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ABSTRACT

Who Benefits from Tax Incentives? The Heterogeneous Wage Incidence of a Tax Credit*

Do workers gain from lower business taxes, and why? We estimate how a large corporate income tax credit in France is passed on to wages and explore the firm- and employee-level underlying mechanisms. The amount of tax credit firms get depends on their payroll share of workers paid less than a wage threshold. Exposure to the policy thus varies both across workers depending on their wage and across firms depending on their wage structure. Using exhaustive employer-employee data, we find that half of the surplus generated by the reform falls onto workers. Wage gains load on incumbents in high-skill occupations. The wage earnings of low-skill workers—nearly all individually eligible—do not change. This heterogeneous wage incidence is unlikely to be driven by scale effects or skill complementarities. We find that the groups of workers benefiting from wage gains are also more likely to continue working for the same firm. Further, we show that firms do not change their wage-setting behavior in response to the individual eligibility status of workers. Overall, our results suggest that the wage incidence of the tax credit operated collectively through rent-sharing and benefited workers most costly to replace.

JEL Classification: D22, H25, H32

Keywords: business taxation, tax incentives, wage incidence, rent sharing

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1 Introduction

In a context of growing fiscal competition between governments, many policymakers attempt to stimulate economic activity, create jobs and boost wages using business tax incentives. Over the past decades, many advanced economies have decreased firms' tax burden by cutting tax rates,[1] or through targeted tax incentives such as tax credits that link fiscal expenditures with specific firm outcomes.[2] The 2017 Tax Cuts and Jobs Act is a recent illustration of this phenomenon that has brought attention on the distributional consequences of tax incentives, and in particular on whether it benefits workers.

Yet, while the literature has documented some wage gains for workers as a group, little is known about which workers benefit from these tax incentives (Slattery and Zidar, 2020). Auerbach (2018) stresses that the recent growth in earnings inequalities compels an investigation into the distribution of the business taxation burden across workers. Does tax policy incidence vary across groups of workers? And if so, is it influenced directly by the design of the policy – which might be targeting a subset of workers – or by other mechanisms?

In this paper, we contribute to answer this question by studying the wage impact of a large French corporate income tax credit and by investigating the underlying worker and firm-level mechanisms. This tax credit is proportional to the payroll share of employees paid less than a wage threshold. The policy thus creates variation across workers – depending on which side of the wage threshold they fall – and across firms – depending on their wage structure which determines the size of the overall windfall. The design of the policy thus provides a unique opportunity to disentangle the role of individual eligibility from firm-level mechanisms in shaping the pass-through of business taxes into wages.

First, we estimate at the firm-level that around half of the tax credit falls onto workers through higher wages. Yet, the wage incidence is not uniform within the firm: high-skill workers benefit from higher wages, low-skill workers do not. We then show that the tax credit does not affect relative employment share by skill-level. The distributive effect we uncover is thus unlikely to be driven by complementarity with

[1] Corporate income tax rates in most advanced economies have experienced downward trends. The 2017 Tax Cuts and Jobs Act reduced the US statutory corporate income tax rate from 35% to 21%, continuing the decline in the effective tax rate over the past 25 years. Similar downward trends of corporate income tax rates are under way in Europe (Auerbach, 2018).

[2] Firm-specific and targeted tax incentives are numerous and have large budgetary impacts. Serrato and Zidar (2018) document that tax base rules and credits explain more of the variation in state corporate tax revenues than tax rates do. Slattery and Zidar (2020) show that in 2014, tax incentives for firms in the form of firm-specific subsidies and general tax credits amounted to nearly 40% of state corporate tax revenues in the United States. In France, corporate income tax credits – incentivizing a host of activities, from R&D to charities etc – accounted for about 43% of gross CIT income in 2015, including the policy we study in this paper.

low-skill workers, but rather points at firm-level surplus sharing. We then show that worker-level eligibility does not drive the distributive effect of the tax credit. We find no bunching in the distribution of new hires' wages near the threshold: firms did not hire more eligible workers eligible. Overall, our results suggest that part of the policy surplus is collectively shifted onto groups of workers through rent-sharing, a process that benefits mainly groups of workers with more bargaining power. We find a significant raise in the workforce retention rate – the share of workers who remain employed in the same firm – for high-skill workers who alone benefit from wage increases. It suggests that cost of replacement might be an important driver of bargaining power.

The tax credit we study, labelled as CICE,[3] provides a quasi-natural experiment allowing to identify the distributive effects of tax incentives and to understand their underlying mechanisms. First, as mentioned above, the design of the policy is such that the amount of tax credit firms get is a fraction of the sum of gross wages of workers paid less than 2.5 times the minimum wage. It thus conveniently creates variation not only across otherwise similar workers on both sides of the eligibility cut-off, but also across firms with different wage structures around the threshold. Next, the policy and its distinct features were not anticipated. The tax credit was implemented starting in 2013 based on a law voted in December 2012 supported by a government elected in May 2012. It applied to all firms and jobs, whether pre-existing or newly created. Take-up by firms was quick and large as dedicated features were rapidly added to common accounting software packages. It resulted in a large fiscal expenditure. In 2015, it amounted to 18 billion euros, which is almost 30 percent of gross corporate income tax revenues.[4]

We combine several administrative data sets to investigate the wage and employment effects of this policy. First, we use firm-level administrative data on the amount of tax credit each firm received under the CICE policy. Second, we use matched employer-employee data on the universe of French private sector employees, providing rich information on the wages and hours worked. We finally use tax return data on the financial results of firms that obtained tax credits. We can thus conduct several empirical analyses, both at the firm- and at the worker-level, to study the distributional impact of this tax credit.

In the first step of our analysis, we study the wage incidence of the tax credit at the firm-level. We identify the causal link between firms' mean hourly wage and the tax credit by exploiting the discontinuity in the schedule: firm-specific decreases in taxes are proportional to payroll share of employees paid less than 2.5 times the minimum

[3] CICE stands for *Crédit d'Impôt pour la Compétitivité et l'Emploi* in French or Competitiveness and Employment Tax Credit.

[4] Gross corporate income tax revenues refer to the corporate income tax revenues before tax credits, with the CICE being the largest corporate income tax credit, which corresponded in 2015 to a fiscal expenditure of 271 euros per French individual.

wage.[5] To isolate the firm-specific variation that directly stems from this worker-level discontinuity, we compare, in an augmented difference-in-differences framework, the wage policy of firms that have similar pre-reform wage distributions, except immediately around the eligibility cut-off. To provide support for our identification strategy, we document that, within groups of similar firms, variation in policy exposure remains large[6] and is nearly as good as random as treatment intensity is uncorrelated with a series of pre-reform observable firms' characteristics.

We find a significant firm-level incidence of the tax credit on wages that unfolds gradually over time. More particularly, a 1 percentage point increase in the tax credit rate translates into a 0.5% increase in the mean hourly wage at the firm level. It suggests that 50% of the tax credit goes to workers. The magnitude of our estimate is in line with a recent body of work measuring the incidence of corporate income taxes using other sources of variation (Arulampalam *et al.*, 2012; Suárez Serrato and Zidar, 2016; Fuest *et al.*, 2018). This result is robust to alternative specifications.

