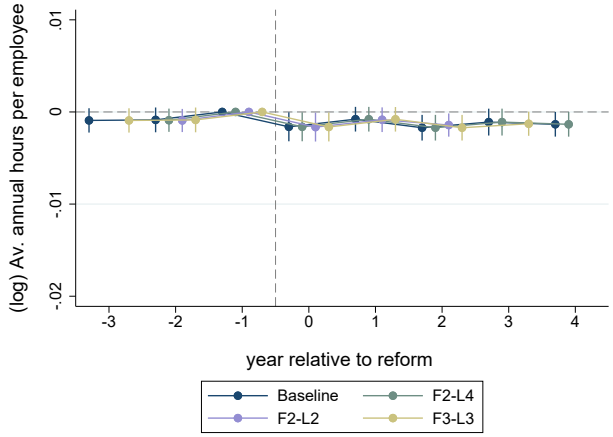
(b) Effect on Capital



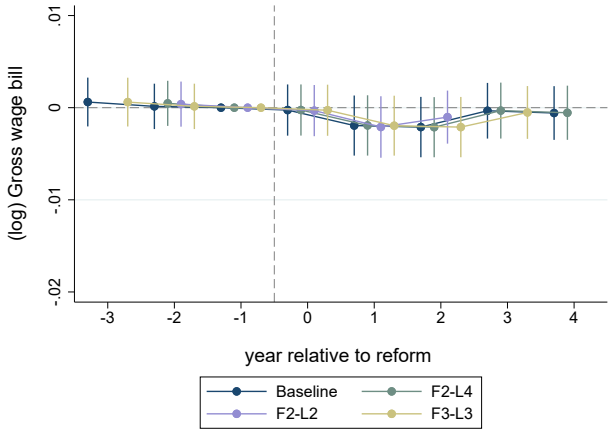
(c) Effect on Employment



(d) Effect on Median hourly wages



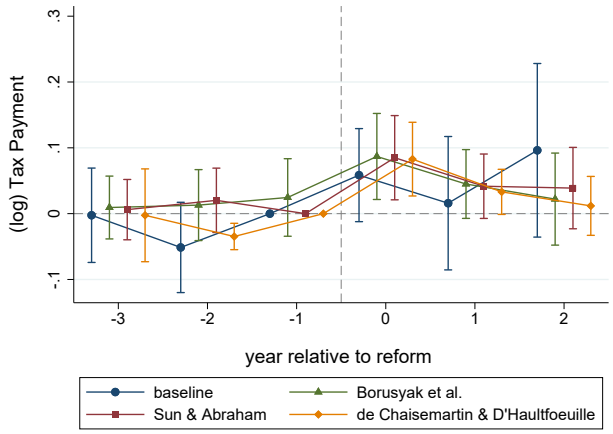
(e) Effect on Annual Hours Worked per employee



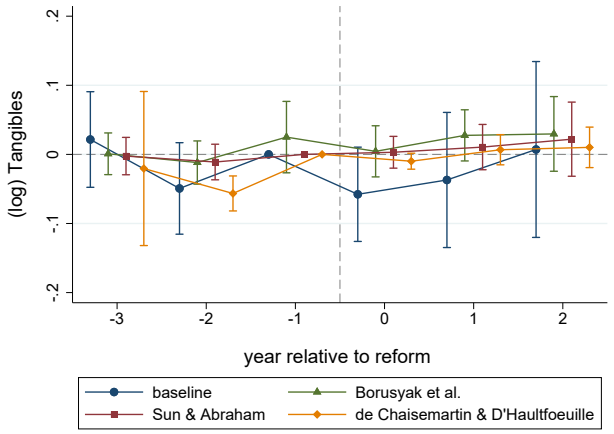
(f) Effects on the Wage bill

Figure 3.B.6: Robustness: Change in the Window.

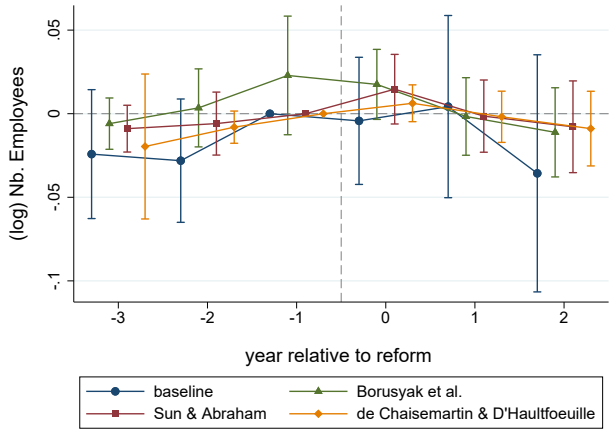
Note: The plot the event study estimates (β_l) and corresponding 95 percent confidence bands of the specification (3.1) and alternative models with different numbers of leads and lags. The dependent variable is the log of tax payment in Panel (a), the log of tangible assets in Panel (b), the log of the number of employees in Panel (c) the log of the median hourly wage in Panel (d), the log of the annual hours worked per employee in Panel (e), and the log of the gross wage bill in Panel (f). The vertical line highlights the implementation of the reform.



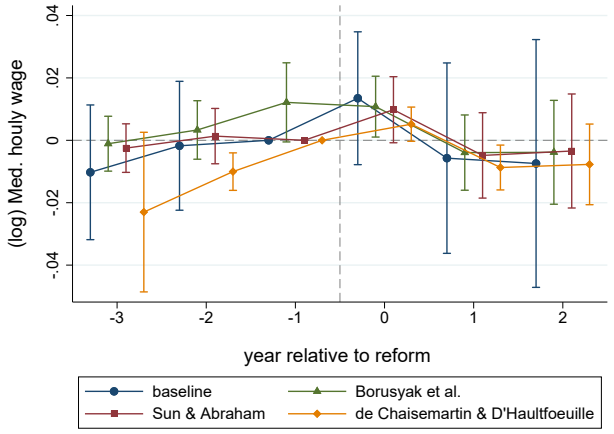
(a) Effect on Tax Payment



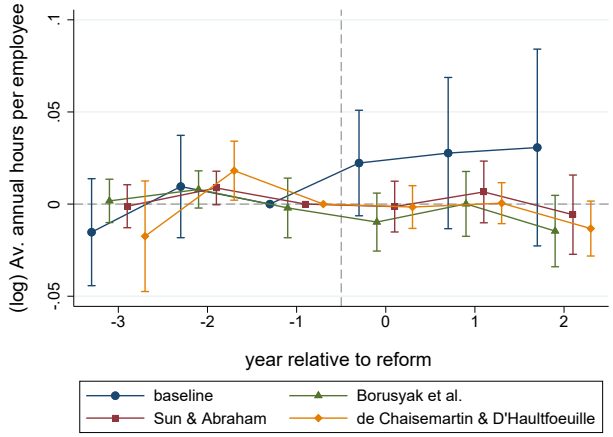
(b) Effect on Capital



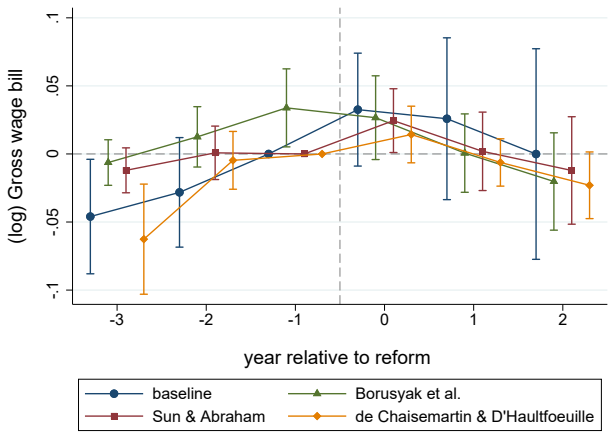
(c) Effect on Employment



(d) Effect on Median hourly wages



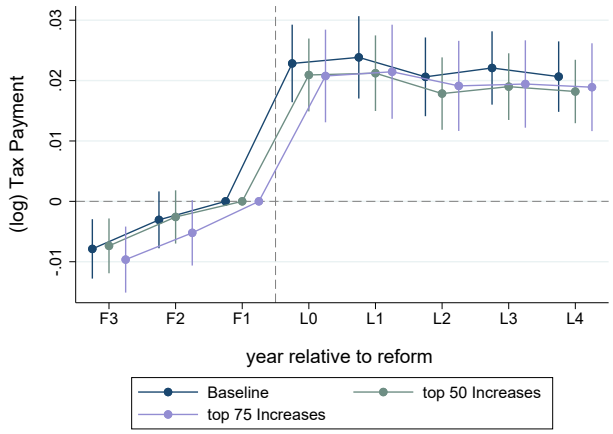
(e) Effect on Annual hours worked



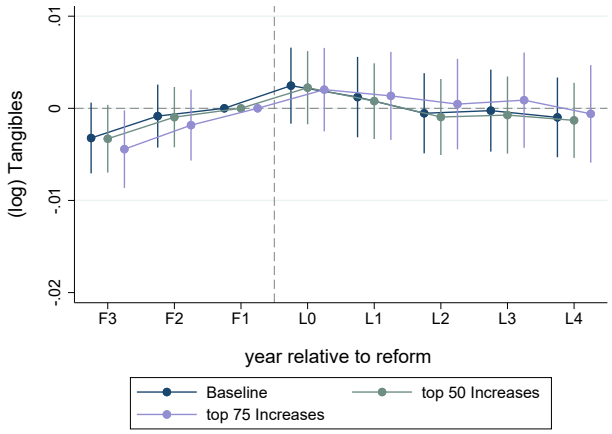
(f) Effect on the wage bill

Figure 3.B.7: Robustness: Change in the Estimator.

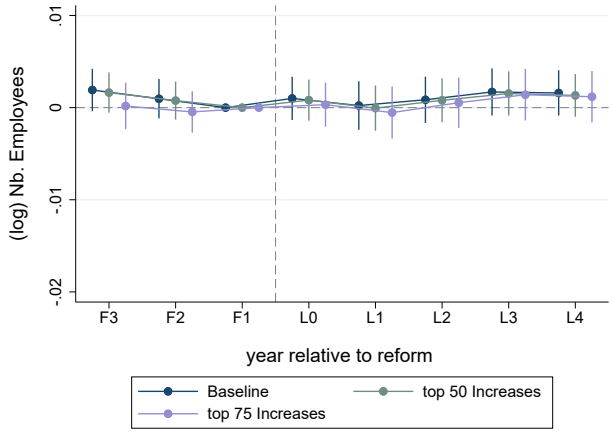
Note: The plot shows the event study estimates (β_l) and corresponding 95 percent confidence bands of the specification (3.1) and alternative estimators (de Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Borusyak et al., 2024). The dependent variable is the log of tax payment in Panel (a), the log of tangible assets in Panel (b), the log of the number of employees in Panel (c) the log of the median hourly wage in Panel (d), the log of the annual hours worked per employee in Panel (e), and the log of the gross wage bill in Panel (f). The vertical line highlights the implementation of the reform.



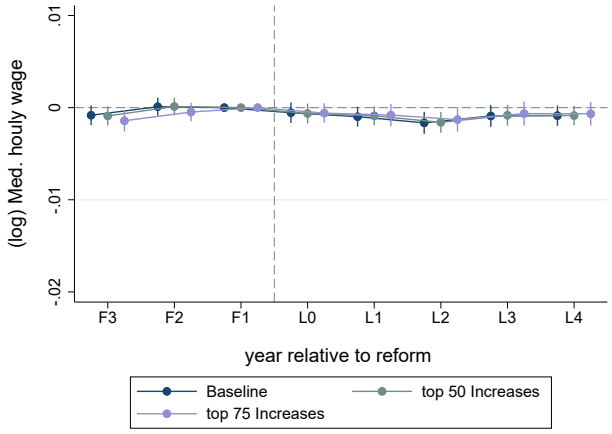
(a) Effect on Tax Payment



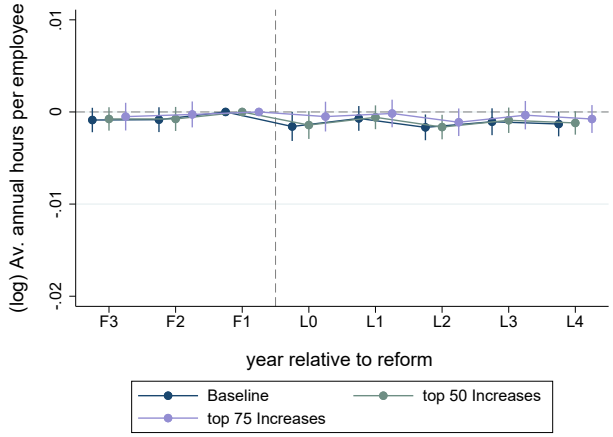
(b) Effect on Capital



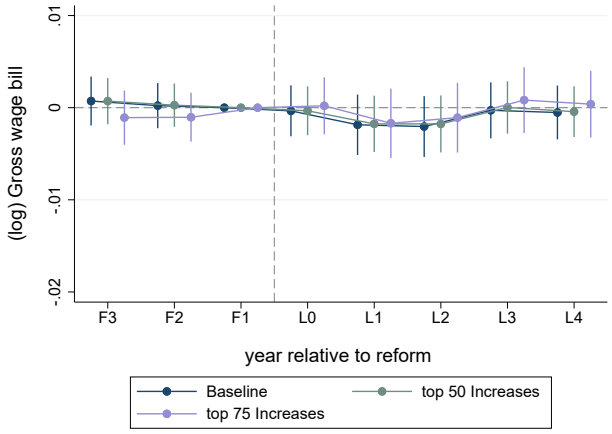
(c) Effect on Employment



(d) Effect on Median hourly wages



(e) Effect on Annual Hours Worked per employee



(f) Effects on the Wage bill

Figure 3.B.8: Robustness: Large Tax Increases.

Note: The plot shows the event study estimates (β_l) and corresponding 95 percent confidence bands of the specification (3.1) and alternative models leveraging larger tax hikes: the top 50% (top 50) and top 25% (top 75) increases in the distribution of the sample. The dependent variable is the log of tax payment in Panel (a), the log of tangible assets in Panel (b), the log of the number of employees in Panel (c) the log of the median hourly wage in Panel (d), the log of the annual hours worked per employee in Panel (e), and the log of the gross wage bill in Panel (f). The vertical line highlights the implementation of the reform.

3.C The French Business Tax before 2010

This summary is based on an official document addressed to tax advisors (DGFIP, 2004) and another addressed to municipal staff (DGCL, 2018).

3.C.1 Definition of the Tax Rate

Local tax rates are voted on in March of year T and take effect in year T+1. The process for setting these rates depends on whether a municipality is part of an EIMC (Establishment for Inter-Municipal Cooperation). Municipalities that are not part of an EIMC have relatively broad discretion in setting their tax rates.[12] For municipalities within an EIMC, tax rate decisions depend on the fiscal regime of the EIMC. There are two types of fiscal cooperation: the single business tax regime and the additive tax regime.

In the single business tax regime, municipalities fully delegate their fiscal authority to the EIMC. A uniform tax rate is then applied across all member municipalities. If substantial disparities exist between pre-existing tax rates when municipalities join the EIMC, a harmonization period may be established. During this period, EIMC members annually vote on a single business tax rate, which serves as a reference point for adjusting individual municipal tax rates. As a result, tax rates across municipalities may differ and fluctuate throughout the harmonization period. This phase typically lasts between 2 and 12 years.

In the additive tax regime, each municipality continues to set its own local tax rate independently. In addition, all members collectively vote on a shared tax rate that is uniformly applied across the EIMC. This common rate is then added to each municipality's individual rate. These fiscal regimes are summarized in Table 3.C.1. Further details can be found in Breuillé et al. (2018) and Tricaud (2025).

[12] However, there are constraints due to interdependencies with other local taxes (e.g., housing or property taxes), which limit the extent to which municipalities can adjust all rates simultaneously.

Table 3.C.1: Tax setting according to the fiscal regime of the EIMC before 2010

Fiscal regime	level	Business Tax
No cooperation	Municipal	t_M
	EIMC	
	County	t_D
	Region	t_R
	Cumulative tax rates	$\tau^B = t_M + t_D + t_R$
Single Business Tax	Municipal	
	EIMC	t_{EIMC}
	County	t_D
	Region	t_R
	Cumulative tax rates	$\tau^B = t_{EIMC} + t_D + t_R$
Additive taxation	Municipal	t_M
	EIMC	t_{EIMC}
	County	t_D
	Region	t_R
	Cumulative tax rates	$\tau^B = t_M + t_{EIMC} + t_D + t_R$

Note: This table summarizes local taxes faced by firms with respect to the different tax regimes of EIMC. It held until 2009. After the 2010 reform, regions and counties no longer collect the business tax.

3.C.2 Definition of the Tax Base

3.C.2.1 Machinery and Equipment

Taxable value: The taxable value of short-duration assets (less than 30 years) is defined by:

$$ME\_base_t = real\_cost_t \times 16\% \tag{3.4}$$

The taxable value of long-duration assets (more than 30 years) is defined by[13]:

$$ME\_base_t = real\_cost_t \times 8\% \tag{3.5}$$

The *real_cost* is given by the cost of producing the asset or the gross price paid to acquire it. The real costs are one-time purchase; they do not include maintenance.

Tax base reduction: There are two possible tax abatements:

- a fixed tax abatement for old establishments of €3,800 (until 1982)
- a progressive tax abatement is defined at the level of the firm as follows:

[13] 9% instead of 8, for assets acquired before 1976.