Next, we show that while the policy caused wage gains on average, the firm-level wage incidence markedly differs across workers. The tax credit increases the wages of high-skill workers but has no impact on low-skill employees'. This distributional effect is particularly striking as it is at odds with the policy's targeting. In other words, low-skill workers, who are virtually all eligible, benefit from no wage increase while high-skill employees, who are less likely to be eligible, benefit from higher wages. The policy thus increases within-firm wage earnings inequalities. We also find that only incumbent employees benefit from wage gains, not entrants. These results suggest that the surplus generated by the policy is shared within the firm differently across groups of workers.

Further, we find no discernible effect of the tax policy on firm-level employment, nor on sales; firms do not expand, suggesting wage gains are not driven by scale effects. We further fail to detect on total employment by skill-level: the heterogeneity in policy's wage effects is not driven by skill complementarity. As such, standard labor market mechanisms do not seem to provide an adequate framework for our findings. On the contrary, we find a positive effevt on the share of high-skill workers. We find no changes in the skill mix of new hires, and show that the increase in the share of high-skill workers is explained by the higher retention rate among these workers, presumably driven by their wage gains.

The second part of our analysis considers worker-level incidence. We explore whether the individual eligibility status of workers impacts the policy pass-through.

[5] Henceforth we refer to employees paid less than 2.5 times the minimum wage as eligible employees for simplicity.

[6] In other words, due to the notch, among firms with similar wage distribution around the eligibility threshold, local differences in the share of workers right below or above that threshold can translate into substantial differences in the degree of exposure to the policy.

Canonical competitive labor market models predict that wages adjust through market mechanisms. Labor demand being more elastic than labor supply, the change in labor cost generated by the tax reform should be passed on to eligible workers' wages. The notch in the reform schedule implies a sharp discontinuity in the labor cost of workers on both sides of the wage threshold. Labor demand for eligible workers should therefore immediately adjust. To formally test for this mechanism, we build on the bunching methodology developed by Kleven and Waseem (2013) and Chetty *et al.* (2011) and test for the presence of an excess mass of new hires paid just below the eligibility cut-off. We graphically document an absence of bunching both before and after the reform. This finding does not result from downward wage rigidity. Complementary estimations corroborate this result, thus invalidating the idea that firms reacted to the tax reform by increasing their demand for eligible employees.

Overall, our findings point to a substantial firm-level incidence of the tax reform on wages that spills over onto ineligible workers together with an absence of adjustment to the policy through employee-level mechanisms. A similar pattern has been documented in a different policy environment by Saez *et al.* (2019) who study Swedish payroll tax cut targeting young workers.[7] These results can be rationalized as follow: first, the tax reform generates firm-specific surpluses that are shared across capital-owners and groups of employees through rent-sharing, which provides a rationale for treatment spilling over onto ineligible workers. Next, *within* groups of workers, incidence is collective, which explains the absence of employee-level response. Wage incidence however vastly differs *across* groups of workers. A potential explanation is that employers increase the wage of employees most costly to replace, here high-skill incumbents, in order to retain them (Kline *et al.*, 2019). In line with this hypothesis, we document that the policy increased the workforce retention rate of high-skill workers but not that of low-skill workers.

Related literature. Our paper contributes to several bodies of research. First, we contribute to the literature studying the incidence of business taxes on wages by causally documenting a sizable impact of a tax reform on wages. We find that around 50% of the surplus generated by the tax credit is shifted onto workers. The magnitude of this effect is in line with a growing literature on corporate income taxes that finds that labor bears between 30% and 50% of the tax burden. Papers in this literature have exploited several types of variation in corporate income tax rates: variation across industries (Liu and Altshuler, 2013), across US states (Suárez Serrato and Zidar, 2016), across German municipalities (Fuest *et al.*, 2018), across European or OECD countries in Europe

[7] They conclude to the absence of wage incidence at the employee level along the age eligibility threshold, but find evidence of firm-level rent-sharing as firms benefiting more intensely from the reform increase wages for both eligible (young) and ineligible (old) workers.

(Arulampalam *et al.*, 2012; Azémar and Hubbard, 2015). We leverage a nation-wide policy in France, ruling out within-country firm or labor mobility as important drivers of our results. Moreover, we compare firms belonging to the same narrowly-defined industry.

By documenting the heterogeneous incidence of this reform across occupational groups of workers, we also address the shortcoming pointed out by Auerbach (2018), who stressed that most papers have so far implicitly considered wage earners as a "monolithic group". We show that the tax credit essentially benefited skilled workers, suggesting heterogeneity in occupation is indeed a crucial element. Fuest *et al.* (2018) study changes in local business tax rates and investigate heterogeneity across groups of workers as well. They find that low-skill workers were the most penalized by tax increases.

Second, although the tax credit we study defines eligibility at the employee level, the surplus generated by the reform is mainly passed on to wages at the firm-level. This finding challenges canonical models of tax incidence that posit that tax incidence is driven by market level mechanisms impacting workers' wages individually (see Anderson and Meyer, 1997, section 2). We contribute to a burgeoning strand of the literature documenting that firm-level mechanisms, rather than individual eligibility, play a key role in shaping tax incidence on wages. For instance, Bosch and Micevska-Scharf (2017) and Bozio *et al.* (2017a) conclude to the absence of payroll tax wage incidence at the employee-level.[8] Our findings are most closely related to Saez *et al.* (2019), who study a payroll tax cut targeted at young workers in Sweden. They uncover an absence of individual-level incidence while evidencing a substantial wage incidence at the firm-level. We depart from their paper in several ways. First, we rely on a corporate income tax credit whose amount depends on employees' eligibility determined individually according to their wage. As wages are co-determined by employers and employees, we can study potential wage manipulations to maximize eligibility—a margin of adjustment that is not possible when eligibility is based on a demographic characteristic such as age whose evolution is exogenous. This difference further suggests that the fairness norms preventing differential pay between employees based on age, which they argue drives their results, probably does not apply in our setting.[9] Second, we detect no employment effect and, if anything, find that the high skill employment share increases. We can therefore rule out that the increase in high-skill workers' wages is driven by complementarities with low-skill workers. These

[8] In a recent article, Bozio *et al.* (2017b) find that the wage incidence of social security contributions in France depends on tax-benefit linkages: there is no effect on net earnings of an increase in health and family contributions – with no contributory link between contributions and benefits – while the incidence of pension contributions – with a clear contributory link – largely falls on workers. The tax credit we analyze in this paper has no contributory link.

[9] This mechanism is unlikely to extend to our setting, as to the limit employees who, absent the policy, would have been paid 2.5^{+} would probably not mind being paid 2.5^{-} MW.

results further strengthen the case for a bargaining mechanism. Finally, we study incidence and its heterogeneity across skill groups as well as across incumbents and entrants, and document stark incidence differentials across these categories. While Saez *et al.* (2019) find that the policy mostly benefits employees in the bottom of the firm wage distribution, we find that it only benefit skilled employees, who earn higher wages on average. Accordingly while their results suggest that the policy decreased firm inequalities, ours imply the opposite. Overall, our work shows that a policy whose legal targeting is progressive ends up benefiting higher-wage workers through rent-sharing. This highlights the need to finely understand the role firms have in shaping primary inequalities in order to predict the redistributive effect of business tax reform.

Finally, our paper relates to the strand of literature in labor economics highlighting the role of firms in shaping labor market inequalities (Card *et al.*, 2018). We contribute to this literature by estimating how a tax reform generating a profit windfall is partially shifted onto workers' wages. Most papers so far have used observational variation in productivity (Guiso *et al.*, 2005) or quasi-experimental research design relying on proxies likely to affect the rents earned by the firm, such as idiosyncratic demand shocks (Garin and Silvério, 2019) or patents (Kline *et al.*, 2019). Most closely related to our paper is Howell and Brown (2019) who study how the R&D grants to small firms affects wages. They find that R&D grants lead to an increase in wages that is too large to be fully explained by firm growth, thus suggesting rent sharing is taking place. Like them, we find positive wage effect associated with the tax credit but no effect on employment growth thus directly pointing at rent sharing. Although we consider a reform that affected most firms in the economy as opposed to the select subset of small innovative firms they look at, we find broadly similar patterns.[10] We find no discernible effect on employment but a strong wage incidence concentrated among high-skill incumbents whose retention rate increases significantly with no effect on the wage of new entrants. Taken together these findings are broadly consistent with monopsony-type models in which firms set wages unilaterally and where rents are shared only because of information asymmetries (Card *et al.*, 2018; Lamadon *et al.*, 2018) and where that mechanism is mostly at play with respect to incumbents workers' outside options implying that the intensity of rent-sharing is particularly strong among these workers – in particular when they are costly to replace (Kline *et al.*, 2019).

Outline of the paper. The rest of the paper is organized as follows. Section 2 presents the policy while section 3 details the data sources we use. The firm-level identification

[10] Unlike Howell and Brown (2019), we can measure hourly wage – as opposed to total compensation – which is a more direct measure of wage rate. A limitation of our study is that we cannot examine how the effect varies among incumbents depending on the length of tenure in the firm (see Section 3).

strategy is described in Section 4. The main results are presented in Section 5. Section 6 investigates several potential channel explaining our results – including employee-level incidence, – discusses our main results and situates them in the literature. Section 7 concludes.

2 Institutional setting

In this section, we detail the main features of the French tax credit studied here as well as its broader institutional setting.

Context and schedule. The policy we study, labeled CICE, was designed in 2012. At the time, the newly elected government wanted to tackle low corporate profits,[11] declining export shares in export markets (Bas *et al.*, 2015) and high unemployment rates. It commissioned a report on the state of the manufacturing sector in France (Gallois, 2012). The report, often referred to as the "Gallois Report" (*rapport Gallois*) is named after its main author, a French businessman. It advocated for cuts in payroll taxes for employees earning up to 3.5 MW. While there had been large payroll tax cuts in France since the early nineties, they targeted low wages (up to 1.6 MW, see Figure A16 in the appendix). Extending the payroll tax cuts to higher wage employees was presented as a way to target manufacturing and exporting firms, that tend to pay higher wages and therefore did not benefit from pre-existing payroll tax cuts. The report did not advocate for tax cuts directly targeted at exporting manufacturing firms because of EU rules to prevent distorting competition.

The policy proposal was criticized by labor economists on the ground that it was very costly and that it was targeted wages too high to increase employment in a cost-effective way. Their main point was that employment effects of cuts in payroll taxes are stronger when targeted at low wages, as demand is more price elastic and unemployment is high (see Cahuc and Carcillo, 2014, for an example of that view).

The government finally set the eligibility cutoff at 2.5 MW. It also designed the policy as a corporate income tax credit, rather than a payroll tax cut, as the effect on public finances would be delayed, which they saw as desirable in a context of tight budget constraints, but still create incentives for firms to hire. The policy was implemented in January 2013, with a tax credit rate of 4%. In January 2014, the rate was raised to 6%.

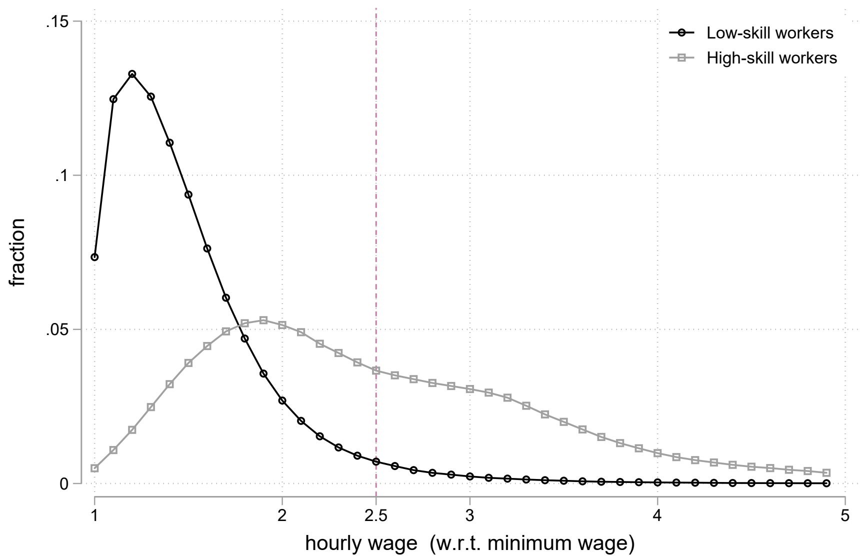
Worker and firm exposure. This cut-off implies variation in exposure to the policy at two levels: across employees and across firms. First, the sharp wage cut-off in-

[11] The profit rate (defined as ratio of gross operating surplus to value added) was at 30.25% in 2012, a figure lower than in previous years (31.7% over the years 2008-2011 and 32.63% for the years 2000-2010. Source: INSEE, Comptes nationaux).

duce substantial variation in workers' individual eligibility, especially among high-skill workers. Figure 1 depicts the distribution of hourly wages relative to the minimum wage by occupation.[12] The red vertical line represents the eligibility cut-off. While virtually all clerks and blue collars earn wages below the 2.5 MW cut-off, a substantial share of managers and professionals earn above this threshold.

Second, this cut-off implies variation at the firm level depending on firms' share of eligible employees. Figure A1 plots the distribution of firms' exposure to the policy by firm size. Exposure to the policy is defined as the amount of tax credit each firm claimed divided by its wage bill as of 2013. The spike at 4% is constituted of firms the most exposed to the policy: all their employees are paid less than 2.5 MW and are eligible to the policy. The amount of tax credit they receive is therefore equal to 4 percent of their wage bill. The policy exposure distribution also exhibits a long left tail with substantial variation in the amount of tax credit that firms could claim, implying firms were heterogeneously affected by the policy.

Figure 1: Distribution of wages by occupation



Notes: The figure plots the 2012 distribution of hourly gross wages of workers by occupation. The sample is restricted to workers employed in firms present in our estimating sample (see section 3.4 for details on the estimation sample). Each dot represents the fraction of workers in an interval of length 0.1 minimum wage.