If the turnover of the firm is between $\overline{Turnover}$ and $2 \times \overline{Turnover}$, the firm can claim a tax base reduction that is computed as follows:

$$abatement_{t,82+} = \begin{cases} \frac{2 \times \overline{Turnover} - turnover_t}{\overline{Turnover}} \times ME\_base_t & \text{if} \quad turnover_t < \overline{Turnover} \\ 0 \quad \text{otherwise} \end{cases} \tag{3.6}$$

The turnover limit ($\overline{Turnover}$) is defined nationwide and depends on the tax regime of the firm. For services, the upper limit is €61,000, and for the rest, it is €152,500. If the firm is multi-establishment, deductions can be claimed by the main establishment (the one that states the VAT) and the pilot establishment. The pilot establishment is defined as the largest establishment in terms of turnover (?)[14]. To compute the deduction, the taxable value used for the pilot establishment is equal to the total value of equipment and machinery of all secondary establishments.

3.C.2.2 Properties

Taxable value: The taxable value of properties is defined by:

$$Property\_base_t = 8\% \times Rental\_value \tag{3.7}$$

With

$$Rental\_value = \begin{cases} Real\_cost \quad \text{if industry} \\ Cadastral\_value \times \delta_t \quad \text{otherwise} \end{cases}$$

For industrial establishments (in opposition to commercial activities), the rental value of properties is the value stated in the balance sheet. Otherwise, the rental value is the cadastral value multiplied by a coefficient δ_t. The aim of this coefficient is to bring the property value closer to reality. It keeps increasing over time, as an example, it was 2.549 for developed properties in 2004.

[14]Not specified, need to look for more information.

3.C.2.3 The Wage bill (until 2002)

Taxable value: 18% of the the wage bill. The wage bill is progressively removed from the tax base starting in 1999. In 2003, establishments are not taxed on the wage bill anymore. See Chapter 2.

Table 3.C.2: Wage Bill Abatement

	1999	2000	2001	2002
max abatement in deflated euros	18,523	55,235	180,413	1,062,362

3.C.3 Tax Payment

The final tax base on which the tax rate is applied to compute the final tax payment is computed as follows:

Table 3.C.3: Tax Base Computation

+	taxable value of machinery and equipment ($=ME\_base_t$)
+	taxable value of properties ($=Property\_base_t$)
+	taxable value of the wage bill
-	fixed abatement (=€3,800) or progressive one ($=abatement_{t,82+}$)
=	Real tax base
-	general abatement (= 16% of Real tax base)
=	**Total tax base**

Note: If the establishment is newly created, the real tax base is divided by two. If the establishment is located in Corsica, there is an additional discount of 25% on the remaining 84% of the tax base.

The tax payment at the establishment level is computed by applying the applicable local tax rate on the total tax base. However, this tax payment is bounded: it has to be at least equal to the minimum tax payment (1.5% of the value added produced by firms) and is reduced when exceeding a given percentage of the value added produced by the firm. The percentage depends on the firm's size and has changed over time, see Table 3.C.4.

Table 3.C.4: Business Tax - Ceiling Evolution

Group	Gross Turnover of the company	Before 2007	After 2007	After 2010
1	inf. 21,35 M Euros	3.5%		
2	from 21,35M Euros 76,225 M Euros	3.8%	3.5%	3%
3	over 76,225 M Euros	4%		

In case the tax payment excess $x_f\%$ of the value added produced at the firm level, the maximum tax payment ($\bar{\rho}_t$) is computed as follows: the total firm liability, minus the liability of reference plus $x_f\%$ of the value added produced at the firm level.

$$\bar{\rho}_t = \sum_e BT_{e,t} \cdot Total\_tax\_base_{e,t} - \left[\sum_e \min\{BT_{e,1995}, BT_{e,t}\} \cdot Total\_tax\_base_{e,t} - x_f\% \cdot ValueAdded_t \right]$$

Each year, firms fill in their tax statements about what they owned in the previous year T-1. In year T, they pay their tax liabilities of the year T which is actually based on what they owned in year T-2. More precisely,

$$TaxPayment_t = TaxBase_{t-2} \times \tau_t \tag{3.8}$$

Timeline:

1. In May of year T-1, each firm declares the value of its real estate and equipment, fixed assets owned during year T-2.

2. In March/April of year T-1, the local authorities vote for a tax rate that will be enforced during year T.

3. Firms must pay the BT during year T. In practice, the firm pays twice, first in May of year T and then in December.

The voted rates are made public on the website of municipalities and EIMCs. The effective tax rates are available in the tax assessment. The voted nor the effective rates are available when firms fill in their tax statement. The tax rates are voted only one year after firms have filed their statement. Voted rates are used to compute maximum contributions.

Chapter 4

The Effects of Subsidized Employment for Youth

This chapter is based on Gharbi et al. (2024).

4.1 Introduction

Youth unemployment remains a persistent challenge for policymakers in developed countries (International Labor Organization, 2024). A common policy response is subsidized employment – an active labor market policy (ALMP) usually involving substantial public investment in wage subsidies, training, and mentoring. Despite the widespread use and high costs of these programs, most studies document null or very small positive effects for disadvantaged youth (Kluve et al., 2019).

In this study, we evaluate a subsidized employment program for disadvantaged youth in France and demonstrate that the modest average effects have limited informativeness and are driven by pronounced heterogeneity by gender. We find that, while male participants benefit through improved labor market attachment and employment outcomes, female participants experience considerably smaller effects. We further provide evidence that this gender differential in effects is likely to be driven by two mechanisms: young female participants are more likely to sort into lower-quality jobs, and are more likely to have a child during the program.

Despite being a popular topic in labor economics, gender heterogeneity has received scant

attention in the ALMP literature, and policymakers have similarly overlooked this aspect when designing and evaluating policies. Factoring in possible differences between genders is seldom a priority in the design of these policies (Muller and Kurtz, 2003; Bergemann and van den Berg, 2008). Indeed, despite political initiatives in the '90s and substantial improvement of data quality, our knowledge about gender-specific effects of active labor market policies is still in a dismal state.[1] It is evident that gender-specific factors such as parenthood and the underlying mechanisms are lacking in existing studies, especially for youth (Card et al., 2018). Overlooking interactions with family responsibilities yields significant repercussions for women, potentially generating lasting income inequalities (Goldin et al., 2021).

In this paper, we aim to close this gap by evaluating a subsidized employment program implemented in France between 2012 and 2018, with a particular emphasis on examining how policy effects and their dynamics vary across genders. We study the effect of the *Emplois d'avenir* program – translated Jobs for the Future program (henceforth JFF). This program subsidized working contracts for unemployed workers 16 to 25 years old who have no or a low level of education and come from deprived areas. It was implemented in the context of a challenging transition from school to employment, thereby reducing the likelihood of securing a full-time job (European Employment Observatory Report, 2010; Cour des Comptes Report, 2016). The subsidized contracts supported the employers by paying 35% to 75% of the wage of the targeted workers for up to three years. In exchange, employers committed to training young workers. This program is particularly well-suited for our study, as it targets both young men and women equally during a phase of life when they may be considering starting a family.

We start by conducting a descriptive analysis of the participants' trajectories and document two key patterns: first, the Ashenfelter (1978)'s dip – the decrease in outcomes of participants in the year before enrollment into the program – exists only for individuals treated at a later age (i.e. closer to the age of 25 in our sample of youth); second, this dip is more pronounced for male participants. These empirical facts indicate the importance of estimating the effects separately

[1]The European Union acknowledged such threats in the '90s when designing the "European Employment Strategy", emphasizing the need to create equal opportunity for men and women. The EU further formalized this goal by developing guidelines in 1999. In particular, they stress the importance of balancing work and family life and urge to account for gender-specific effects when evaluating labor market policies.

for cohorts (in terms of age at participation) and by gender. Considering this, we evaluate the program's effectiveness using a dynamic difference-in-differences approach and exploit eligibility criteria for the program. We document that subsidized employment is more effective for men and childless women, while mothers benefit less. We contribute to the literature by highlighting the role of gender, parenthood, and age at participation as crucial factors in the effectiveness of these subsidies – aspects previously overlooked in this context.

As with many policies, evaluating the JFF program raises the issue of selecting the appropriate counterfactual: treated individuals are only observed in the treatment case (Heckman et al., 1999; Blundell and Costa Dias, 2000). We use this policy's eligibility criteria to construct a counterfactual. More precisely, we exploit the quasi-random variation in the distribution around the age cutoff of 25 and apply the Difference-in-Differences method. We restrict our study to individuals between 24 and 26 for treated individuals and up to three years older than 26 for the control group. Individuals must be 25 when signing the contract, but can be 26 when starting it; hence, the effective cutoff is at 26. To bring together more comparable individuals, we restrict the control group to those with no or a low level of education and/or from deprived areas. The control group is thus made up of slightly older individuals who would have been eligible had such a policy existed for them. This allows us to compare the outcomes of participants with the typical life-cycle trajectory of comparable youth in terms of education and unemployment backgrounds, as they meet the program's eligibility criteria. We assume that participants would have followed a similar life-cycle trajectory if they had not had a bad year and then participated in the program. Building on the findings from descriptive analysis, we use the estimator developed by de Chaisemartin and d'Haultfoeuille (2024), as it allows us to analyze by cohort (in terms of age at participation) and gender, accounting for heterogeneous effects along these two dimensions.

Using rich, longitudinal administrative data, we identify workers who benefit from the JFF program and follow them before, during, and after the subsidy. We find that completing the program leads to increased annual earnings and increased hours worked. In the medium run – 3 years after the program, beneficiaries raise their annual earnings by €2,000 and work 200 hours more annually. However, the average effect on the employment probability – the primary

objective of these policies – is not significantly different from 0. Yet, we demonstrate that such a null result masks significant disparities by gender and parental status. We find significant and positive effects on all outcomes on extensive and intensive margins for men. They earn, on average, an additional €2,500 per year and are 6.5 percentage points more likely to be employed than their counterfactual. For women, the program fails to enhance their employment prospects in a sustainable manner.

As parenthood contributes significantly to the prevailing gender inequality in the labor market (Blau and Kahn, 2017), we also carry out heterogeneity analysis along these lines. Importantly, we find no systematic difference between the age at which female beneficiaries have their first child and their counterfactual have theirs. Then, we highlight that the event of childbirth and its timing are crucial within the context of this program. Earnings and hours worked improve for all groups except mothers and even worsen for those giving birth during or after the program and, therefore, dropping out.

Moreover, we show that at the start of the subsidized contract, women sort into routine and non-cognitive occupations compared to men, who tend to work in public administration and construction doing more non-routine cognitive tasks. Furthermore, young female participants are over-represented in the healthcare and social sectors and often find employment in firms where most workers are female. These results indicate the gender-based sorting into jobs of different quality.

Related literature. This paper connects two strands of the literature: the ALMP and the gender literature. We contribute to the general ALMP literature by investigating the effects of subsidized employment combined with training and job-search support. Using a quasi-experimental framework, we evaluate the impact of such programs at different points in time. Evaluating such programs in the short and longer terms is crucial as lock-in effects are known to dominate in the short run, especially for policies with long training time (Fitzenberger and Völter, 2007; Lammers and Kok, 2021). Yet, evidence, in the long run, is limited (see for example the reviews of Bergemann and van den Berg, 2008; Crépon and van den Berg, 2016).

Concerning the effects of ALMPs on youth, evidence has been rather pessimistic on their

employment prospects so far. In a recent study, Kluve et al. (2019) document that only one-third of the studies focusing on youth find small significant positive average effects. The authors emphasize that the specific design features, the country context, and the beneficiaries' characteristics are important for the effectiveness of a program. However, most studies limit their evaluation to the average treatment effect. We bring new dimensions to this literature by investigating the effect of such policy across gender but also parenthood.

The investigation by gender is not systematic in the ALMP literature. The little evidence highlights that ALMPs have rather positive effects on women, as summarized by Card et al. (2018). However, the literature focusing on young female workers is more nuanced. Some papers find that such programs were more favorable for girls, without being necessarily positive (e.g. Katz, 1996; Attanasio et al., 2011). Others find a limited effect for young women (Centeno et al., 2009). This evidence rarely suggests mechanisms at play for the difference in effectiveness.

While the heterogeneous effects by gender have been only partly researched, the role of young parenthood remains understudied. Groh et al. (2012) highlight that subsidized contracts do not provide a stepping-stone to employment. This null effect is highly correlated with marriage.[2] Interestingly, programs combined with additional financial support to cover childcare expenses have strong and positive effects on women's outcomes (Attanasio et al., 2011).

In the French labor market, there have been several types of subsidized working contracts aiming to help youth. Roger and Zamora (2011) highlight that the "Youth-in-Business" policy failed to introduce school drop-outs to the labor market in a sustainable way.[3] We are aware of only two papers investigating the effects of the JFF program. Cahuc et al. (2021) and Hervelin and Villedieu (2022) send fictitious résumés to real job postings in France and look at the callback rates of different young unemployed profiles. Cahuc et al. (2021) provide suggestive evidence that employers do not stigmatize JFF beneficiaries. Hervelin and Villedieu (2022) find that the subsidized work experiment of school dropouts significantly increases the probability of job interviews. To the best of our knowledge, we are the first to evaluate the JFF program

[2]Married women are significantly less likely to work, and marital status in the Jordanian context is likely to be tied to parenthood. 28% of the girls benefiting from the subsidized job quit for family reasons or dislike of the occupation. Employers report that 30% of the girls quit because of a competitive job offer or marriage.
[3]This is probably the closest policy to the JFF program. It was implemented from 2002 to 2008.

on earnings, hours worked, and the probability of being employed in the short, medium, and long runs, in addition to providing explanations for the average zero effect of the policy.

Overall, we add to the literature by showing that the average treatment effects of ALMPs can hide substantial heterogeneity across genders. By unpacking this average effect, we deepen our understanding of the success or failure of ALMPs by identifying a key factor, especially for women: the event of childbirth and its timing. Although policy evaluations are always context-specific, here we identify a feature inherent to any woman entering any job market. To our knowledge, we are the first to show that parenthood should be considered when evaluating and designing ALMPs, particularly for youth. Indeed, considering this factor while designing policies could be an additional instrument to promote gender equality in the labor market.

We also contribute to the literature on gender inequalities in the labor markets. Women tend to sort across different occupations and industries from men, usually opting for more flexible and less competitive positions and therefore less paying jobs (Blau and Kahn, 2017). Despite aiming to fight against gender sorting across industries, we observe that the JFF program further contributes to it: women are sorted into jobs where women account for 70% of the labor force. In addition, we identify that the event of childbirth and its timing is also critical for the effectiveness of ALMPs, at least in the case of disadvantaged youth. Our study therefore provides additional evidence of the mother penalty (e.g. Adda et al., 2017; Goldin et al., 2022), and in particular, the one that emerges in early working life.

Outline. The paper is organized as follows: Section 4.2 provides information about the institutional background; Section 4.3 describes and summarizes the data; Section 4.4 describes the identification strategy; Section 4.5 presents the results; Section 4.6 discusses potential mechanisms explaining our results; Section 4.7 concludes.

4.2 Institutional Setting

This section outlines the setting of the JFF subsidized employment policy. Section 4.2.1 details the three profiles of eligible workers defined in the program. Section 4.2.2 provides a brief explanation of the terms and conditions for the subsidized jobs.

4.2.1 Workers Eligibility

Subsidized jobs have existed in France since 1984, but their design has evolved. In 2010, a reform established a general framework for all subsidized jobs: the Single Inclusion Contract (henceforth, SIC).[4] Such contracts combine training and/or professional support for their beneficiaries and financial assistance for employers. They are targeted at people having difficulties finding a job. The JFF program is part of this framework, targeting young people. It is complemented by close monitoring of the beneficiary by the unemployment agency. The primary objective of the JFF policy is to assist young workers in entering the labor market and acquiring experience and qualifications. The program was in place from October 2012 to December 2018. The last cohort exited the program in 2020. The program ended as it was depicted as inefficient.

To be eligible, young workers must be between 16 and 25 years old.[5] The required level of education depends on the time spent looking for a job and the place of residence. In this program, looking for a job was defined as periods during which young people work fewer than 78 hours per month. Eligible youths can be grouped into three profiles: (1) youths who left their initial education without a diploma; (2) youths with up to lower secondary level degree, looking for a job for at least 6 months during the last 12 months;[6] (3) youths with up to an undergraduate diploma, living in a deprived area and having looked for a job at least 12 months during the last 18 months. These three profiles are summarized in Table 4.2.1.

The share that each profile represents among JFF beneficiaries is found in Appendix 4.A, Table 4.A.1. It is worth noticing that while youth living in deprived areas are explicitly identified only in the third profile, they are always given priority when they have profiles (1) or (2).[7] We identify that around 57% of the participants are likely to live in high-priority areas.[8]

[4] In French, Contrat Unique d'Insertion (CUI).

[5] Eligible young workers are 16-25 years old at the date of the contract signature, meaning they can be 26 when they start working under such contracts. People with disabilities are eligible up to the age of 30. However, we do not provide specific insights on the effect of the program on those individuals as the data do not allow us to identify disabled people in the control group.

[6] This means that they have up to 1 or 2 years professional training and left schools around 16.

[7] The government aimed at having at least 30% of these contracts granted to youth living in such areas.

[8] Urban deprived areas are usually limited to a few neighborhoods of a city, but we have only the location of individuals at the municipality level. Therefore, we identify people living in a municipality with at least a deprived neighborhood. The classification into a high-priority area is made at the government's discretion in collaboration with local authorities.

Table 4.2.1: Profiles of Eligible Individuals

Profile	Education	Location	Job search duration
Profile 1	no diploma	none	none
Profile 2	vocational diploma in secondary school	none	at least 6 months during in the last 12 months
Profile 3	up to undergraduate diploma	high-priority area	at least 12 months in the last 18 months

Notes: This table summarizes the eligibility criteria. All workers must be 16-25 years old and up to 30 years old if disabled.

Not all eligible workers were granted such subsidized working contracts. The decision was made by the French public unemployment agency and various local job agencies specializing in working with young people, known as *Mission Locale*. Additional elements, such as personal situation and previous experiences, could also be considered when choosing a young participant who must meet the above-mentioned criteria.

4.2.2 Program Characteristics

The JFF contracts last at least one year and up to three years. They are mainly full-time contracts – 35 working hours a week. Priority is always given to open-ended contracts, but only a small share of contracts are open-ended, especially for women (21.4% for all beneficiaries and 16.5% for women, see Table 4.A.2 in Appendix 4.A). Overall, women are less likely to be employed on permanent, full-time contracts. Their contracts last, on average, fifty days less, and they are less likely to be employed in the private sector.

Training is a key element of the program. When the subsidized contract is signed, employers

There are two types of priority areas: the Sensitive Urban Area (*Zone Urbaine Sensible –ZUS*) and the Priority rural development areas (*Zone de Revitalisation Rurale – ZRR*). Sensitive Urban Areas are neighborhoods facing economic and social difficulties due to a dense population, a high share of young people, a high unemployment rate, and a large share of low-skilled workers. In 2015, this was replaced by Priority neighborhoods (*Quartiers Prioritaires de la politique de la Ville – QPV*), for which the share of inhabitants earning less than 11,250 euros annually is the only relevant criterion. This change added neighborhoods to the list of high-priority areas rather than removing some. We can check this in the data. Only 9% of the priority municipalities had neighborhoods reclassified out of the priority areas. Priority rural development areas are declining regions located in the countryside. They are characterized by some of these criteria: a decreasing number of inhabitants, a shrinking share of workers, and/or a very large share of farmers among the working population. There are no precise thresholds above which the areas become priority places. While this second type of area encompasses a whole municipality, the Sensitive Urban Area covers usually only a few neighborhoods of a city.

commit to a training plan that emphasizes the development of transferable skills. Table 4.A.2 in Appendix 4.A lists the varying types of training provided and the proportion of contracts concerned. Financial support is available and is tailored to specific sectors. Training can take place before the start of the subsidized job to prepare the worker for the position (pre-qualifying, refresher, or adaptation training), or during the contract to equip them with new skills and enhance their employability (qualifying training or skill development). The training might, but is not required to provide an official certification.

In addition, young workers can be assigned a tutor, who may be an employee of the French public employment agency, a local job center, or the employer. This tutor provides career guidance, supports integration at the workplace, helps search and apply for additional training, and prepares for the exit of the subsidized contract. In any case, the implementation of the program is closely monitored: meetings with participants take place three months after hiring, at mid-term, and two months before the end of the contract.

Employers receive, on average, a subsidy covering 35% of the minimum hourly gross wage in the for-profit sector and 75% of it in the non-profit sector.[9] For example, for an employee working full-time paid at the minimum wage rate, the monthly gross earning is €1,425 in 2012, and the monthly cost for the employer in the non-profit sector after the subsidy is €355 and €925 for a private employer. In 2015, the cost of the program was estimated at 1.3 billion euros for 121,672 beneficiaries throughout the year – resulting in a unit cost of about €11,000, excluding exemptions from social security contributions and training expenses according to the Cour des Comptes Report (2016). Employers can also apply for additional funding to finance the training scheme.

While eligibility for non-profit firms is straightforward, it is more restricted to for-profit firms. Private entities must belong to the priority sectors defined in the regional development plan or identified in national agreements – making them eligible within the territory. Nonetheless, most JFF contracts are made with employers in the public sector, such as non-profit organizations, regional and local authorities, or hospital establishments and medico-social services. The system is open to other employers in a limited way. In addition, for the private

[9] About 80% of youth benefiting from such contracts earn exactly the minimum wage.

sector, priority is given to open-ended contracts.

4.3 Data and Descriptive Statistics

This section presents the data sources used for our evaluation and the treated and control groups obtained. Section 4.3.1 details the three sources used and how we identify recipients of the JFF program in the panel of workers. Section 4.3.2 provides some descriptive statistics.

4.3.1 Data

Data on JFF contracts. We have access to the exhaustive list of JFF contracts that started between 2012 (the start of the policy) and 2015 (*Emplois d'Avenir* – JFF). This source contains information on the beneficiaries: gender, date of birth, municipality of residence and workplace, initial qualification, and the unemployment duration at the hiring date. Additional information is available on the contract and the employer such as the amount of subsidy granted for the contract, the beginning and end date of the contract, and the firm and establishment identifiers (*siren* and *siret*, respectively). The training plan, as well as the additional support provided, are detailed.

Social security and census data. We rely primarily on administrative linked employer-employee data which come from the French annual declaration of social security (*Déclaration Annuelle des Données Sociales* – DADS). It is a mandatory procedure that any employer has to fulfill every year. For each job-year spell, firms must state the wage paid, the number of hours worked, the dates of the beginning and the employment spell, the type of contract (fixed term or open-ended contract as well as whether this is a subsidized contract), and the occupational classification, among other job characteristics.

To carry out the analysis at the employee level, we use the panel version of DADS matched with the permanent demographic sample (*Echantillon Démographique Permanent* – EDP). This panel is a random sample of the standard DADS source, covering about 4% of the French workforce. In addition to information on labor market outcomes, this data source provides the date of birth, marital status, and birth dates of children. It includes a unique individual identifier

enabling us to follow people across the years. Using the identifier, we aggregate yearly information at the individual level, summing variables when numerical and taking characteristics of the main occupation.[10] Therefore, the main outcomes of interest are the annual total net earnings and the total number of hours worked. We also compute the probability of being employed, a dummy variable.

It should be noted that the panel's coverage depends on employment. When an individual is not on the panel during a year, it may be because they are unemployed or self-employed; however, we do not have this information. Therefore, we do not include the unemployment indicator in our outcomes, and we only assess the extent to which the policy affects labor outcomes for salaried occupations. We assume that individuals dropping out of the sample are unemployed and, therefore, set their outcomes to 0.

Demographic data. We complement the data with regional characteristics such as the unemployment rate and the share of people living in a deprived area at the municipal level. These data are publicly available on the French Statistical Office website, INSEE. As the finest location information available in the panel of workers is the municipality, we can only determine whether an individual is located in a municipality with neighborhoods classified as a Sensitive Urban Area, which serves as a proxy for the local labor market conditions.

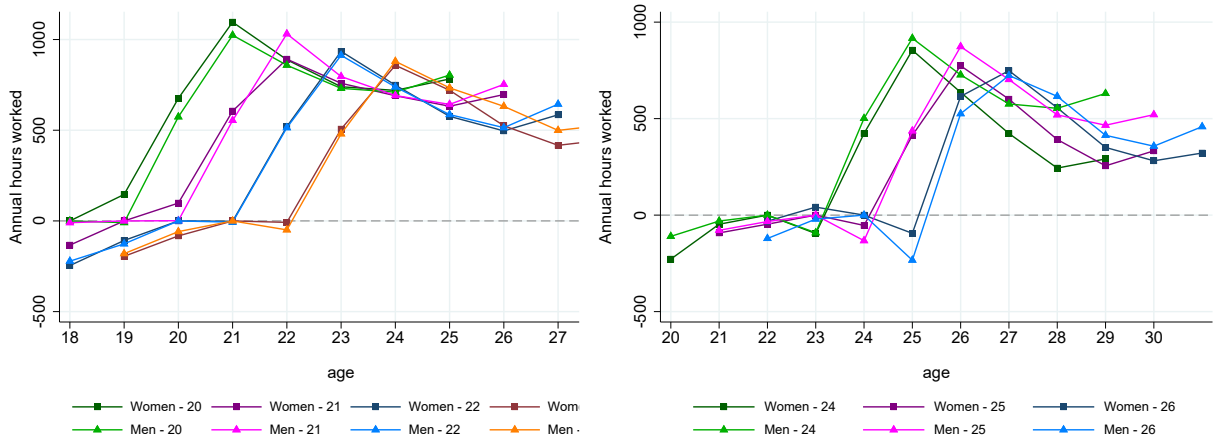
Construction of the panel of analysis. We do not have a unique employee identifier allowing us to merge social security and census data with data on JFF contracts. Therefore, to identify the recipients in the DADS panel, we use the date of birth, gender, municipal identifier of residence, firm identifier, types of contracts, and the date the contract began. We identify around 5,315 treated individuals (about 2.5% of the overall treated) in the panel of workers.[11]

[10] The main occupation is defined as the one providing the largest income and the highest number of hours worked.

[11] Only beneficiaries born between January 2nd and 5th, April 1st and 4th, July 1st and 4th, or October 1st and 4th are likely to be in the panel of workers – the scope of the DADS panel merged with the EDP. 19,000 JFF contracts signed between 2012 and 2015 involve an individual born on one of those days. Overall, 224,204 individuals benefited from JFF contracts between 2012 and 2015.

4.3.2 Descriptive Statistics

We start with a descriptive analysis of participants' outcomes. For this part, we restrict the sample to participants between 20 and 26 to have enough observations for obtaining cohort-specific effects.[12] This provides us with important insights into the employment trajectories of beneficiaries. We begin with simple averages over the life cycle by cohort and normalize these averages to two years prior to program entry. In Figure 4.3.1, we plot the average annual hours worked over the life cycle by gender for each cohort.



(a) Cohort 20-23 **(b) Cohorts 24-26**

Figure 4.3.1: Average Hours Worked over the Life Cycle by Cohort and Gender

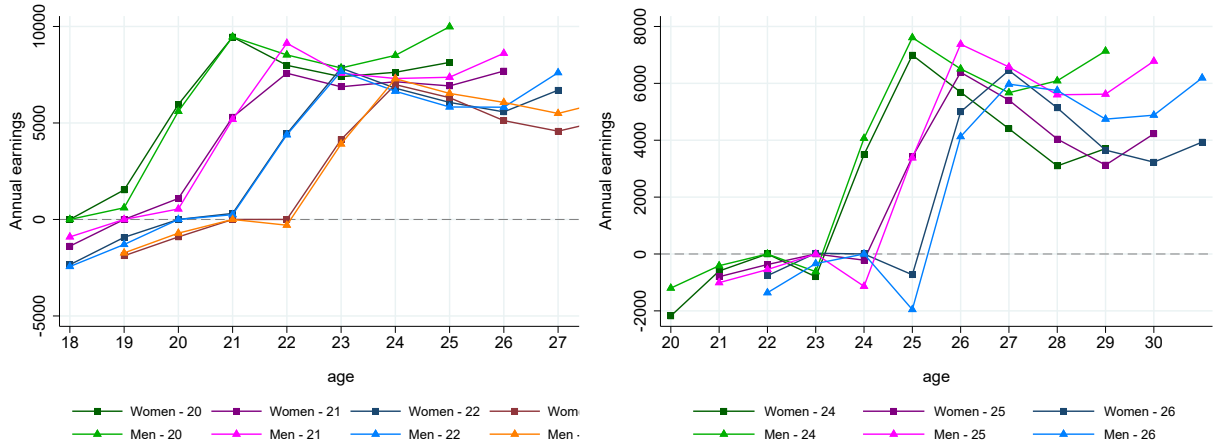
Notes: These figures plot the average hours worked over the life cycle by age at treatment and gender. The averages are normalized to two years before entering the treatment. The cohort treated between 20 and 23 are on panel (a), and those treated between 24 and 26 are on panel (b).

Four patterns stand out. First, we observe that the older individual who enter the program, the worse their situation is in the year before entering. For cohorts treated between 20 and 23, individuals have a relatively stable situation before starting the program. In contrast, for the cohorts treated at 24 or later, we observed a "dip" in labor market outcomes before entering the program. This "dip" is a well-known phenomenon in the literature – the so-called Ashenfelter (1978) dip. The dip represents the selection into the active labor market policy out of experiencing worse labor market outcomes, such as unemployment. Here, we show that individuals entering the program at older ages seem to have a larger dip, suggesting the situation

[12] Cohorts of participants outside of this range are too small.

before joining the program worsens with age. Second, the Ashenfelter (1978) dip appears to be more severe for men than women. Men tend to enter the program with worse employment histories than women. Third, the effectiveness of the program varies by age at the time of treatment. Focusing on the average 3-4 years after the program's start (the average end of the subsidized contract), we observe that the number of hours worked decreases slightly but remains significantly higher than before treatment. This improvement, however, diminishes with age at treatment. Cohorts treated at 20-21 work on average 700 hours after the program. In contrast, older cohorts tend to work fewer than 500 hours. Fourth, the effectiveness of the policy differs by gender for older cohorts. While we observe hardly any differences between men and women for the cohorts treated between 20 and 22, we observe some differences for cohorts treated at 23 or later. The gap between men and women at the end of the program seems to widen with age at treatment. It is worth noting that there are no significant differences between men and women during the program regarding hours worked and earnings (see Figure 4.3.2).

This exercise offers key insights into participants' trajectories. It suggests that women do not benefit from the program as much as men, even though men typically start subsidized employment from a more disadvantaged position. The differences between men and women become larger when individuals are treated at a later age.



(a) Cohort 20-23 **(b) Cohorts 24-26**

Figure 4.3.2: Average Earnings over the Life Cycle by Cohort and Gender

Notes: These figures plot the average annual earnings over the life cycle by age at treatment and gender. The averages are normalized to two years before entering the treatment. The cohort treated between 20 and 23 are on panel (a), and those treated between 24 and 26 are on panel (b).

One may wonder whether these differences by gender stem from initial differences in the training and support individuals get during the subsidized contract. When describing the JFF program in the Section 4.2.2, we noted differences between men and women regarding the probability of having an open-ended contract and working in the private sector. Women are also less likely to have such contracts in this subsample (see Tables 4.A.10 and 4.A.11 in Appendix 4.A). This could arguably affect the effectiveness of the policy. We come back to this in Section 4.6. Men tend to have more support at the program's start, potentially because they have a worse background. There are otherwise no substantial differences between men and women regarding the JFF package.

4.4 Empirical Strategy

Section 4.4.1 explains our empirical strategy based on the age cut-off and eligibility criteria and details our baseline specification of a dynamic Difference-in-Differences model. With this approach, we assume that treated individuals would have had the life-cycle trajectory of youth with similar educational levels and unemployment history, had they not experienced the Ashenfelter dip at a certain age. Under this assumption, the estimated effect of the program reflects both the recovery from this dip and any additional gains in employment outcomes relative to the life-cycle trajectory of a comparable control group, which was eligible but slightly too old to participate in the program at the time of its introduction. Later on, we plan to relax this assumption and use a matching method to bring together more comparable – i.e., all experiencing the Ashenfelter dip at a certain age – individuals in treated and control groups and isolate the effectiveness of the program conditional on experiencing a bad year.

4.4.1 Differences-in-Differences at the Age Cut-off

As discussed previously, the JFF program is targeted at youth under 25, in addition to the criteria of low educational levels, long-term unemployment experience, or residency in deprived areas (Table 4.2.1). Hence, among the youth with one of these profiles, the age restriction and the year of policy introduction create a sharp cut-off, specifically at 26 years old in 2013.

The main idea is to compare treated individuals to a control group of slightly older individuals who would have been eligible had such a policy been in place. For example, among the people meeting the criteria, someone aged 26 in 2013 is not eligible only because she is already too old. In contrast, another person aged 25 in 2013 can participate just because she was born one year later.[13]

Therefore, we use a Differences-in-Differences approach, exploiting the age cutoff in the year of program introduction. However, the younger or older the individuals are in relation to this threshold, the less comparable the treated and control groups become. Hence, we restrict our sample to individuals close to the cutoff. Namely, we compare treated individuals who were 24-26 years old when they started the JFF contract (in 2013, 2014, or 2015) to individuals who had one of the three profiles when they were aged 24 to 26 but were 27-29 in 2013 – the start of the policy implementation.[14]

As we have seen, the age at treatment matters for the effectiveness of the program, so we focus primarily on the age dimension rather than the calendar year. Formally, we regress the outcome $y_{i,t}$ of the individual i at age t on dummies $\mathbb{1}_{k(i)}$ equal to k years before/after the individual i starts working under the subsidized contract. The model is the following:

$$y_{i,t} = \sum_{k=-4}^{5} \beta_k \mathbb{1}_{k(i)} \times \mathbb{1}_{treated_i} + \gamma_i + \lambda_t + \epsilon_{i,t} \tag{4.1}$$

We allow for constant differences over age between the two groups using age fixed effects (λ_t). Using individual fixed effects (γ_i), we control for time-invariant individual characteristics and potential unobservable factors. $\epsilon_{i,t}$ denotes the error term. We cluster standard errors at the individual level, accounting for potential correlations across periods. As the individuals are treated at different ages (between 24 and 26), we use the estimator of de Chaisemartin and d'Haultfoeuille (2024) which takes effects heterogeneity across cohorts treated at different ages into account.[15]

[13] We choose the year cutoff of 2013 because only 0.6% of the JFF contracts were granted between October 2012 and December 2012. These were, in addition, granted to people under 24 when they started.

[14] As explained before, the maximum age to be eligible for the program is 25. Eligible individuals must be no older than this age at the date of contract signature, but may be 26 when they start working under such contracts. Moreover, the age in our database is not observed to the nearest month, so individuals may be in their 26th year and have started their contract before reaching the age of 26. Therefore, the individuals in our treatment group can be up to 26 years old.

[15] We use the command `did_multiplegt_dyn` available on Stata.

To address the Ashenfelter dip in our DiD design, we assume that the parallel trends assumption holds until the -2 period (two years before program entry and one year before the Ashenfelter dip), and use this last period as a reference point. This allows us to compare the outcomes of participants with the typical trajectory of comparable youth who have similar educational and unemployment backgrounds, i.e., those who would have met the program's eligibility criteria. The control group consists of individuals who would have qualified for the program if they had been born a few years later, representing the likely path participants would have followed without the dip and program enrollment. The key coefficient, β_k, estimates the program's impact on helping participants recover from the Ashenfelter dip and return to (or surpass) the expected trajectory of comparable youth in the k^{th} period after the program starts.