[12] As in Caliendo *et al.* (2015), we split workers into groups according to their occupational category. The first group includes senior staff, professionals, associate professionals, technicians, and employees at the supervisor level, corresponding to classes 3 and 4 of the French occupational classification system. The second category encompasses clerical employees and blue-collar workers, i.e. classes 5 and 6. We refer to these groups as, respectively, *managers and professionals* and *clerks and blue-collars* for brevity. Given that occupational categories also correspond to different skill levels, we interchangeably refer to these two groups as *high-skills* and *low-skills* respectively.

Timing of benefits. The CICE is a tax credit, which entails a delay between the time wages are paid (and opens rights to the CICE) and the time when a firm benefits from the tax credit. Profitable firms get the tax credit at the end of the policy's first year, when paying corporate income taxes. Firms that are not profitable and thus pay no corporate income tax benefit from the tax credit after three years as a payment from the government, or after one year if they qualify as SMEs or go through restructuring. It implies that all firms, profitable or not, benefit from this tax credit, but at different time according to their profitability and official size category. For simplicity and because profitability and size could be endogenous to the policy, we only consider how much tax credit a firm is entitled to, rather than how much it actually benefited from, to measure their exposure to the policy.

Anticipations. Firms did not anticipate this policy as it was implemented short after a new government was elected, it was not part of the winning candidate's platform. Indeed, in 2012, a new French government came to power, in the context of an economic slowdown. This new government commissioned a working group to issue a report on how to tackle high unemployment and bolster French firms' performances.[13] It advocated for payroll tax cuts up to 3.5 MW in the manufacturing sector to boost firm competitiveness. This cut-off was chosen to target exporting firms, as they pay higher wages on average. Deemed too high by labor economists on the ground that the elasticity of labor demand is low for high-wage employees, the cut-off was eventually brought down to 2.5 MW.[14] Moreover, little time elapsed between the release of the report – November 5^{th} 2012 – and the implementation of the CICE beginning January 1^{st} 2013, making any anticipation effects quite unlikely.

Perceived duration. When the policy was introduced, it was meant to be a permanent shift and no end date was set. It was introduced in 2013 and lasted as a corporate tax credit until 2019. In 2019, the policy was redesigned as a payroll tax cut but retained the same schedule and in particular, the same 2.5 MW eligibility cutoff. So this conversion did not fundamentally alter the parameters of the policy.

Yet, despite this ex-post durability, uncertainties surrounded the policy from its inception. Interviews realized with CEOs and interventions by employers organizations suggest businesses might have perceived uncertainty early about the durability of the

[13] This report is often referred to as the *Gallois Report* in journalistic debates, after the name of Louis Gallois a businessman who chaired the working group.

[14] See Fabre (2012) for a journalistic account of the debate just before the adoption.

policy over time.[15] Business leaders tend to perceive tax credit as easier to repeal than payroll tax cuts (France Stratégie, 2015), and the transformation of the policy into a payroll tax cut in 2019 was partly thought as a way to signal that it would be long lasting. Therefore, while as of today the policy has proved to be long lasting, there is ample anecdotal evidence that, at the time, employers where uncertain as to whether the policy would be discontinued.

Take up. The take-up was high. First, since most firms have at least one employee paid below 2.5 MW, they are almost all eligible to the tax credit. Second, implementation was made easy as accounting software packages rapidly included new features dedicated to the policy. Third, workers' wages, the only criterion for eligibility, is directly observed by firms. As a result, a very large share of French firms benefited from the tax credit. For this reason, we decide to study firms differently exposed to the policy given their *ex ante* wage structure rather than compare firms that claim the tax credit to those that do not.

Other policies. The CICE follows other policies aimed at creating fiscal incentives for firms to increase hires, mainly payroll tax cuts. These policies however targeted workers paid significantly lower wages. Pre-existing payroll tax cuts targeted low wages, and amounted to about 26 percent of the gross wage at the minimum wage-level, gradually decreasing to reach zero at 1.6 MW. In contrast, the CICE's tax credit rate is flat up to 2.5 MW, and sharply falls down to 0 above this threshold. The 2.5 MW eligibility cut-off is specific to this policy, and significantly different from past thresholds. Accordingly, exploiting this discontinuity to identify the reform's impact should not pick up the effect of another policy. In 2016, new payroll tax cuts targeting workers with wages up to 3.5 MW were implemented. We therefore end our analysis in 2015 as these new tax incentives may affect our identification strategy.

Budgetary impact. The CICE had sizable budgetary consequences. The direct cost of the policy was 18.1 billion euros in 2015, which is equivalent to .82 percent of GDP, 30

[15] For instance, in commission hearings on the CICE at the National Assembly, the head of a think tank close to employer organizations named Coe-Rexecode made the following statement "*I would like to come back on the comparison of the CICE with a payroll tax cut. I think it is clear that designing the policy as a tax credit weakened its effects, as tax credits are easier to repeal. By contrast, firms need a long term vision when planning investments. The weak point of this policy is the uncertainty about its continuation.*" (p. 134). The report can be accessed at `https://www.assemblee-nationale.fr/14/pdf/rap-info/i2239.pdf` (in French). France Stratégie, an agency affiliated with the Prime Minister's administration wrote in a reports that "*various interviews reveal a recurring uncertainty among business leaders about the sustainability of the scheme over time. Although announced as lasting at least four years when it was introduced, whether the CICE would be continued gave rise throughout 2014, and again in 2015, to public debates and interventions. [...] In general, companies seem to perceive tax credits as more uncertain than payroll tax cuts.*" See France Stratégie (2015).

percent of corporate income tax gross revenue, and 272 euros per capita.[16] This quantitatively large policy was financed by several measures affecting households rather than firms, such as increases in VAT rates.[17] Although labor market outcomes may be affected by these contemporary measures through general equilibrium, the discontinuity in the tax schedule we leverage for identification is unrelated to these simultaneous reforms. Moreover, we absorb aggregate shocks by using year or sector - year fixed-effects and focusing on firms and individuals directly affected by the discontinuity.

3 Data

We use information from three administrative sources. Matched employer-employee data come from social security declarations. Data on firms' financial performances come from corporate income tax returns and data on the amount of tax credit each firm claimed come from the public finance administration. In this section, we describe the data sources and detail the matching procedure as well as the characteristics of the resulting estimating sample.

3.1 Employer-employee data

The main source on employment and wage is the *DADS* (*Déclarations Annuelles des Données Sociales*) data set. It is a matched employer-employee data set based on social security records which covers the universe of French firms and private sector jobs. Data are provided at the contract-level and contain information on gross and net wage earnings, hours worked, occupation, type of contract (short term or long term), as well as some employee characteristics (age, gender). Each firm is identified with a unique administrative number called *SIREN* that remains the same across years. It allows matching each employee to an employer and tracking firms' employment and wage policy over time. Each individual in the data set is assigned a unique identifier each year, which allows tracking individuals across employers within a year. While this identifier changes over time and therefore does not allow to track individuals across years, the characteristics of the employee's past year's contract are provided (past year's employer, wage, hours worked, etc.), which allows to know whether the

[16]The corporate income tax gross revenue is the corporate income tax before tax credits, whose major share is actually constituted by the CICE.