Sample. As we want to compare individuals benefiting from the program to the older ones who could not participate, we restrict the sample to recipients relatively close to the age threshold of 26 when starting the program. More precisely, from the 5,315 individuals treated, we retain only those between 24 and 26 years old at the start of their subsidized contract. This leaves us with 1,159 treated young workers.

The descriptive statistics for this sample before the program begins are presented in Table 4.A.3 of Appendix 4.A. Due to the target of the policy, most of the treated individuals have a vocational or professional degree, and 56% lived in a municipality with deprived areas. Women are relatively more educated than men, a well-known difference in the literature (Di Paola et al., 2023). The shares of women with a bachelor's and professional A-level are significantly higher than those of men. Conversely, the shares of men with a vocational certificate or minimum schooling are higher than those of women. Regarding the unemployment history of individuals, there is little to no difference in unemployment rates between genders. Before the program, 76.6% were employed but worked on average only 500 hours annually and earned €4,632 per year, and gender differences were not statistically significant at 5%.

The average JFF package they benefit from is described in Table 4.A.4 in Appendix 4.A. Virtually all participants have received some training and professional support, and 88% received training providing new skills. The average contract duration is about three years. Women

are more often employed in the non-market sector than men, and they are also less likely to be employed. We note again that men are more likely to be in the private sector and access open-ended contracts as a result of the program. Comparing treated women and men in terms of family background, we observe that the share of ever-married women is higher than that of married men. Women get married and also have a first child earlier (see Table 4.A.5 in Appendix 4.A), a trend also observed in population-based statistics. There are no apparent differences in the package or program duration between men and women. In short, the main difference between men and women lies in the family background and access to open-ended contracts at the start of the program.

The control group is composed of individuals between 27 and 29 years old in 2013.[16] In addition, we require them to have met the eligibility criteria – i.e., have one of the three profiles described in Table 4.2.1– when they were between 24 and 26 and the JFF program didn't exist. This leaves us with a control group of 30,663 individuals. Descriptive statistics of the control group in 2012 can be found in Tables 4.A.6 and 4.A.7 in Appendix 4.A. The sample is mainly composed of individuals with a low level of education. In 2012, around 41% of control individuals had a vocational secondary school diploma, 62% had been looking for work for at least 6 months. We, therefore, managed to select people away from the labor market in the control group. Individuals in the control group are more likely to have children, but this may be because they are older. We report average outcomes over the life cycle for the treated and control groups and by gender in Figure 4.B.1.

4.5 Results

4.5.1 The Limited Average Effects of the Subsidized Job

Employment probability. As the primary goal of ALMPs is to help people with low employment prospects find a job, we start by investigating the effect of the JFF contracts on the probability of employment. This probability equals 1 when a positive wage is reported and

[16] One might be concerned about the comparability of the control group and the 2015 treated cohort. However, the absence of a pre-trend, in addition to the age-fixed effect, provides evidence that individuals are comparable, despite the widening age gap.

0 otherwise. In Figure 4.5.1, we plot the coefficients β_k resulting from the estimation of the model (4.1). We normalize the coefficients and set β_{-2} to 0.

To causally interpret the post-period coefficients, we should first assess the plausibility of the parallel assumption – the absence of pre-trend in this setting. Focusing on the pre-period coefficients (-4 to -1), we note that most coefficients are not significantly different from 0. Only the coefficient in period -4 is significant. However, coefficients from -3 to -1 are not different from 0 and flat, so we believe that we are comparing similar individuals and can plausibly assess the causal effect of the JFF program on the probability of being employed.

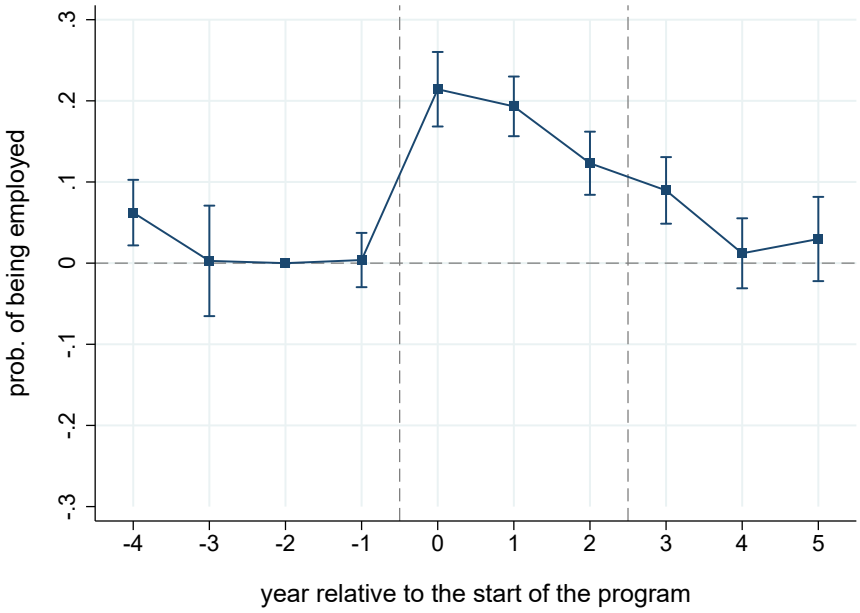


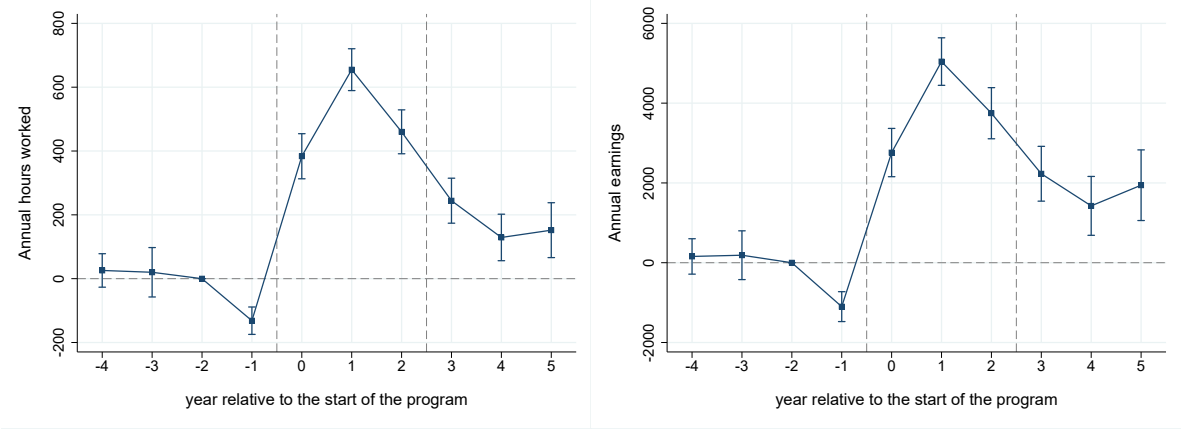
Figure 4.5.1: Effect on the Employment Probability

Notes: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1). Coefficients are normalized to period -2. The dependent variable is the probability of being employed. The first vertical line highlights the start of the program, and the second is the average end of the program in the sample.

As expected, in Figure 4.5.1, we notice that the program significantly increases the probability of being employed in the short run by about 20 percentage points, with coefficients remaining positive and large in periods 1 and 2, when subsidized employment occurs. This 20 percentage points increase represents a rise of 31% compared to the average probability of being employed by the control group 2 years before the start of the program. Effects are not constant across periods 0, 1, and 2 because the subsidized workers have different contract durations (going from 1 to 3 years). The effect begins to shrink in period 3 due to the phasing out

of the program and its potential lock-in effect. In period 4, the positive effect on employment vanishes. In the medium run, the JFF program does not significantly increase the probability of employment: the effects on the extensive margin become close to zero after the subsidized employment ends. These results align with the literature for subsidized employment, and public one in particular (Card et al., 2018; Kluve et al., 2019). Nevertheless, these findings could be considered puzzling since this program also includes training and job search assistance, which are known to have at least a small positive effect in the medium to long run.

Hours worked and earnings. To causally interpret the post-period coefficients, we should again assess the plausibility of the parallel assumption – the absence of pre-trend in this setting. Focusing on the pre-period coefficients (-4 to -2), we note that most coefficients are not significantly different from 0. Control and treated individuals evolved in the same manner until treated individuals find themself in a bad economic situation and enter the program. This materializes with a significant and negative coefficient in period -1. In this case, we estimate the combination of helping the participants get out of the Ashenfelter dip and benefiting from the program. As shown in Figure 4.5.2a, in the first two post-periods, the number of hours worked in the year increases by 600 hours. This is unsurprising, as the program was usually directed toward full-time contracts – 1820 hours per year. Then, the effects fade, but contrary to the probability of employment, remain positive and significant in the medium run. The total annual earnings follow a similar pattern. In the short term – one year after the beginning of the JFF – the annual earnings increase by just up to €5,000 due to the larger amount of hours worked, an increase of 68% on the average annual earnings of the control group in 2012. In the medium run, the effect is smaller – €2,000 or 27% – but stable over time.



(a) Effect on Hours Worked **(b) Effect on Earnings**

Figure 4.5.2: Effect on Hours Worked and Earnings

Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1). Coefficients are normalized to period -2. The dependent variable is on panel (a) the total number of hours worked in the year, and on panel (b) the total annual net wage. The first vertical line highlights the program's start, and the second is the average end of the program in the sample.

Robustness. We verify the robustness of our results using different age bandwidths. More precisely, we change our baseline bandwidths by (i) extending the age bandwidth of the control group by one year, from 27-29 in 2013 to 27-30 in 2013, and (ii) restricting the age bandwidths of the treated and control groups, from 23-26 to 25-26 and from 27-29 to 27-28, respectively. Results are reported in Figure 4.B.2 in Appendix 4.B. They are similar, underlining that our findings are not the result of the definition of the age bandwidth.

We further check the robustness of our results by changing the specification of our baseline model. First, we control non-parametrically for local shocks using local labour market by year fixed effects or county-by-year fixed effects. Results are not affected, see Figure 4.B.4 in Appendix 4.B. This suggests that those results are not driven by different economic situations between the cohorts. We also demonstrate that the standard errors remain unaffected by a change in the clustering level (see Figure 4.B.3 in Appendix 4.B). Finally, we show that we obtain similar point estimates when estimating the model on different windows (see Figure 4.B.5 in Appendix 4.B). All together, these suggest that the estimates we obtain are not an artifact of our baseline specification.

4.5.2 Gender Heterogeneity in Policy Effectiveness

As motivated in the introduction, we observe discrepancies in the unemployment rate of young women and men. In the descriptive statistics, we have further seen that there seem to be differences in the program's effectiveness by gender. We therefore investigate whether a difference in the effectiveness of subsidized employment could partly explain this. To do so, we conduct a heterogeneity analysis along the gender dimension using a fully interacted model.

Employment probability. The baseline effects are a weighted average of marginally positive effects for men and null effects for women (see Figure 4.5.3). The coefficients for men remain positive and significant after the end of subsidized employment (see Table 4.A.8 in Appendix 4.A). In contrast, the effects for women are not significantly different from zero, driving the average zero result presented in the previous section.

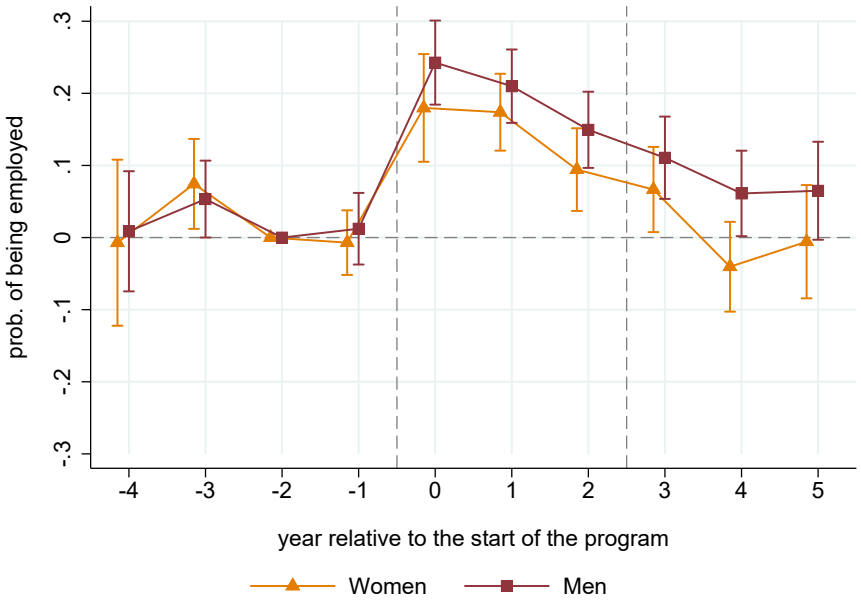
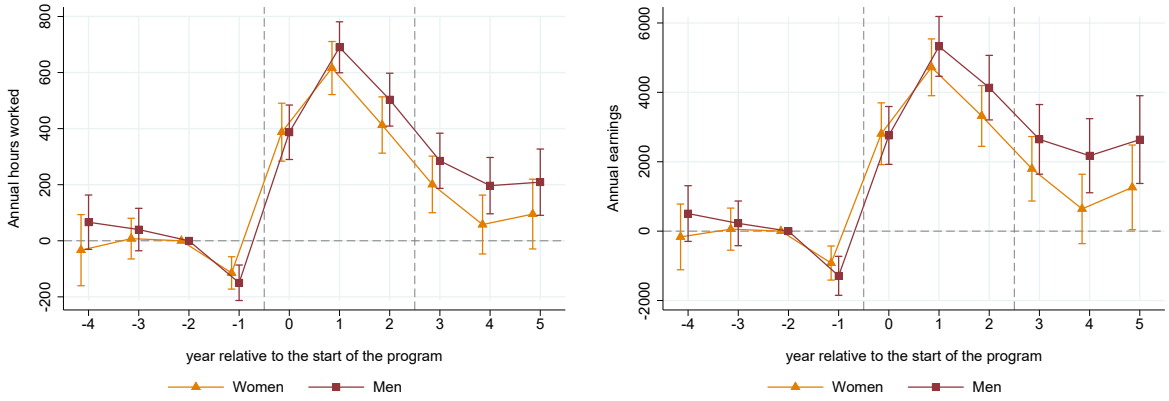


Figure 4.5.3: Effect on the Probability of Being Employed by Gender

Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1) fully interacted with the gender dummy. Coefficients are normalized to period -2. In red, we observe the effects for men, and in orange for women. The dependent variable is the probability of being employed. The first vertical line highlights the program's start, and the second is the average end of the program in the sample.

Hours worked and earnings. In Figure 4.5.4, we estimate the gender-specific effects of participating in the program. In the short run, the program promotes both women and men to

similar positions in terms of earnings and hours worked. Coefficients in periods 0 and 1 do not significantly differ for each gender. From period two onward, the gap begins to widen at the end of the program. The effect on women in the medium term is null, while it remains positive and significant for men. At the end of the program, men work 200 hours more and earn up to €2,600 more than they would have if they had not participated (see Table 4.A.8 in Appendix 4.A). The coefficient for women in period 5 is significantly different from 0, but the gap between men and women remains substantial: men earn approximately €1,300 more than women.



(a) Effect on Hours Worked **(b) Effect on Earnings**

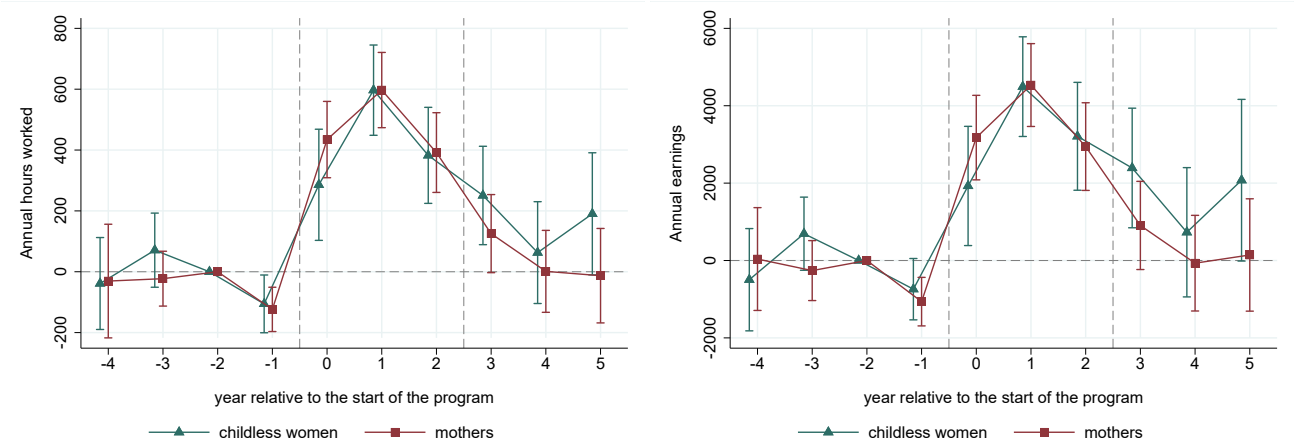
Figure 4.5.4: Effect on Hours Worked and Earnings by Gender

Notes: Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1) fully interacted with the gender dummy. Coefficients are normalized to period -2. The dependent variable is on panel (a) the total number of hours worked in the year, and on panel (b) the annual earnings. The first vertical line highlights the program's start, and the second is the average end of the program in the sample. In red, we observe the effects for men, and in orange for women. The first vertical line highlights the program's start, and the second is the average end of the program in the sample.

4.5.3 Parental Status and Policy Effectiveness

Since childbirth is considered to be the main factor for the remaining gender inequality in the labor market (Goldin et al., 2022), we look into the consequences of becoming parents when participating in the program. This time, we restrict the sample to men or women and interact the model with a dummy indicating whether the individual became a parent at some point. We do not observe much heterogeneity in terms of parental status for men (see Figure 4.B.7 in Appendix 4.B). However, in Figure 4.5.5, we observe that the program has a positive effect on hours worked and earnings for childless women (the coefficient in period 5 is significantly

different from 0 at 10% confidence level; see Table 4.A.9 in Appendix 4.A). We do not find significant differences in the probability of being employed by parenthood for women (see Figure 4.B.6 in Appendix 4.B).