[17]The CICE necessitated public finance reforms to be funded. There is no unique funding scheme dedicated to CICE, the government actually funded it through the general budget, by decreasing a broad range of expenditure and by increasing some mandatory levies on households, mainly VAT.

individual changed firm or job and to compute year-on-year changes in wage earnings or hours worked. We use data for the years 2009 to 2015.[18]

Based on gross wage earnings and hours worked, we compute the gross hourly wage of each employee. This allows us to determine precisely whether each employee is eligible to the tax credit based on whether her gross hourly wage is lower than 2.5 times the minimum wage.[19] Aggregating across individual within a firm, we can compute the payroll share of employees paid less than the eligibility threshold. We thus precisely know both employees' and firms' exposure to the tax policy.

We also use this data set to build our main outcomes of interest. At the employee-level, we compute employees' hourly gross wage earnings. We also compute wage growth for incumbent employees, which we defined as workers working in the same firm two years in a row under a permanent contract.[20] At the firm-level, we compute the mean hourly gross wage earnings for all employees as well as for two broad sets of occupations.[21]

3.2 Tax credit data

The amount of CICE tax credit claimed by each firm is documented in an ad-hoc file provided by the Public Finance Administration (*DGFiP*). This file is called the CICE MVC (*Mouvements de Créances*) dataset. The first vintage is 2013 as it corresponds to the first year the policy was implemented. The distribution of tax credit amounts is very skewed. Micro firms (less than 10 employees), receive on average 2,756 euros each year, SMEs (10 to 250 employees) get 24,492 euros. The 288 largest firms in the data set get a tax credit amount approximately equal to that of the 496,750 micro firms.

3.3 Balance sheet data

We use income statement and balance-sheet data coming from the *FARE* database. This dataset is built using the tax returns of firms and their social security declarations. This dataset covers the universe of firms, except those in the financial and agricultural sectors. It provides detailed information on firms' revenues and expenses. We use this

[18]The methodology and perimeter of the dataset underwent a major change in 2009.

[19]Gross wage earnings are defined as "all remunerations received by the employee under her contract of employment, before deducting compulsory contributions". It does not allow breaking down compensation by type (fixed salary, bonuses, etc.). The minimum wage in France is set in terms of gross hourly wage.

[20]In France, by law, employees with fixed-term contracts must receive a bonus at the end of their employment period equal to 10% of the amount received during the contract period. It is likely to impact the measured year-on-year hourly wage growth of employees with fixed-term contracts. We therefore decide to set aside fixed-term contract workers in the employee-level analysis of wage growth.

[21]Similarly, firm-level mean hourly wage excludes hours worked and compensations of workers with a fixed-term contract.

database to build firm-level control variables: labor productivity (value added divided by average workforce) and assets (tangible and intangible).[22]

3.4 Matching and final datasets

From the three data sets mentioned above, we build two estimation samples: one at the firm level to study collective incidence (section 4), and one at the contract-level to study individual incidence (section 6.1). The two estimation samples include the same firms, only the unit of observation differs. Firms are matched using their administrative identifier (*SIREN*), and need to meet the following criteria:

1. Firms need to be in the three data sets. It implies that all firms in the estimation sample claimed the tax credit. Therefore, in our estimation sample, the take up rate is 100%, by construction. We compare firms differently exposed the tax reform, conditionally on claiming the tax credit.

2. Firms need to exist over the whole 2009-2015 time period. We obtain a balanced sample of firms.

3. Firms need to be continuing employers. We keep only firms with employment equal to at least one full time equivalent each year. The firm-level mean hourly wage cannot be zero.

4. Firms do not exhibit extreme values for the following measures: ratio of tax credit to wage bill, profit margin, assets per worker and mean hourly wage growth. We exclude firms that are in the top and bottom percentiles in at least one year, as it is likely to reflect measurement errors.[23]

The resulting dataset contains 325,329 firms that account for 64.6% of the total CICE credit in 2013.[24]

We study two outcomes of interest at the employee-level: the gross hourly wage of new hires and the year-on-year variation in gross hourly wage of continuing workers (both are expressed in logs). The outcome of interest at the firm-level is the average

[22]This data set also includes data on measures of profits. However we do not use variables such as reported profits due to accounting issues. Indeed, firms were allowed to account for the tax credit either as a decrease of wage bill or as a decrease of corporate taxes. Firms should find the second option more attractive, as the first option inflates the corporate income tax base. Therefore, an increase in reported profits could either reflect an increased profitability, or an accounting procedure choice.

[23]We test the robustness of our results to including firms with extreme values for wage. Tables A8 and A14 report estimates from Equation (2). Coefficients are very similar to baseline estimates in Tables 2 and 6. It implies that sample restrictions minimally affect the results. For this reason, and because extreme values are likely to reflect input errors and cannot be meaningfully interpreted in economic terms, we see these sample restrictions as warranted and use them to define our estimation sample.

[24]They account for almost identical shares for the following years: 64.3% in 2014 and 63.8% in 2015.

hourly wage of workers with a permanent contract, working substantial hours (at least 60% of a full time) and who were already working for the firm the preceding year.[25]

4 Empirical strategy

We want to assess whether workers benefit from tax incentive policies through higher wages, and if so whether all workers are impacted. We address this question by using firm-level variation in exposure to the tax policy generated by firms' pre-reform wage structure. In this section, we present in details our identification strategy. We first specify how we measure the intensity of exposure to the policy (4.1) before detailing how we adjust our estimation approach in order to isolate variation in exposure to the policy related to local differences in pre-reform wage distribution around the policy threshold at 2.5 minimium wage (section 4.2).

4.1 Treatment intensity

Our empirical strategy exploits the between-firm variation in exposure to the policy. Yet, a firm's exposure can be driven by its behavioral response to the policy itself. For example, if the tax credit causes a firm to hire more low-wage (eligible) workers, then its exposure to the policy will endogenously increase. For this reason, we firms' pre-reform wage structure to measure their exposure to the policy (as in Auten and Carroll, 1999; Saez *et al.*, 2019).[26] We define our treatment intensity variable as the pre-reform payroll share of workers paid less than 2.5 MW, scaled by the tax credit rate. It corresponds to the predicted effective tax credit rate. More formally, it writes:

$$Z_i = \frac{\tau \cdot \sum_{j \in i} w_{j,t_0} h_{j,t_0} \cdot \mathbb{1}(w_{j,t_0} < 2.5MW_{t_0})}{\sum_{j \in i} w_{j,t_0} h_{j,t_0}} \tag{1}$$

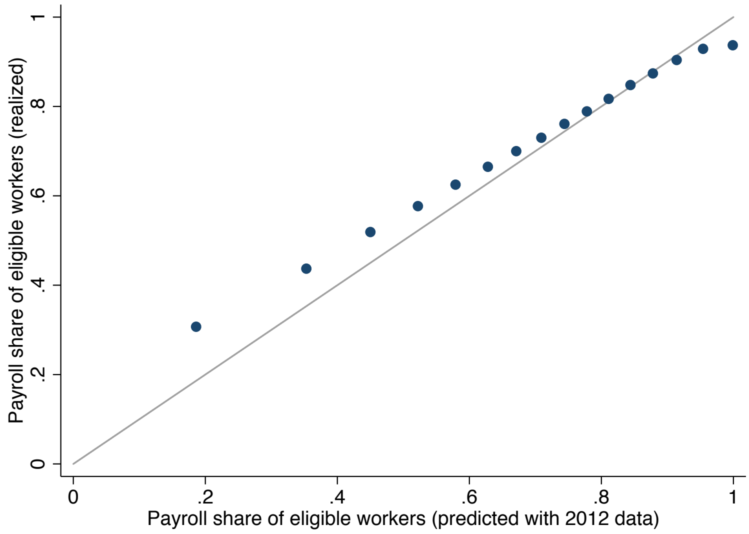
where w_{j,t_0} and h_{j,t_0} denote the gross hourly wage and hours worked of employee j in firm i during the last pre-reform year denoted t_0. τ is time-invariant and is set to reflect the average rate of subsidy over the period (the mean of 4% in 2013, 6% in 2014 and 6% in 2015 is 5.33%).