(a) Effect on Hours Worked **(b) Effect on Earnings**

Figure 4.5.5: Effect on Women by Parental Status

Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1) fully interacted with a dummy equal to 1 if the woman is ever a parent in the period we observe. Coefficients are normalized to period -2. The dependent variable is on panel (a) the total number of hours worked in the year, and on panel (b) the annual earnings. The first vertical line highlights the program's start, and the second is the average end of the program in the sample. In red, we observe the effects for mothers, and in green for childless women. The first vertical line highlights the program's start, and the second is the average end of the program in the sample.

It is important to note here that these effects are not driven by the systematic age differences between the control and treatment groups at the date of birth of the first child (see Figure 4.B.8 in Appendix 4.B). In particular, for women, we cannot reject the null hypothesis that the distributions are the same: using a Mann–Whitney Test, we find a p-value of 0.901. In Figure 4.B.9 in Appendix 4.B, we further show that women benefiting from the program are less likely to have a child during subsidized employment.

4.6 Mechanisms

We have seen that the program's effectiveness differs substantially by gender; women benefit less, and mothers in particular do not seem to take full advantage of this program. Section 4.6.1 aims to identify plausible mechanisms explaining why women do not perform as well as

men. Section 4.6.2 aims to understand why mothers do not benefit from the program at the end of it.

4.6.1 Explaining the Gender Heterogeneity: Differences in Occupations

Table 4.6.1 displays the average share of JFF beneficiaries by gender and industries at the start of their subsidized contract. It also displays the average share of women in the workplace. In addition, it also shows indicators related to tension in the labor market for the occupation and the task content of the occupation during subsidized employment.[17] There are three takeaways from this table. First, during the subsidized contract, women are employed in firms where most workers are female, which is partly explained by their overrepresentation in the healthcare and social sectors. Women are three times more likely to work in the Health and social sectors than men. In contrast, men are more likely to be in the Public Administration and Art sectors. Second, men are more likely to work in jobs with higher hiring intensities and a higher link between training and the occupation – meaning that replacing such a worker is potentially more costly. On the other hand, women are more likely to be in jobs with labor shortages, which suggests an intense workload. Third, women are more likely to be assigned to jobs with a high routine index, while men are more likely to have both non-routine cognitive tasks and manual ones. Additionally, we have observed that men were more likely to have an open-ended contract when they started the program (see Table 4.A.4 in Appendix 4.A).

Altogether, this suggests that despite aiming to fight against gender sorting across industries, the JFF program actually further contributes to it.[18] The program does not seem to provide the same opportunities for men and women, as it first does not introduce them to equivalent occupations. The source of the sorting remains, however, ambiguous for now. It could stem from the employee's preferences, the employer's decisions, or the recommendations provided by the caseworker.

[17] For the indicators related to the tensions in the labor market, these are provided by the French Ministry of Labor and are derived by region*occupation. These indicators do not cover occupations in the public sector and are sometimes not reported when the quality of the indicator is considered low. We use the measures adapted to the French occupational classification by Le Barbanchon and Rizzotti (2020) for the task content indexes; unfortunately, not all positions matched the task content.

[18] See https://www.gouvernement.fr/action/les-emplois-d-avenir on the objective to fight against gender sorting: "Priority is also given to those whose jobs contribute to balancing the gender ratio in a sector of activity."

Table 4.6.1: Characteristics of the Subsidized Employment by Gender

	Women		Men		
	Mean	Nb. obs	Mean	Nb. obs	Diff
Share of female colleagues	0.686	578	0.422	573	0.263***
Public sector	0.479	582	0.390	577	0.089***
Industry					
G-Trade	0.052	582	0.069	577	-0.018
N-Admin services	0.072	582	0.094	577	-0.021
O-Public Admin.	0.216	582	0.314	577	-0.097***
Q-Health&Social	0.328	582	0.106	577	0.222***
R-Art&Show	0.041	582	0.080	577	-0.038***
S-Other Services	0.069	582	0.059	577	0.010
Tension in the LM					
high hiring intensity	0.297	461	0.397	443	-0.100***
high labor shortage	0.228	461	0.093	443	0.135***
high unsustainable empl.	0.833	461	0.822	443	0.011
high training-empl. link	0.134	461	0.228	443	-0.094***
high bad working cond.	0.471	461	0.560	443	-0.089***
high geographic mismatch	0.223	461	0.309	443	-0.086***
high tightness	0.130	461	0.115	443	0.015
Task content of the JFF job					
high Routine index	0.719	392	0.652	417	0.067**
high Routine cognitive	0.385	392	0.365	417	0.021
high Routine manual	0.610	392	0.741	417	-0.131***
high Non-routine analytic	0.102	392	0.132	417	-0.030
high Non-routine cognitive	0.161	392	0.360	417	-0.199***
high Non-routine manual	0.564	392	0.791	417	-0.228***

Notes: Sector and type of firms treated women and men were employed during the JFF. *Share of female* is the average share expressed as a percentage of the firm workforce. All the other characteristics are expressed as a share of women or men. For the indicators related to the tensions in the labor market, these are provided by the French Ministry of Labor and are derived by region and occupation. These indicators do not cover occupations in the public sector and are sometimes not reported when the quality of the indicator is considered low. We use the measures adapted to the French occupational classification by Le Barbanchon and Rizzotti (2020) for the task content indexes, unfortunately, not all positions matched the task content. Characteristics related to the tension in the labor market and the task content are dummies equal to one when the value of the index is above the median of the occupations. These statistics are made on the treated group, obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*.

4.6.2 Understanding the Motherhood Penalty

No self-selection into motherhood. When describing the sample of estimations, we have seen that treated women are more likely than treated men to have a child. However, mothers do not seem to self-select into the treatment. The age distribution at first birth does not differ between treated mothers and their counterfactuals (see Figure 4.B.8 in Appendix 4.B). On top of that, women are less likely to have a child after entering the program. In Figure 4.B.9

in Appendix 4.B, we estimate that treated women are less likely to have a kid after starting subsidized employment.

Effects of daycare. The availability of childcare is an important determinant of women's participation in the labor market (see e.g., Bauernschuster and Schlotter, 2015). Mothers may reduce their labor participation due to increasing opportunity costs after giving birth. To verify whether this hypothesis holds in our setting, we study the effect of the availability of daycare spots in the place of residence in 2013 – or at the start of the program when treated, on mothers' employment rates and hours worked. We define a municipality as a location with high availability when the number of spots per thousand inhabitants exceeds the median of the sample.[19] We then conduct a heterogeneity analysis using the baseline model fully interacted with a dummy equal to if the availability is high – above the median. The results suggest that mother beneficiaries do not reduce their labor supply because of a high opportunity cost due to a lack of daycare (see Figure 4.B.11 in Appendix 4.B). The number of hours worked remains the same regardless of the availability of daycare spots.

The timing of childbirth matters. To better understand how childbirth is associated with the program's ineffectiveness for mothers, we examine how the timing of the first childbirth affects the policy's impact on women. We restrict the sample to women and estimate the interacted model with parental status and the timing of childbirth – before, during, or after participation in the program – as interaction variables. To investigate the effect of the timing of parenthood relative to the start of the program, we also need to define a timeline for the control group. Therefore, we take the age of 25 as a reference point. Individuals of the control group giving birth before 25 years old serve as the control for the evaluation of the effect of having a kid before the start of the program, between 25 and 30 for those having a kid during, and more than 30 for those having a kid after.

Results can be found in Figure 4.B.10 in Appendix 4.B. The employment probability of mothers who have their child during or immediately after subsidized employment barely im-

[19] We use the daycare facilities data from Tricaud (2025).

proves during the subsidized contract and becomes negative by the end. The effects on childless women and mothers who had their children before the program are null.

The positive effects on hours worked and earnings are driven by women who have no kids or who have one before the start of the program. Women who have children later see no improvement in any of their outcomes. Women entering the program with a child may have already organized the daycare for their child. In contrast, women who have a child during the program may not have readily available solutions, and the program may not have been designed to address such situations. Women having a kid during the program might have no choice but to drop out. This program presumably fails to introduce young mothers to the labor market in a permanent way.

4.7 Conclusion

In this paper, we have analyzed the effects of subsidized employment with training and assistance on the labor market outcomes of disadvantaged youth by studying the case of the "Jobs for Future" program in France. We use a Differences-in-Differences approach, exploiting the cutoff by age 25 in the policy. Estimating average effects, we document that the JFF program fails to improve the employment prospects of beneficiaries in the medium run and that those who manage to stay employed after the program, on average, work 200 hours more per year and raise their annual net earnings by €2,000. However, we highlight that those average effects hide heterogeneity between men and women.

While subsidized employment significantly improves employment probability for men, women do not experience higher rates of employment after the end of the program. Even if the effect is significantly positive for hours worked and earnings, male beneficiaries perform better than women again. This difference seems to be driven by women who become mothers during the program, who do not experience this positive effect.

Therefore, our findings suggest that policymakers should take gender heterogeneity and young parenthood into account when active labor market policies targeted at youth are designed. We also document pronounced gender-based sorting into different occupations in terms

of job type and quality. Therefore, the effectiveness of the policy could be potentially increased by supporting beneficiaries who become parents and accounting for the sorting. Further research can also be done on whether the event and timing of childbirth have similar consequences on the effectiveness of ALPMs targeted at older workers with low employment prospects.

4.A Additional Tables

Table 4.A.1: Characteristics of Participants to the JFF Program

	All		Women		Men		Diff. in mean
	Mean	Sd.	Mean	Sd.	Mean	Sd.	Women - Men
Share of men	0.481	0.500	0.000	0.000	1.000	0.000	-1.000
Municipality with deprived area	0.574	0.495	0.582	0.493	0.565	0.496	0.017***
Age at JFF start	22.837	1.723	22.852	1.724	22.820	1.722	0.032***
Degree							
non certified skills	0.000	0.020	0.000	0.017	0.000	0.022	-0.000**
Master degree	0.000	0.007	0.000	0.008	0.000	0.005	0.000
Bachelor	0.034	0.180	0.047	0.211	0.019	0.137	0.028***
Professional bachelor	0.040	0.195	0.051	0.220	0.027	0.163	0.024***
A-level	0.056	0.231	0.059	0.235	0.054	0.226	0.005***
Professional A-level	0.149	0.356	0.167	0.373	0.129	0.335	0.038***
Vocational training	0.099	0.298	0.081	0.273	0.118	0.322	-0.037***
Vocational degree	0.375	0.484	0.384	0.486	0.366	0.482	0.017***
Unemployment history							
Less than 6 months	0.296	0.457	0.292	0.455	0.300	0.458	-0.008***
6-11 months	0.270	0.444	0.262	0.440	0.278	0.448	-0.016***
12-23 months	0.234	0.423	0.234	0.424	0.233	0.423	0.001
24+ months	0.117	0.322	0.115	0.320	0.119	0.324	-0.003**
Not reported	0.083	0.276	0.096	0.294	0.069	0.254	0.026***
Eligibility criteria							
Profile 1	0.160	0.367	0.132	0.338	0.191	0.393	-0.059***
Profile 2	0.225	0.417	0.235	0.424	0.214	0.410	0.022***
Profile 3	0.549	0.498	0.552	0.497	0.546	0.498	0.006***
Not matched	0.066	0.248	0.081	0.273	0.050	0.218	0.031***
Start of the JFF contract							
2012	0.004	0.064	0.004	0.066	0.004	0.062	0.001*
2013	0.282	0.450	0.285	0.452	0.279	0.449	0.006***
2014	0.274	0.446	0.276	0.447	0.272	0.445	0.003*
2015	0.200	0.400	0.195	0.396	0.205	0.404	-0.010***
Observations	224,204		116,251		107,953		224,204

Notes: Demographic and employment characteristics of all the JFF beneficiaries and for women and men separately. All the characteristics except for the *Age at JFF start* are expressed as a share of the JFF beneficiaries. *Municipality with deprived area* identifies JFF beneficiaries living in a Priority rural development area (*Zone de Revitalisation Rurale – ZRR*) or in a city with deprived neighborhoods (*Zone Urbaine Sensible – ZUS* or *Quartier Prioritaire de Ville – QPV*). The three profiles for *Eligibility criteria* are described in subsection 4.2.1. These statistics are from the data source listing all the JFF contracts created during the period 2012-2015 (*Emplois d'Avenir* – EA) restricted to individuals benefiting from the program when they are between 20 and 26. Column (4) reports the differences between the means of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 4.A.2: Characteristics of the JFF Packages

	All		Women		Men		Diff. in mean
	Mean	Sd.	Mean	Sd.	Mean	Sd.	Women - Men
Duration of the JFF package in days	751.173	357.320	725.768	357.738	778.531	354.842	-52.762***
Early termination	0.357	0.479	0.337	0.473	0.380	0.485	-0.043***
Monthly gross wage in euros	1401.678	1403.827	1370.806	1358.354	1434.993	1450.570	-64.188***
Adapt position to the worker	0.823	0.382	0.810	0.392	0.836	0.371	-0.025***
Learning acreditiomn	0.028	0.164	0.031	0.172	0.024	0.154	0.006***
Contract type							
Fulltime	0.829	0.376	0.772	0.420	0.891	0.312	-0.119***
Open ended	0.214	0.410	0.164	0.370	0.269	0.443	-0.105***
Private sector	0.247	0.431	0.165	0.371	0.336	0.472	-0.171***
Support							
Social support	1.000	0.000	1.000	0.000	1.000	0.000	0.000
Back to work	0.254	0.436	0.243	0.429	0.267	0.442	-0.023***
Position	0.841	0.366	0.828	0.378	0.855	0.352	-0.027***
Project	0.500	0.500	0.516	0.500	0.482	0.500	0.034***
Skill	0.510	0.500	0.507	0.500	0.513	0.500	-0.006***
Job search	0.226	0.418	0.222	0.416	0.229	0.421	-0.007***
Training							
Pre-qualifying	0.080	0.272	0.095	0.293	0.065	0.246	0.030***
Refresher	0.091	0.288	0.089	0.285	0.093	0.291	-0.004***
New skills	0.884	0.321	0.885	0.319	0.882	0.323	0.004***
Qualifying	0.301	0.459	0.298	0.457	0.305	0.460	-0.007***
Certifying	0.056	0.231	0.056	0.230	0.057	0.231	-0.001
Observations	224,204		116,251		107,953		224,204

Notes: Characteristics of the JFF contracts for all beneficiaries and for women and men separately. The wage is expressed in deflated euros. Except for *Duration of the JFF package*, all the other characteristics are expressed as a share of JFF contracts. *Early termination* identifies JFF contracts that ended before the end date provided at the beginning of the contract. These statistics are from the data source listing all the JFF contracts created during the period 2012-2015 (*Emplois d'Avenir* – EA) restricted to individuals benefiting from the program when they are between 20 and 26. Column (4) reports the differences between the means of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 4.A.3: Treated Individuals before the Start of the Subsidized Job

	(1) All		(2) Women		(3) Men		(4) Diff. in mean
	Mean	Sd.	Mean	Sd.	Mean	Sd.	Women - Men
General characteristics							
Share of men	0.498	0.500	0.000	0.000	1.000	0.000	-1.000
Age	23.680	0.718	23.643	0.696	23.718	0.737	-0.075*
Municipality with deprived area	0.557	0.497	0.595	0.491	0.518	0.500	0.076***
Degree							
Non certified skills	0.003	0.051	0.002	0.041	0.003	0.059	-0.002
Bachelor	0.022	0.148	0.022	0.148	0.023	0.149	-0.000
22.0000	0.002	0.042	0.003	0.059	0.000	0.000	0.003
23.0000	0.015	0.120	0.022	0.148	0.007	0.083	0.015**
Professional bachelor	0.053	0.223	0.077	0.267	0.028	0.164	0.050***
A-level	0.057	0.232	0.069	0.253	0.045	0.208	0.024*
Professional A-level	0.119	0.324	0.143	0.350	0.095	0.294	0.047**
Vocational training	0.118	0.323	0.084	0.278	0.153	0.360	-0.068***
Vocational degree	0.388	0.488	0.404	0.491	0.373	0.484	0.031
Employment history							
Age at JFF start	24.680	0.718	24.643	0.696	24.718	0.737	-0.075*
Prob. of being employed	0.766	0.423	0.787	0.410	0.745	0.436	0.042*
Annual earnings	4632.789	5034.156	4556.877	4699.340	4709.358	5353.713	-152.480
Annual hours worked	501.473	537.632	505.895	518.845	497.012	556.354	8.883
Average hourly wage	7.159	6.775	7.107	4.792	7.210	8.314	-0.103
Unemployment duration							
less than 6 month	0.251	0.434	0.241	0.428	0.262	0.440	-0.021
6-11 months	0.274	0.446	0.259	0.439	0.289	0.454	-0.030
12-23 months	0.252	0.434	0.258	0.438	0.246	0.431	0.012
24+ months	0.141	0.348	0.149	0.357	0.132	0.338	0.018
Not reported	0.082	0.274	0.093	0.290	0.071	0.257	0.022
Eligibility criteria							
Profile 1	0.182	0.386	0.122	0.328	0.243	0.429	-0.121***
Profile 2	0.215	0.411	0.225	0.418	0.205	0.404	0.021
Profile 3	0.504	0.500	0.527	0.500	0.480	0.500	0.047
Not matched	0.099	0.299	0.125	0.331	0.073	0.260	0.053***
Observations	1,159		582		577		1,159