Our choice of treatment intensity relies on the persistence of firms' wage structure across time. Figure 2 depicts the relationship between the payroll share of eligible workers following the policy implementation (2013-2015) and the payroll share of eligible workers in 2012, which we use to define our treatment intensity. The payroll share of eligible workers in 2013-2015 is measured as the payroll share of workers

[25] The average wage is simply the ratio of the overall wage bill to total hours worked. More detailed definitions of all variables used in the analysis are included in Section OA1 of the Online Appendix.

[26] Auten and Carroll (1999) use this method to estimate of the elasticity of taxable income. They apply the variation in the rate to the earned income the year preceding the reform.

Figure 2: Payroll share of eligible workers: pre- vs. post-reform



NOTES: This binned scatterplot depicts the relationship between firms' payroll share of eligible workers as measured by the ratio of CICE amount over payroll divided by the applicable tax credit rate on the y-axis and the instrument on the x-axis. The instrument corresponds to payroll share of eligible workers predicted using 2012 wage structure. The right-hand side variable is grouped into 30 bins.

triggering a tax credit as backed out using the amount of CICE firms obtained and divided by the applicable rate (4% in 2013 and 6% in 2014 and 2015). It is then expressed as a share of the firm's payroll. The relationship is strongly positive and dots all lie close to the 45 degree line. A one percentage point increase in the 2012 payroll share of eligible workers is associated with a .76 percentage point increase in the 2013-2015 payroll share of workers opening rights to the tax credit. It implies there is substantial persistence in firms' wage structures across years.[27]

4.2 Using the discontinuity to compare similar firms

As mentioned above, variation in treatment intensity across firms is driven by differences in wage structures. A potential challenge is that these differences are associated with unobservable firms' characteristics that impact firms' dynamics. To take an extreme example, firms employing only minimum wage workers are likely to differ from firms employing only high wage workers in fundamental ways, observable and non-observable, which are likely to result in biased estimates. The notch created by the policy at the 2.5 minimum wage-level is useful for identification. Ideally, we would like to compare the wage dynamics of firms whose employees are all paid just a little

[27]We focus on reduced-form estimates as they can be identified while remaining agnostic about the lag structure of the effect of the endogenous variable. On the contrary, the structural coefficients cannot be identified without making further restrictive assumptions. See section OA5 in the Online appendix for a formal exposition of this argument.

less than 2.5 MW in 2012 to firms whose employees are all paid just a little more than 2.5 MW as in a regression discontinuity type approach. This strategy is not feasible as no firms strictly match the required criteria. Instead, we approximate this approach by comparing firms with similar wage distribution, except just around the eligibility threshold, thus ensuring that variation in treatment is driven by local differences in wage distribution.

To this end, we group firms based on their wage structure as of 2012. For each firm, we compute the 2012 payroll share of workers paid less than 2.2 MW and the payroll share of workers paid less than 2.8 MW.[28] We then discretize these shares, and interact them. Our preferred step to discretize payroll shares is 3.33 percentage points (30 categories),[29] which yields 30 × 30 groups of firms that have a similar payroll share of workers paid less than 2.2 MW and a similar payroll share of workers paid less than 2.8 MW. We call these groups "bins". Within a bin, firms have a similar wage structure, except immediately around the eligibility threshold, they are thus similar but differently exposed to the tax policy.

Figure 3 provides a graphical illustration of our methodology. Firms in bin A have between 95% and 100% of their payroll going to workers paid less than 2.2 MW, they are intensely exposed to the CICE. To the opposite, firms in bin B have between 0% and 5% of payroll going to workers paid less than 2.8 MW. They are (almost) not exposed to the CICE. Firms in these two groups are very differently exposed to the policy, but also likely too different to be comparable. Instead, our methodology compares firms with a similar wage distribution, except immediately around the eligibility threshold. Figure A2 depicts the number of firms in each bin.

To ensure that within a bin, firms do vary in their policy exposure, we define two sub-samples of firms according to their payroll share of workers paid between 2.2 and 2.8 MW. The first sub-sample includes only firms for which this payroll share is at least 30%, the second sub-sample includes firms whose payroll share is at least 50%.[30] We therefore exploit large across firms variations in policy exposure that derive from small variations in wage structure.

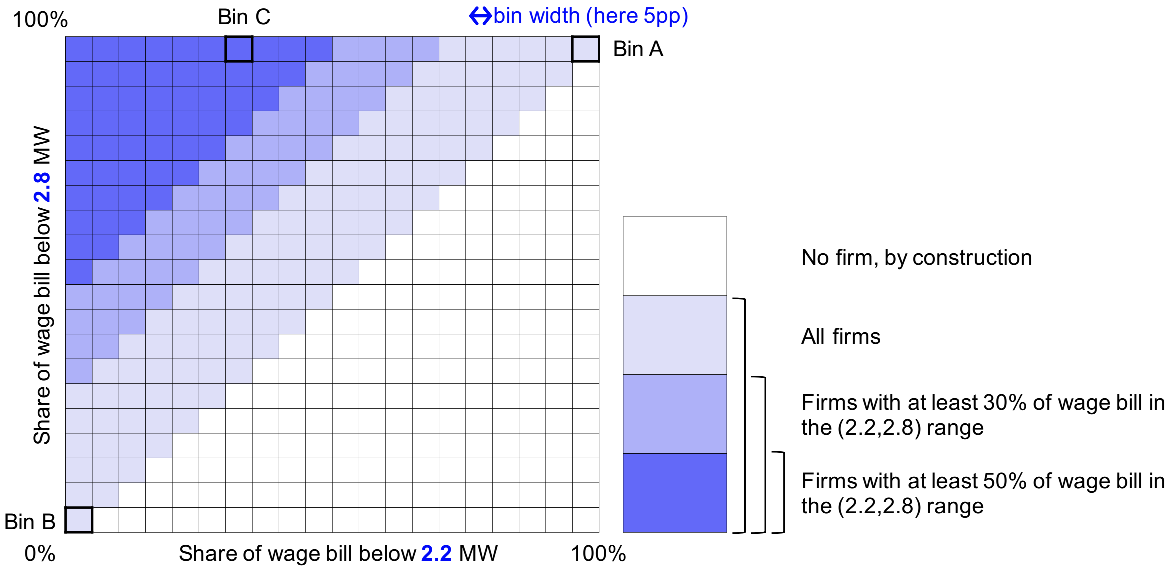
Figure A3, left panel, plots the cumulative density function of wages of all firms with at least 30% of their payroll share going to workers earning between 2.2 and 2.8 MW (in black). The blue (respectively grey) line corresponds to the same density function for firms with an above (below) median payroll share of eligible employees. Firms most exposed to the policy (blue) have a higher payroll share of low-wage workers than least exposed firms (grey). Figure A3, right panel, plots similar cumulative density function, except that above and below median treatment intensity groups are

[28] As a sensitivity test, we also set the bounds to 2.3 MW and 2.7 MW.