Notes: Characteristics of all JFF workers and of women and men separately, in our sample of analysis, one year before the start of their subsidized job. Earnings are expressed in net deflated euros. Ages are expressed in years. All the other characteristics are expressed as a share of JFF workers. *Municipality with deprived area* identifies JFF beneficiaries living in a Priority rural development area (*Zone de Revitalisation Rurale – ZRR*) or in a city with deprived neighborhoods (*Zone Urbaine Sensible – ZUS* or *Quartier Prioritaire de Ville – QPV*). These statistics are made on a sample of people between 24 and 26 years old at the start of their subsidized contract, obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*. Column (4) reports the differences between the means of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 4.A.4: Treated Individuals in the First Year of the Subsidized Job

	(1) All		(2) Women		(3) Men		(4) Diff. in mean
	Mean	Sd.	Mean	Sd.	Mean	Sd.	Women - Men
Annual earnings	9172.420	4545.200	8777.957	4354.507	9570.301	4700.080	-792.344***
Annual hours worked	1068.601	506.727	1028.689	494.308	1108.860	516.260	-80.171***
Average hourly wage	8.230	1.107	8.203	1.076	8.258	1.138	-0.054
Duration JFF (years)	3.069	1.080	3.086	1.078	3.052	1.082	0.034
Early termination	.308	.462	.288	.453	.329	.470	-0.040
Municipality with deprived area	0.586	0.493	0.622	0.485	0.549	0.498	0.073**
Leaning accreditation	.033	.178	.031	.174	.034	.183	-0.003
Adapt position	0.843	.363	0.835	.371	0.851	.356	-0.016
Contract							
Open ended	.212	.409	.175	.385	0.249	0.433	-0.074**
Full time	.890	.312	.845	.361	.936	.916	-0.09***
Cohort							
2013	0.346	0.476	0.352	0.478	0.340	0.474	0.013
2014	0.375	0.484	0.376	0.485	0.374	0.484	0.002
2015	0.279	0.449	0.271	0.445	0.286	0.452	-0.014
Occupation							
Not observed - no salaried work	0.006	0.078	0.005	0.072	0.007	0.083	-0.002
Farmers, craftsmen, business owners	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Managerial and professional occupations	0.005	0.072	0.007	0.083	0.003	0.059	0.003
Intermediate professions	0.115	0.319	0.113	0.317	0.116	0.321	-0.003
Employees	0.616	0.487	0.761	0.427	0.470	0.500	0.291***
Workers	0.258	0.438	0.113	0.317	0.404	0.491	-0.290***
Support							
Social support	0.112	0.316	0.101	0.302	0.123	0.329	-0.022
Back to work	0.243	0.429	0.220	0.415	0.267	0.443	-0.047*
Position	0.851	0.357	0.845	0.362	0.856	0.351	-0.011
Project	0.478	0.500	0.478	0.500	0.478	0.500	-0.001
Skill	0.521	0.500	0.536	0.499	0.506	0.500	0.030
Job search	0.224	0.417	0.201	0.401	0.248	0.432	-0.047*
Training							
Pre-qualifying	0.068	0.252	0.081	0.273	0.055	0.229	0.025*
Refresher training	0.103	0.304	0.108	0.311	0.097	0.296	0.011
New skills	0.882	0.323	0.885	0.319	0.879	0.327	0.006
Qualifying	0.298	0.457	0.275	0.447	0.321	0.467	-0.046*
Observations	1,159		582		577		1,159

Notes: Characteristics of the JFF contracts and JFF workers retained in our sample of analysis in the first year of their subsidized job, for all the beneficiaries and women and men separately. Earnings are expressed in net deflated euros and duration in years. All the other characteristics are expressed as a share of JFF workers. The three profiles for *Eligibility criteria* are described in subsection 4.2.1. Column (4) reports the differences between the mean of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively. These statistics are made on a sample of people between 24 and 26 years old at the start of their JFF contract. They are obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*.

Table 4.A.5: Family Background of Treated Women and Men

	(1) Women		(2) Men		(3) Diff. in mean
	Mean	Sd.	Mean	Sd.	
Nb. of marriages	0.235	0.441	0.149	0.356	0.086***
Nb. of children	1.031	1.016	0.678	0.915	0.353***
Ever married	0.229	0.420	0.149	0.356	0.079***
Ever parenting	0.619	0.486	0.447	0.498	0.171***
Age at first marriage	25.707	2.915	26.523	2.660	-0.816**
Age at first child	24.300	3.708	25.837	3.243	-1.537***
Timing of the first kid					
Before JFF	0.316	0.465	0.137	0.344	0.179***
During JFF	0.129	0.335	0.128	0.335	0.001
After JFF	0.174	0.379	0.182	0.386	-0.008
Observations	582		577		1,159

Notes: Family background of women and men JFF workers retained in our sample throughout the analysis. The number of weddings and children report the number over the period. Ages are expressed in years. All the other characteristics are expressed as a share of JFF workers. Column (3) reports the differences between the mean of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively. These statistics are made on a sample of people between 24 and 26 years old, obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*.

Table 4.A.6: Characteristics of the Control Group

	(1) Control group		(2) Treatment group		(3) Diff. in mean
	Mean	Sd.	Mean	Sd.	Contol-Treated
General characteristics					
Share of men	0.522	0.500	0.498	0.500	0.024
Age	27.000	0.810	22.747	1.026	4.252***
Municipality with deprived area	0.530	0.499	0.545	0.498	-0.016
Completed degree					
Not observed	0.280	0.449	0.003	0.051	0.277***
Elementary school	0.000	0.000	0.106	0.308	-0.106***
Secondary School - general	0.041	0.198	0.121	0.326	-0.080***
Secondary School - vocational	0.411	0.492	0.515	0.500	-0.104***
Gen./Voc./Tech. A-level	0.161	0.367	0.179	0.383	-0.018
Bachelor degree	0.108	0.310	0.054	0.225	0.054***
Graduate	0.000	0.000	0.023	0.149	-0.023***
Employment history					
Prob. of being employed	0.648	0.478	0.763	0.426	-0.115***
Annual earnings	7301.207	8191.409	5068.125	5254.068	2233.082***
Annual hours worked	730.581	774.312	568.987	589.821	161.594***
Average hourly wage	6.186	7.064	6.714	4.348	-0.528**
Public sector	0.125	0.330	0.193	0.395	-0.069***
Occupation					
not observed - no salaried work	0.353	0.478	0.244	0.430	0.109***
Farmers, craftsmen, business owners	0.001	0.030	0.000	0.000	0.001
Managerial and professional occupations	0.014	0.119	0.007	0.083	0.007**
Intermediate professions	0.073	0.260	0.071	0.257	0.002
Employees	0.282	0.450	0.410	0.492	-0.128***
Workers	0.276	0.447	0.268	0.443	0.008
Working contract					
Open-ended	0.305	0.460	0.186	0.390	0.118***
Fixed-term	0.167	0.373	0.299	0.458	-0.132***
Temporary	0.089	0.285	0.115	0.319	-0.026***
Apprenticeship and internship	0.003	0.055	0.038	0.191	-0.035***
Subsidized jobs	0.021	0.144	0.076	0.265	-0.055***
Others	0.000	0.006	0.000	0.000	0.000
No contract	0.042	0.201	0.035	0.185	0.007
Not working	0.352	0.478	0.237	0.426	0.115***
Missing	0.021	0.143	0.014	0.117	0.007
Unemployment duration					
Looking for a job for at least 6 months	0.624	0.485	0.726	0.446	-0.103***
Looking for a job for at least 12 months	0.479	0.500	0.443	0.497	0.037**
Observations	30,663		1,159		31,822

Notes: Characteristics in 2012 of people in the control group retained in our sample of analysis. Earnings are expressed in net deflated euros. Age is expressed in years. All the other characteristics are expressed as a share of people in the sample. *Municipality with deprived area* identifies JFF beneficiaries living in a Priority rural development area (*Zone de Revitalisation Rurale – ZRR*) or in a city with deprived neighborhoods (*Zone Urbaine Sensible – ZUS* or *Quartier Prioritaire de Ville – QPV*). Column (3) reports the differences between the mean of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively. These statistics are made on the control group obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*.

Table 4.A.7: Descriptive Statistics of the Control and Treatment Group in 2012

	(1)		(2)		(3)
	Control group		Treatment group		Diff. in mean
	Mean	Sd.	Mean	Sd.	Contol-Treated
Nb. marriages	0.280	0.468	0.192	0.403	0.088***
Nb. children	1.382	1.245	0.855	0.982	0.526***
Ever married	0.272	0.445	0.189	0.392	0.083***
Ever parenting	0.684	0.465	0.533	0.499	0.151***
Age at first marriage	27.081	4.006	26.027	2.840	1.054***
Age at first child	26.127	4.145	24.942	3.599	1.186***
Timing of the first kid					
Before JFF or 25	0.259	0.438	0.227	0.419	0.032**
During JFF or 25-29	0.263	0.440	0.129	0.335	0.135***
After JFF or after 29	0.162	0.368	0.178	0.382	-0.016
Observations	30,663		1,159		31,822

Notes: Family background of people in the control group retained in our sample throughout the analysis. The number of weddings and children reports the number over the period. Ages are expressed in years. All the other characteristics are expressed as a percentage of the entire sample. Column (3) reports the differences between the means of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively. These statistics are made on the control group obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*

Table 4.A.8: Dynamic Differences-in-Differences Estimates by Gender

	Employment Probability		Annual Hours Worked		Annual Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	Women	Men	Women	Men	Women	Men
F4	-0.0071	0.0087	-33.50	66.18	-166.95	506.59
	(0.0587)	(0.0425)	(64.72)	(49.48)	(483.18)	(409.45)
F3	0.0745	0.0534	7.74	40.11	57.35	224.40
	(0.0318)	(0.0272)	(37.03)	(38.60)	(309.59)	(328.66)
F2	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
F1	-0.0070	0.0123	-114.68	-149.79	-920.01	-1290.00
	(0.0229)	(0.0253)	(29.44)	(32.28)	(250.39)	(286.52)
L0	0.1798	0.2427	387.27	387.01	2806.31	2757.95
	(0.0381)	(0.0297)	(52.75)	(49.55)	(454.56)	(425.34)
L1	0.1739	0.2099	616.09	690.05	4722.37	5325.16
	(0.0272)	(0.0260)	(48.29)	(46.37)	(417.79)	(439.77)
L2	0.0943	0.1495	412.92	503.38	3320.32	4135.75
	(0.0292)	(0.0270)	(51.22)	(48.10)	(447.13)	(474.58)
L3	0.0667	0.1107	200.96	285.34	1796.84	2644.99
	(0.0301)	(0.0291)	(51.50)	(50.20)	(473.93)	(512.10)
L4	-0.0404	0.0612	57.91	196.71	640.46	2173.78
	(0.0318)	(0.0302)	(53.57)	(51.26)	(510.12)	(544.87)
L5	-0.0057	0.0649	95.23	209.04	1261.47	2638.24
	(0.0401)	(0.0347)	(63.49)	(60.31)	(622.76)	(645.14)
Individual FE	✓		✓		✓	
Age FE	✓		✓		✓	
Observations	338,184		338,184		338,184	

Notes: This table reports coefficients and standard errors in parentheses of the estimated differences-in-differences model fully interacted with a gender dummy. Coefficients are normalized to period F2.

Table 4.A.9: Dynamic Differences-in-Differences Estimates by Parenthood for Women

	Employment Probability		Annual Hours Worked		Annual Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	Childless	Mothers	Childless	Mothers	Childless	Mothers
F4	-0.0495	0.0193	-38.66	-30.62	-496.53	37.81
	(0.0752)	(0.0849)	(77.08)	(95.29)	(674.20)	(677.82)
F3	0.0853	0.0681	70.92	-22.70	692.81	-260.50
	(0.0466)	(0.0416)	(62.17)	(45.98)	(482.12)	(395.27)
F2	0.0000	0.0000	0.00	0.00	0.00	0.00
	(0.0000)	(0.0000)	(0.00)	(0.00)	(0.00)	(0.00)
F1	0.0090	-0.0152	-105.60	-123.74	-740.90	-1060.00
	(0.0355)	(0.0300)	(48.44)	(37.16)	(404.54)	(319.89)
L0	0.1202	0.2150	285.79	434.43	1926.75	3176.42
	(0.0610)	(0.0490)	(93.20)	(64.00)	(785.95)	(557.34)
L1	0.1011	0.2175	596.87	597.18	4493.50	4534.55
	(0.0412)	(0.0360)	(75.69)	(63.09)	(657.21)	(546.14)
L2	0.0694	0.1046	382.58	391.71	3210.10	2944.14
	(0.0429)	(0.0396)	(80.53)	(66.84)	(711.34)	(578.01)
L3	0.0662	0.0630	250.70	125.30	2391.25	906.04
	(0.0442)	(0.0409)	(82.50)	(65.48)	(788.51)	(581.45)
L4	-0.0523	-0.0421	62.93	1.32	729.99	-69.07
	(0.0482)	(0.0423)	(85.39)	(68.69)	(852.39)	(630.63)
L5	0.0500	-0.0443	190.41	-12.93	2075.11	142.93
	(0.0555)	(0.0551)	(102.36)	(79.19)	(1065.86)	(740.92)
Individual FE	✓		✓		✓	
Age FE	✓		✓		✓	
Observations	160,556		160,556		160,556	

Notes: This table reports coefficients and standard errors in parentheses of the estimated differences-in-differences model fully interacted with a dummy equal to 1 if the individual has at least one child at some point. The sample is restricted to women. Coefficients are normalized to period F2.

Table 4.A.10: Profiles of Treated Individuals

	Women	Men	Diff in mean
Municipality with deprived area	0.594	0.532	0.061***
Age at JFF start	22.238	22.143	0.095
Unemployment history			
less than 6 month	0.274	0.280	-0.006
6-11 months	0.277	0.307	-0.029**
12-23 months	0.239	0.234	0.005
24+ months	0.105	0.104	0.000
missing	0.104	0.075	0.030***
Degree			
non certified skills	0.000	0.001	-0.001
bachelor	0.029	0.016	0.013***
Professional bachelor	0.048	0.017	0.031***
A-level	0.075	0.058	0.017**
Professional A-level	0.131	0.091	0.040***
vocational training	0.097	0.166	-0.069***
vocational degree	0.386	0.382	0.004
Start of the JFF contract			
2013	0.371	0.373	-0.002
2014	0.378	0.361	0.018
2015	0.250	0.266	-0.016
Eligibility criteria			
Profile 1	0.143	0.221	-0.078***
Profile 2	0.205	0.221	-0.016
Profile 3	0.516	0.486	0.030*
Not matched	0.136	0.072	0.064***
Observations	4059		

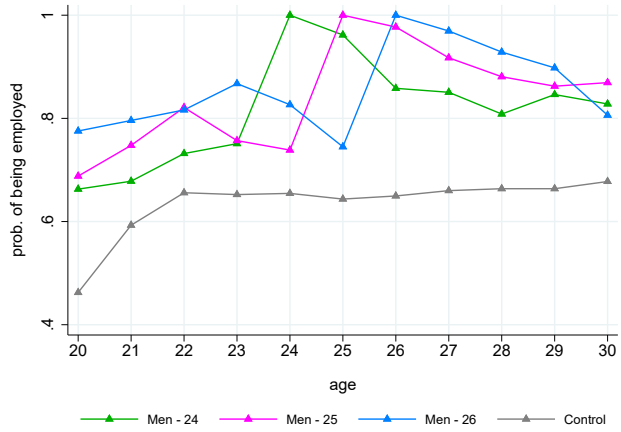
Notes: Characteristics of all JFF workers and of women and men separately, in our sample of analysis one year before the start of their subsidized job. Earnings are expressed in net deflated euros. Ages are expressed in years. All the other characteristics are expressed as a share of JFF workers. *Municipality with deprived area* identifies JFF beneficiaries living in a Priority rural development area (*Zone de Revitalisation Rurale – ZRR*) or in a city with deprived neighborhoods (*Zone Urbaine Sensible – ZUS* or *Quartier Prioritaire de Ville – QPV*). These statistics are made on a sample of people between 20 and 26 years old at the start of their subsidized contract, obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*. Column (3) reports the differences between the mean of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively.

Table 4.A.11: Packages of Treated Individuals

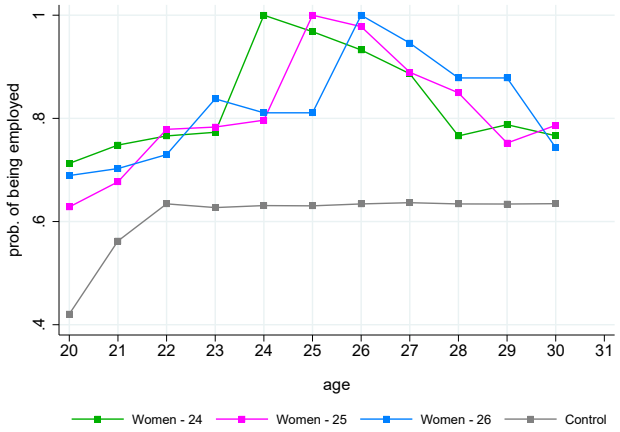
	Women	Men	Diff in mean
Share of men	0.000	1.000	-1.000
Early termination	0.310	0.339	-0.029*
Open ended_ea	0.170	0.245	-0.075***
JFF in private sector	0.181	0.313	-0.132***
Adapt position	0.821	0.845	-0.024**
leaning accreditation	0.038	0.025	0.013**
Support			
Social support	0.109	0.120	-0.010
Back to work	0.234	0.273	-0.039***
Position	0.825	0.860	-0.034***
Project	0.527	0.498	0.028*
Skill	0.506	0.516	-0.010
Job search	0.224	0.255	-0.031**
Training			
Pre-qualifying	0.113	0.070	0.043***
refresherF	0.106	0.114	-0.008
New skills	0.889	0.881	0.008
Qualifying	0.273	0.308	-0.035**
Certified goal	0.053	0.069	-0.017**
Observations	4059		

Notes: Characteristics of the JFF contracts and JFF workers retained in our sample of analysis in the first year of their subsidized job, for all the beneficiaries and women and men separately. Earnings are expressed in net deflated euros and duration in years. All the other characteristics are expressed as a share of JFF workers. The three profiles for *Eligibility criteria* are described in subsection 4.2.1. Column (3) reports the differences between the mean of women and men, as well as the significance level of this difference resulting from a t-test. ***, ** and * denote significance at 1%, 5% and 10%, respectively. These statistics are based on a sample of people between 20 and 26 years old at the start of their JFF contract. They are obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*.