[29] We additionally show that our results are robust to other parameters.

[30] These subsets are not representative of the full sample. They tend to include smaller, more productive and more skill intensive firms. Descriptive statistics are reported in Table A1.

Figure 3: Illustration of the bin method



Notes: Firms in bin A have between 95% and 100% of their wage bill accruing to workers paid less than 2.2 MW. Firms in bin B have between 0% and 5% of their wage bill paid to workers earning less than 2.2 MW. Firms in bin C have between 30% and 35% of their wage bill paid to workers earning less than 2.2 MW and between 95% and 100% of their wage bill paid to workers earning less than 2.8 MW. More than 50% of their wage bill accrues to workers paid between 2.2 and 2.8 MW. The bottom right hand corner contains no firm, by construction.

defined within each bin. It shows that firms in the two groups differ in their wage distribution only immediately around the eligibility cut-off. Their wage distributions are otherwise similar.

Table A2 shows the pre-reform correlations between the instrument and firms' observables (firm's assets, labor productivity, share of low-wage employees) in Column (4). Column (5) plots the correlations between the residuals of the instrument and firms' observables after absorbing industry and size category fixed effects. Column (6) plots the equivalent correlations after absorbing sector $\times$ size $\times$ bins fixed effects. Controlling for industry and size fixed-effects only slightly reduces the correlation between firms' observables and the instrument. To the opposite, in the last column, the correlation between firms' observables and the instrument is significantly reduced. It suggests that within bin the variation in *ex ante* policy exposure is nearly as good as random.

Is within bin variation in exposure to the policy sufficient for identification? Table 1 shows how much variation in policy exposure remains after absorbing sector $\times$ size $\times$ bins fixed effects. In the full sample, only 8% of the variation in policy exposure is between firms of the same bin. Yet, for subsamples of firms that have a larger share of workers paid wages close to the wage cut-off, within bin variation in policy exposure is

Table 1: Between/within bin variation in treatment

Statistic	Sample	# firms	Std deviation	Between Cells	Within Cells
$\hat{\mathbb{V}}(Z_i)$	all	311,284	0.0094	92.0%	8.0%
$\hat{\mathbb{V}}(Z_i)$	% WB > 0.3	29,114	0.0107	50.6%	49.4%
$\hat{\mathbb{V}}(Z_i)$	% WB > 0.5	7,840	0.0120	28.2%	71.8%

Notes: A bin is defined as a unique value of the proportion of wage bill accruing to workers making less than 2.2 and less than 2.8 MW (both variables are discretized through truncation into 31 values). A cell is defined a specific bin $\times$ sector $\times$ size category combination. "% WB $> x$" refers to a sample restriction to firms whose share of the wage bill constituted of wages between 2.2 and 2.8MW is above x.

substantial. Within bin variance accounts for 50% of total variance (72%) among firms with 30% (respectively 50%) of their payroll share going to workers paid between 2.2 and 2.8 MW. Additionally, the total variance in these subsamples is larger than that in the full sample of firms.

4.3 Specifications

Difference in differences. Our main difference in differences specification writes as follows:

$$\ln(Y_{i,t}) = \alpha_i + \alpha_{c,t} + \beta \cdot Z_i \cdot \mathbb{1}\{t \geq 2013\} + \sum_{d=2009}^{2015} X_i' \mathbb{1}\{t = d\} \cdot \gamma_d + \varepsilon_{i,t} \tag{2}$$

where Y_{it} is the average hourly gross wage earnings of employees in firm i at time t. The term α_i refers to a firm fixed-effect, $\alpha_{c,t}$ corresponds to cell $\times$ year fixed effects. We defined cells as the interaction of bin categories with industry and size categories. The inclusion of cell $\times$ year fixed effects implies that we are comparing *ex ante* similar firms in terms of wage distribution at 2.2 and 2.8 MW, industry and size category. The common trend assumption needs only to hold within-cell. Z_i is the instrument, it corresponds to the predicted policy exposure of the firm given its pre-reform wage structure, as defined in equation (1). $\mathbb{1}\{t \geq 2013\}$ is an indicator variable equal to one if $t \geq 2013$. The coefficient β can be interpreted as a semi-elasticity of the variable $Y_{i,t}$ relative to the tax credit rate. We show in Appendix OA4 that this semi-elasticity can be interpreted as a close approximation of the share of the tax credit incidence borne by labor. The vector X_i includes the share of employees paid less than 1.5 MW,[31] as of 2012 interacted with year dummies. Since our source of variation in treatment intensity is quasi-random, these control variables should not affect point estimates,

[31] Another policy targeting these employees was implemented in 2015.

but might help improve the precision of our estimates. Standard errors are clustered at the firm level to account for serial correlation.

Event study. To test for diverging pre-trends and assess how the effect unfolds overtime, we also implement an event study specification. It writes as follows:

$$\ln(Y_{i,t}) = \alpha_i + \alpha_{c,t} + \sum_{\substack{d=2009 \\ d\neq 2012}}^{2015} \beta_d \cdot Z_i \cdot \mathbb{1}\{d = t\} + \sum_{d=2009}^{2015} X_i' \mathbb{1}\{t = d\} \cdot \gamma_d + \varepsilon_{i,t} \tag{3}$$

This specification is similar to equation (2), except the instrument is interacted with a full set of year dummy variables. 2012 is the reference year. Standard errors are clustered at the firm-level.

5 Results

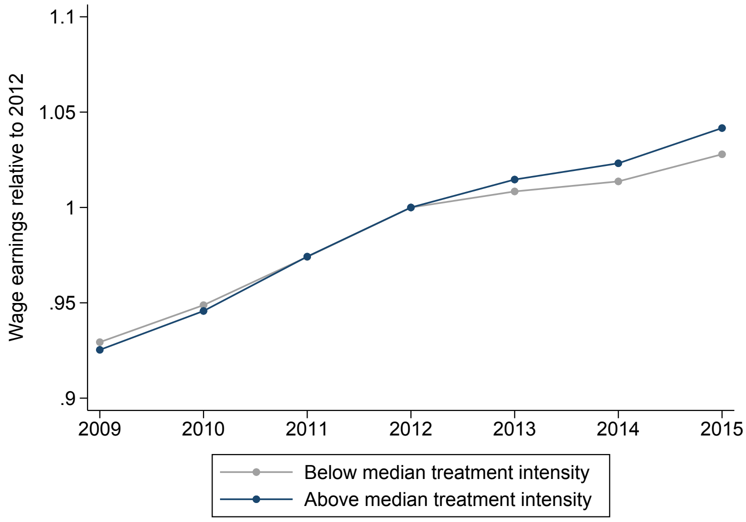
In this section, we present our main results showing workers on average substantially benefit from the tax policy through higher wages in Section 5.1. In Section 5.2, we show our results are robust to alternative specifications and tests.