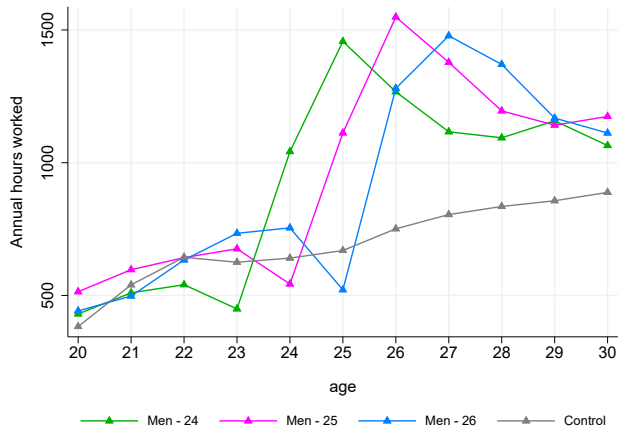
4.B Additional Figures



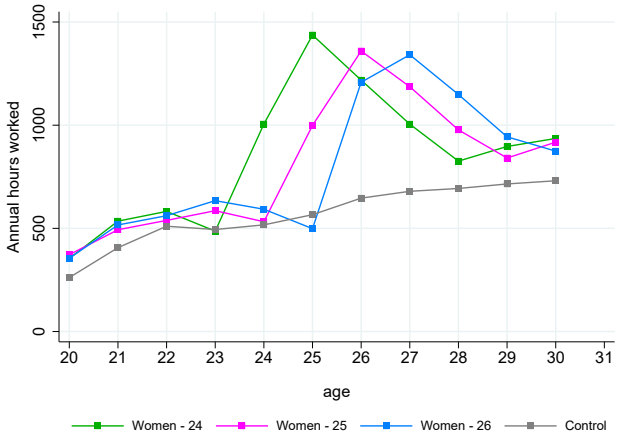
(a) Employment Probability – Men



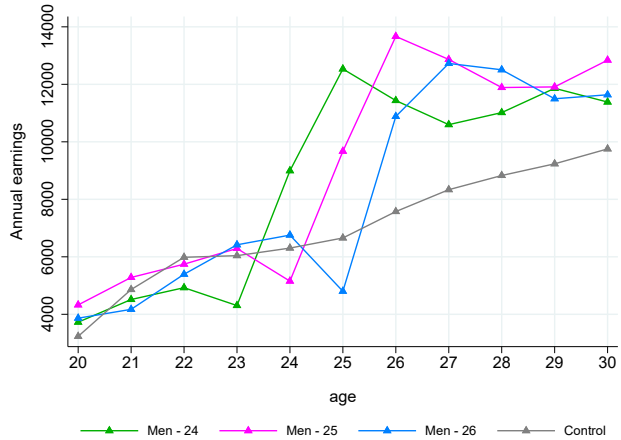
(b) Employment Probability – Women



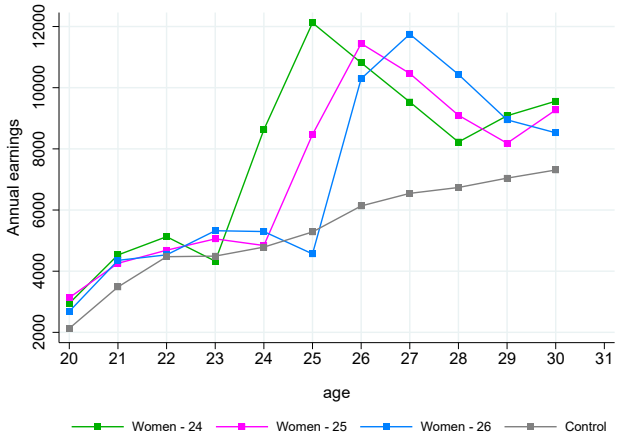
(c) Hours Worked – Men



(d) Hours Worked – Women



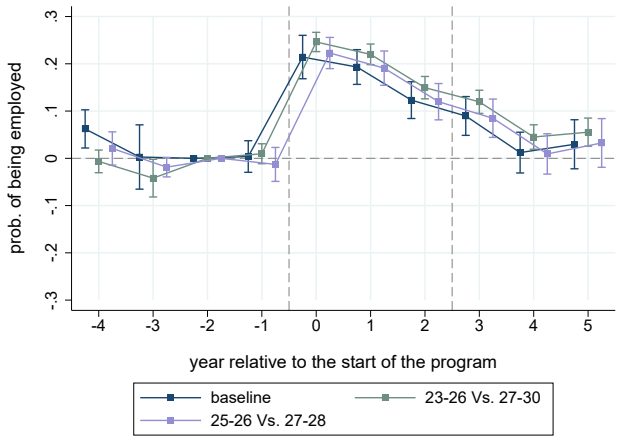
(e) Earnings – Men



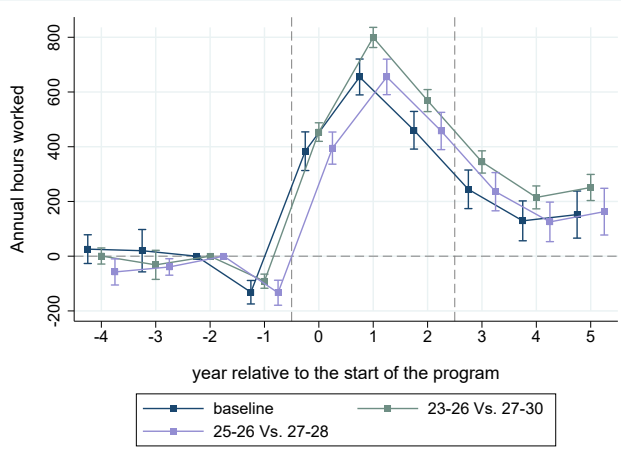
(f) Earnings – Women

Figure 4.B.1: Averages Outcomes over the Life Cycle by Treatment, Cohort and Gender

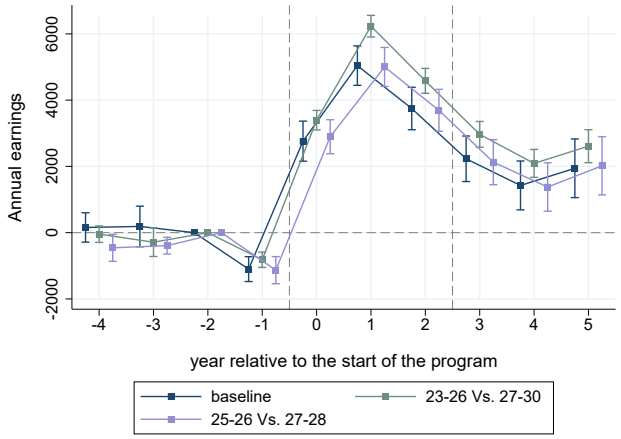
Notes: These figures plot averages over the life cycle for the treated cohorts and the control group. The dependent variable on panel (a) and (b) is the probability of being employed, panel (c) and (d) the total number of hours worked in the year, and panel (d) and (e) the annual earnings.



(a) Effect on Employment Probability



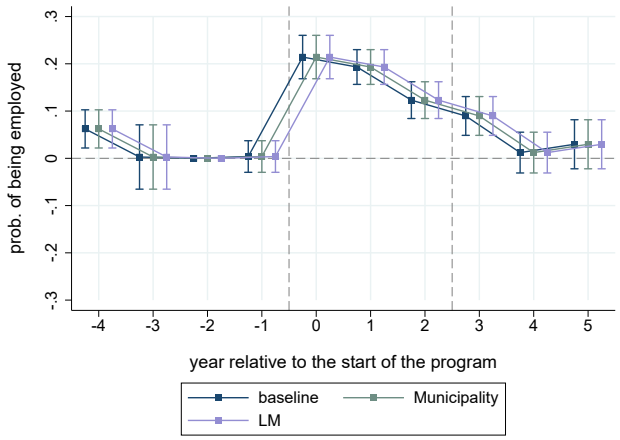
(b) Effect on Hours Worked



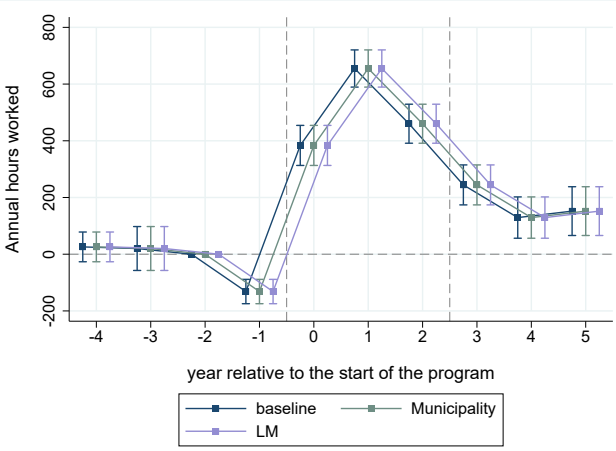
(c) Effect on Earnings

Figure 4.B.2: Robustness: Different Age Bandwidths

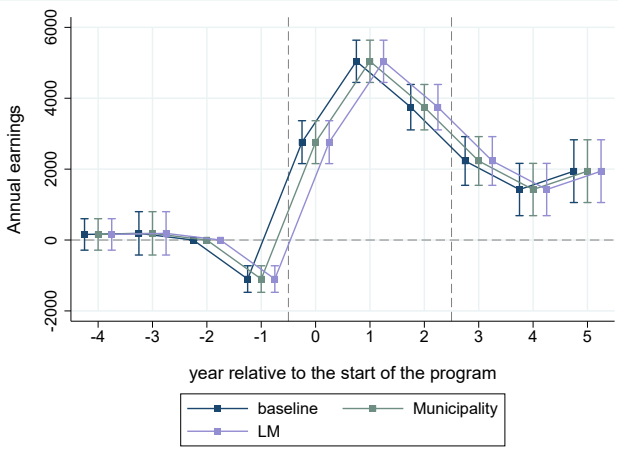
Notes: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1). Coefficients are normalized to period -2. The baseline is plotted in blue, the larger bandwidths in green, and the smaller in violet. The dependent variable is the probability of being employed in panel (a), the total hours worked in panel (b), and the annual net wage in panel (c). The first vertical line highlights the start of the program, and the second is the average end of the program in the sample.



(a) Effect on Employment Probability



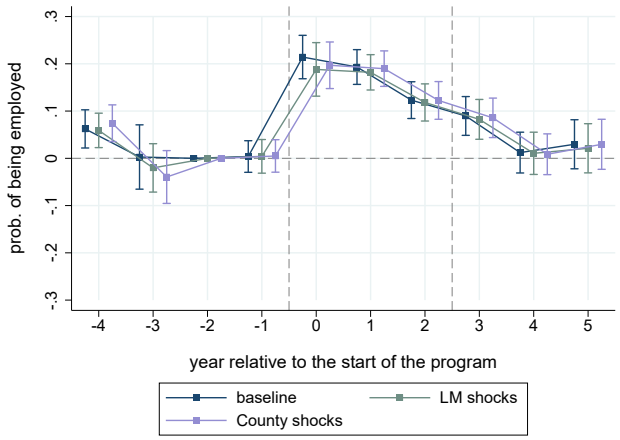
(b) Effect on Hours Worked



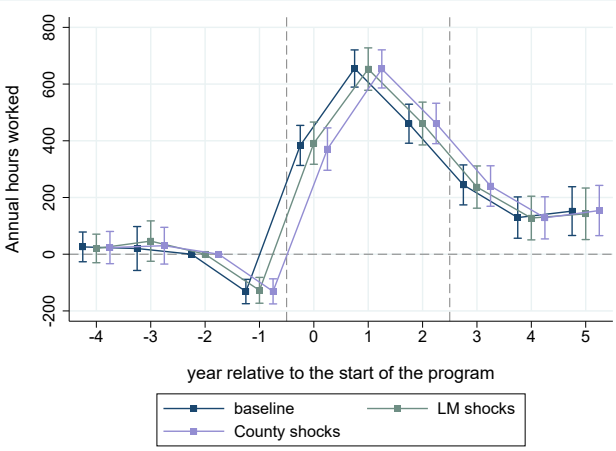
(c) Effect on Earnings

Figure 4.B.3: Robustness: Different Clustering Levels.

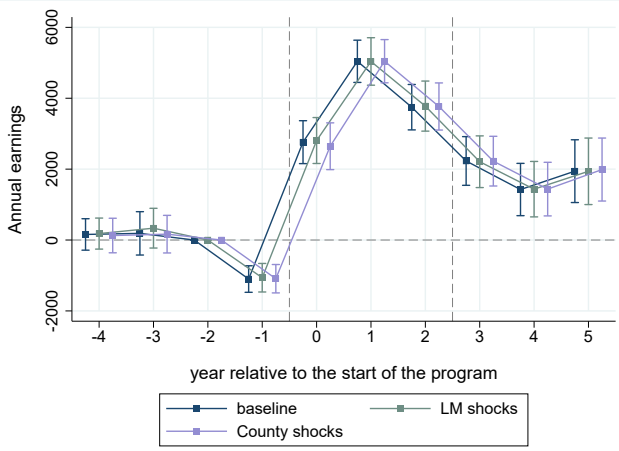
Notes: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1). Coefficients are normalized to period -2. The baseline is plotted in blue. In green, we report point estimates with standard errors clustered at the municipality level, and in violet, we report point estimates with standard errors clustered at the Local labor market level. The dependent variable is the probability of being employed in panel (a), the total hours worked in panel (b), and the annual net wage in panel (c). The first vertical line highlights the start of the program, and the second is the average end of the program in the sample.



(a) Effect on Employment Probability



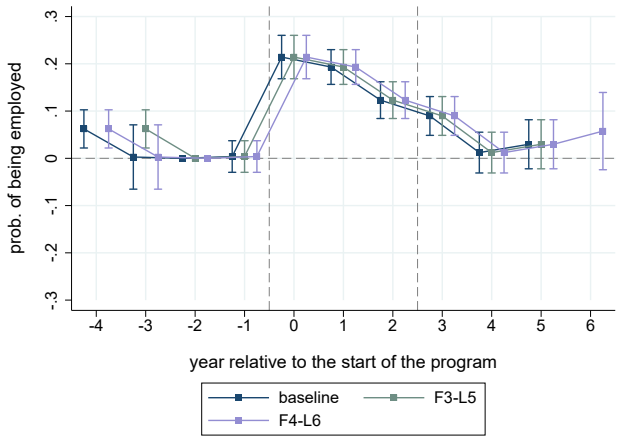
(b) Effect on Hours Worked



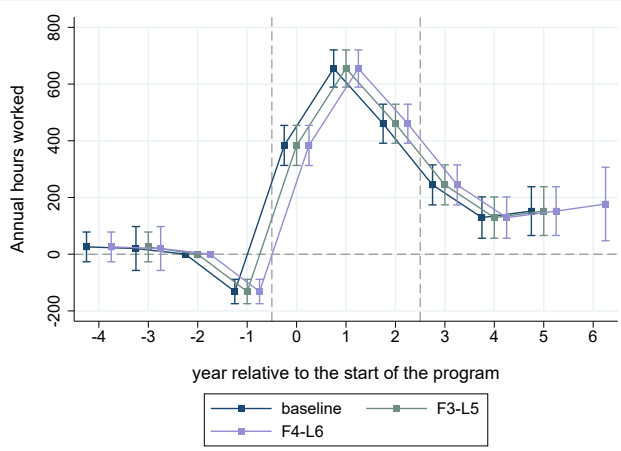
(c) Effect on Earnings

Figure 4.B.4: Robustness: Additional Controls.

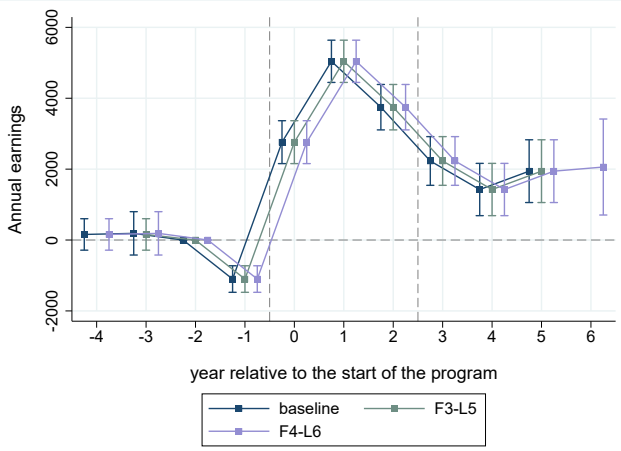
Notes: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1). Coefficients are normalized to period -2. The baseline is plotted in blue, in green we further control for local shocks at the labor market level, and in violet we further control for local shocks at the county level. The dependent variable is the probability of being employed in panel (a), the total hours worked in panel (b), and the annual net wage in panel (c). The first vertical line highlights the start of the program, and the second is the average end of the program in the sample.



(a) Effect on Employment Probability



(b) Effect on Hours Worked



(c) Effect on Earnings

Figure 4.B.5: Robustness: Different Windows.

Notes: This figure plots the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1). Coefficients are normalized to period -2. The baseline is plotted in blue. In green, we report point estimates of a similar model with 3 leads and 5 lags, and in violet, we report point estimates of a similar model with 4 leads and 6 lags. The dependent variable is the probability of being employed in panel (a), the total hours worked in panel (b), and the annual net wage in panel (c). The first vertical line highlights the start of the program, and the second is the average end of the program in the sample.

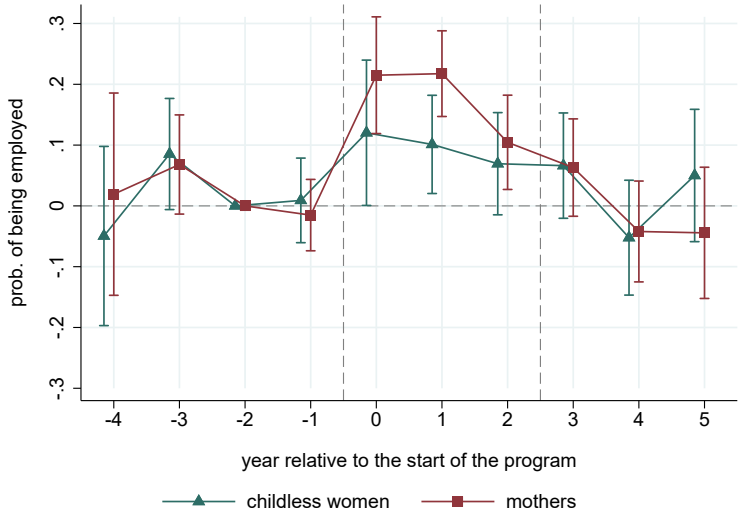
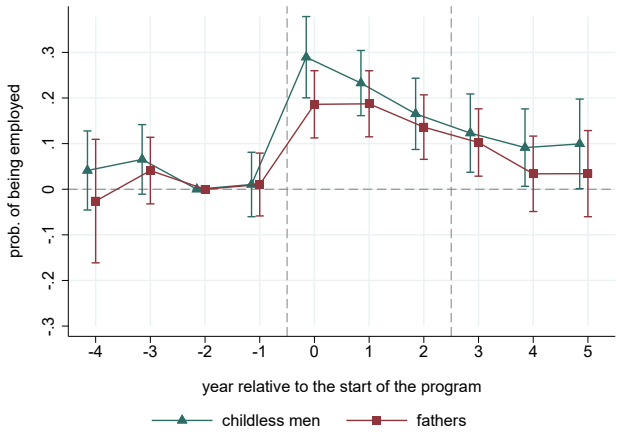
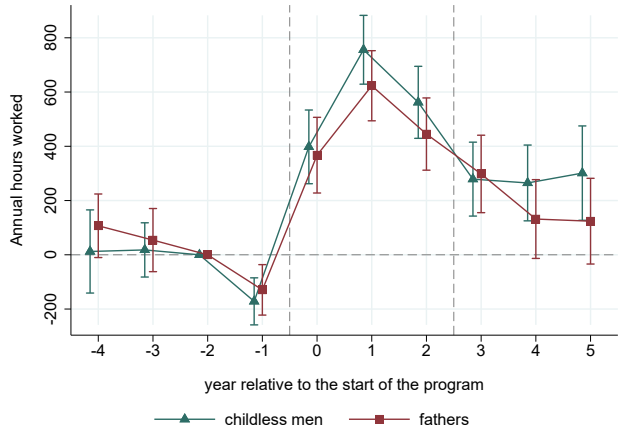


Figure 4.B.6: Effect on the Employment Probability of Women by Parenthood

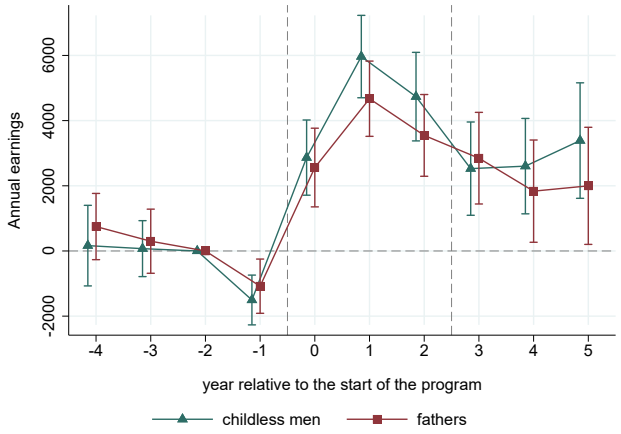
Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1) fully interacted with a dummy equal to 1 if the individual is ever a parent in the period we observe. Coefficients are normalized to period -2. The dependent variable is the probability of being employed. In red, we observe the effects for mothers, and in green for childless women. The first vertical line highlights the program's start, and the second is the average end of the program in the sample.



(a) Effect on Employment Probability



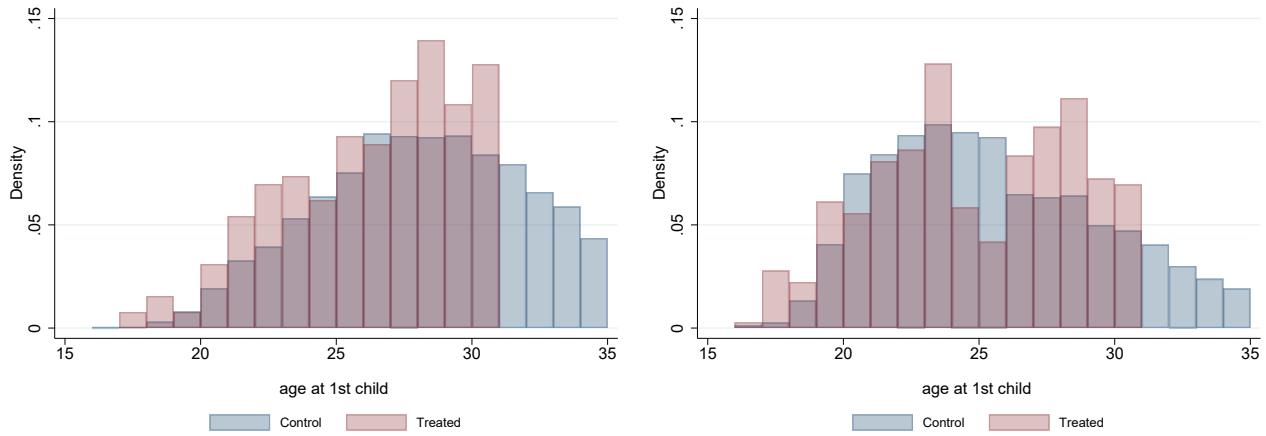
(b) Effect on Hours Worked



(c) Effect on Earnings

Figure 4.B.7: Effects on Men by Parental Status

Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1) fully interacted with a dummy equal to 1 if the individual is ever a parent in the period we observe. Coefficients are normalized to period -2. The dependent variable on panel (a) is the probability of being employed, panel (b) the total number of hours worked in the year, and panel (c) the annual earnings. The first vertical line highlights the program's start, and the second is the average end of the program in the sample. In red, we observe the effects for fathers and in green for men. The first vertical line highlights the program's start, and the second is the average end of the program in the sample.



(a) Men **(b) Women**

Figure 4.B.8: Distribution of Age at First Childbirth

Notes: These figures plot the distributions of age at the birth of the first kid by treatment status for both men – panel(a) – and women – panel(b). The age distribution ends at 31 for treated individuals, as we do not observe them for a longer period. These statistics are made on the treated and control groups obtained by merging the databases *Panel tous salariés-EDP* and *Emplois d'Avenir*.

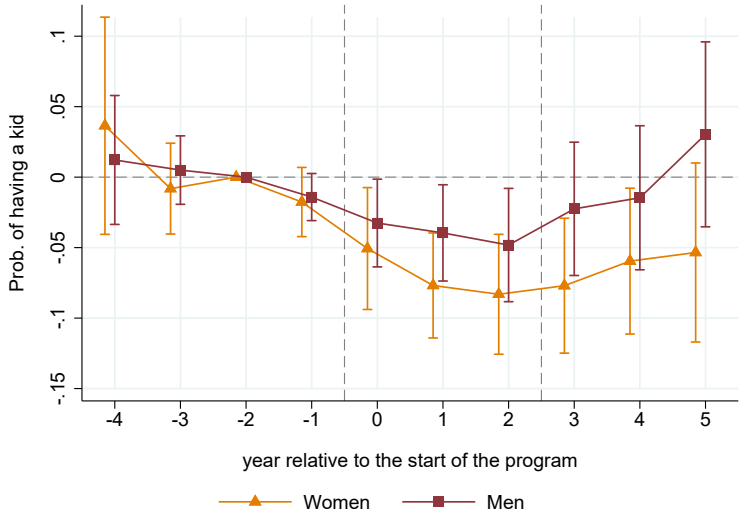
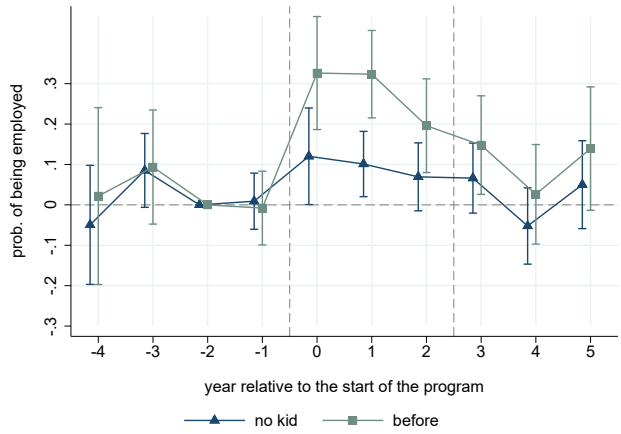
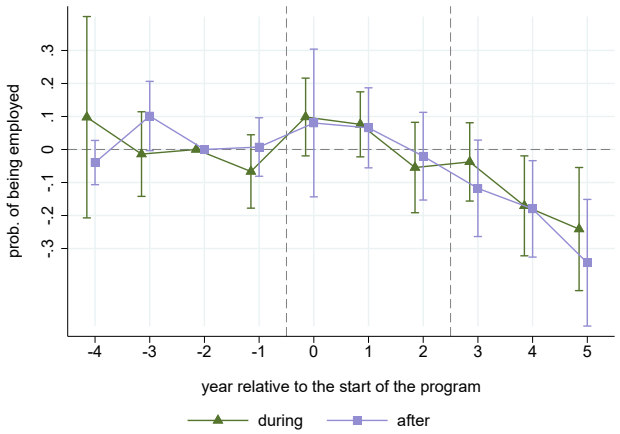


Figure 4.B.9: Effect on the Probability of Having a Child by Gender

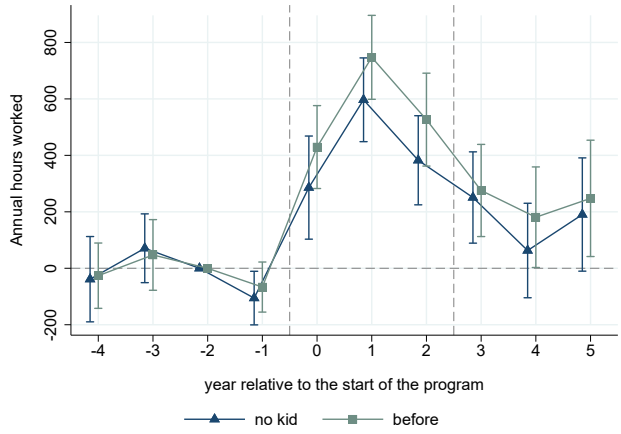
Notes: This figure plots the event study estimates and corresponding 95 percent confidence bands of the interacted specification (4.1). The model is interacted with the gender dummy. In red, we observe the effects for men, and in orange for women. The dependent variable is the probability of having a first kid. The first vertical line highlights the start of the program, and the second is the average end of the program in the sample.



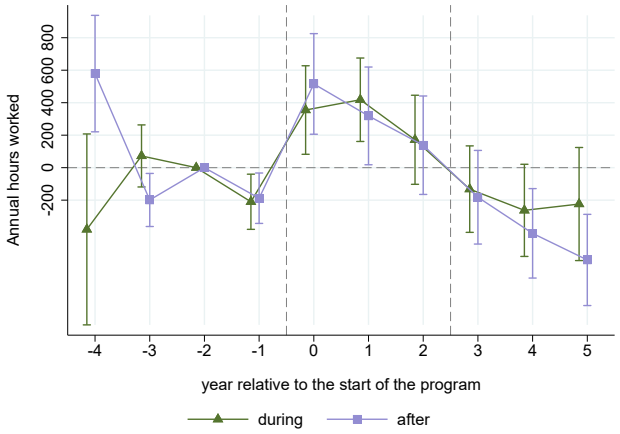
(a) Effect on Hours Worked - No Kid/Before



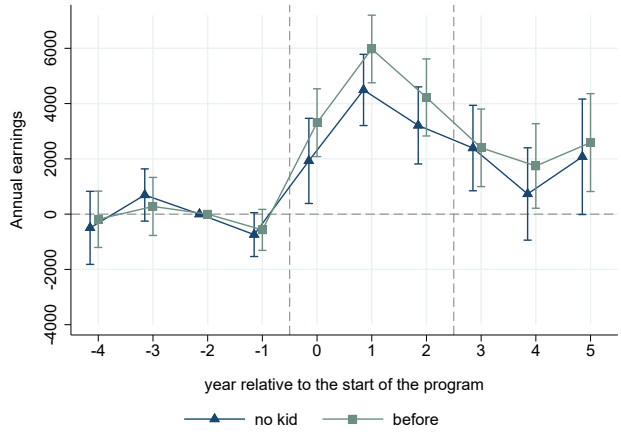
(b) Effect on Hours Worked - During/After



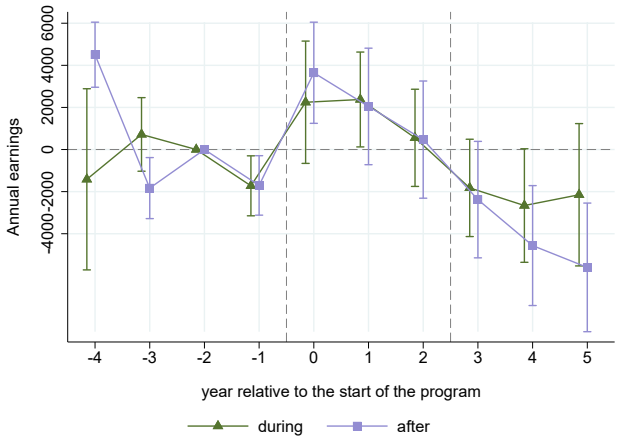
(c) Effect on Employment Prob. - No kid/Before



(d) Effect on Employment Prob. - During/After



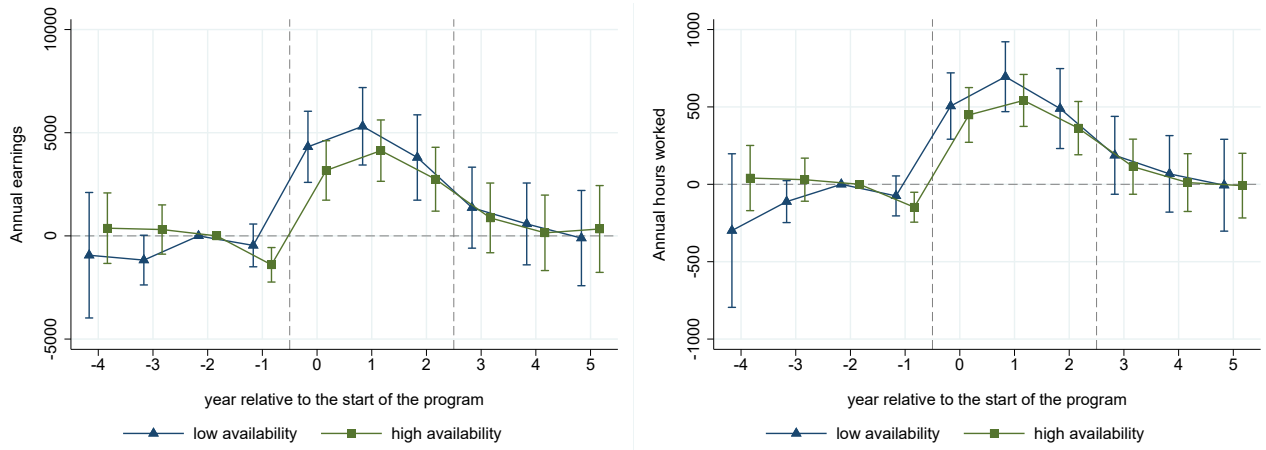
(e) Effect on Earnings - No kid/Before



(f) Effect on Earnings - During/After

Figure 4.B.10: Effects on Women by the Timing of Parenthood

Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the specification (4.1) fully interacted with dummies, accounting for the timing of the birth of the first child to the program (no kid, before, during, or after). Coefficients are normalized to period -2. The dependent variable on panel (a) and (b) is the probability of being employed, panel (c) and (d) the total number of hours worked in the year, and panel (d) and (e) the annual earnings. It is estimated on the sample of women.



(a) Effect on the Employment Probability **(b) Effect on Hours Worked**

Figure 4.B.11: Effects on Mothers by Daycare Availability

Notes: These figures plot the event study estimates (β_k) and corresponding 95 percent confidence bands of the interacted specification (4.1). The model interacts with the municipal daycare spot availability in 2013. In yellow, we observe the effects for people living in a municipality with a number of daycare spots below the median and in green above the median. The dependent variable is on panel (a) the probability of being employed and on panel (b) the total number of hours worked in the year. The first vertical line highlights the start of the program, and the second is the average end of the program in the sample.

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List of Applied Software

- Stata MP 17.0: Applied to perform the empirical analyses in Chapters 2-4.
- RStudio: used to import data in Chapters 2-4.
- TeX Live Version 2021: Used to compile the drafts of the single papers and the final thesis.