5.1 Main Results

Graphical evidence. Figure 4 plots the evolution of the weighted gross hourly wage (base 1 in 2012) in two groups of firms, according to whether their policy exposure is above or below the within bin median. The two groups of firms have parallel trends prior to the reform and the group the most exposed to the policy experiences faster growth in mean hourly wages. In 2015 relative to 2012, wages in most-treated firms have grown by 4.20% and by 3.02% in least treated firms.

Event study regressions. Figure 5 plots the estimation results of Equation 3 where dots correspond to point estimates and vertical bars to 95-percent confidence intervals. The specification chose corresponds to that of column (9) in Table 2. The outcome variable is the log of the weighted average of gross hourly wages of full-time workers at the firm-level. First, coefficients before 2012 are all close to zero and not statistically significant, confirming the absence of pre-trend. To the contrary, from 2013, we ob-

Figure 4: Firm-level impact on gross hourly wages



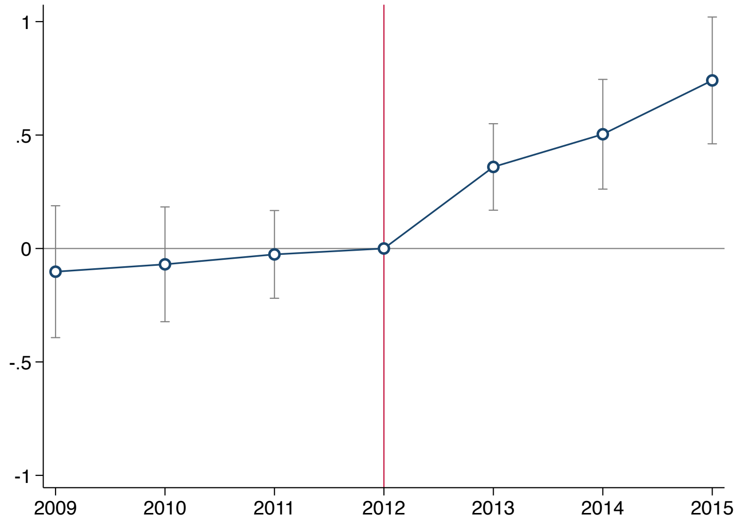
Notes: This figure plots the mean gross hourly wage of firms (relative to 2012) across a balanced sample of firms with at least 50% of their payroll paid to workers earning between 2.2 and 2.8 MW. We consider two groups of firms according to whether their 2012 payroll share of workers paid less than 2.5 MW is above or below within-bin median. Bins are defined as detailed in Section 4. On average, the predicted effective rate of CICE of firms with an above median payroll share of eligible workers is 4.84%. It is equal to 2.41% for firms in the other group.

serve a large increase in mean hourly wages that unfolds gradually. We comment on the magnitude of the estimates based on the static difference-in-differences below.[32]

Difference in differences estimates. Table 2 presents the formal difference in differences estimation results corresponding to Equation 2. The outcome variable is the log of the average hourly wage in a given firm. The first two columns of the table correspond to the full sample, the next two columns to the subset of firms with at least 30% of their wage bill accruing to workers paid between 2.2 and 2.8 MW and the last two columns to the subset of firms with at least 50% of their wage bill in this window. In Columns (1), (3) and (5), regressions include year 2009 to 2015 with no controls. Columns (2), (4) and (6) include controls measured in 2012 interacted with time fixed-effects. Our preferred specification is that in Column (6) as the sample definition is the strictest and controls are included. We estimate a positive, sizable and statistically significant impact of the CICE on wages at the firm-level. For the 30% sample (column 5), an increase in the effective tax credit rate by 1 percentage point translates into a 0.46% increase in firms' average wage. The analogous figure for the 50% sample in columns

[32]We notice that the effect keeps growing over time. Some firms might have increased wages only when paying their corporate income tax while others anticipated it. It could also be that firms are gradually learning about the policy. Note however that this increase in the estimated coefficients is much less pronounced in the 30% sample (see Figure A4 and associated comments in the paragraph "Rationale for within cell estimation" in section 5.2).

Figure 5: Impact on mean hourly wage earnings (log), event study estimates



NOTES: This figure plots the point estimates and 95-percent confidence intervals from the event study regression defined in Equation 3. The unit of observation is the firm. The dependent variable is the mean gross hourly wage of workers, weighted by hours worked, in logs. The independent variable is the 2012 payroll share of workers earning less than 2.5 MW multiplied by the tax credit rate and interacted with year dummies. 2012 is the reference year. The sample includes firms with at least 50% of their payroll paid to workers earning between 2.2 and 2.8 MW. Bins are defined as in Table 2. Robust standard errors are clustered at the firm level.

(6) is 0.58, suggesting that a little over half of the tax credit was passed on to workers.[33] Point estimates are stable across specifications and samples (although they tend to be somewhat larger among the 30 and 50% sample). They are all strongly significant.

Weighting by firm size. Our main specification is at the firm-level, and a potential concern is that the point estimates are driven by small firms, which ultimately account for a small number of employees. To gauge whether this is the case, we estimate our main specification weighted by firm pre-reform employment. In addition, we also explore weighting our specifications with the square root of employment, as very large firms could be outliers and drive the effects.

Columns (1)-(3) of Table 3 present estimates of our baseline according to different weighting schemes. It is not weighted in the first row, weighted by pre-reform employment in the second row, and weighted by the square root of employment in third one. We find that point estimates are nearly unchanged, both in terms of magnitude and significance. It implies that our results are not driven by wage setting behaviors that are specific to small firms. Coefficients are particularly close for 30% and 50% samples, in columns (2) and (3) respectively. They tend to be slightly smaller for the 0%

[33]See Appendix OA4 for a simple derivation on the link between the estimated semi-elasticities and the labor share of the tax credit incidence.

Table 2: Impact on mean hourly wage earnings (log), difference in differences

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.359***	0.359***	0.463***	0.466***	0.584***	0.587***
	(0.0639)	(0.0639)	(0.0790)	(0.0790)	(0.117)	(0.117)
Observations	830144	830144	159551	159551	42854	42854
R^2	0.935	0.935	0.863	0.863	0.772	0.773
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	118592	118592	22793	22793	6122	6122

NOTES: This table reports the estimated impact of the CICE tax credit on wages. The unit of observation is the firm. The dependent variable is the mean gross hourly wage. The independent variable is the 2012 payroll share of workers earning less than 2.5 MW multiplied by the tax credit rate and interacted with an indicator variable equal to one after 2013. Estimated coefficients correspond to Equation 2. Regressions include cell $\times$ year FE, as defined in Section 4.2. We use the payroll share of workers paid less than 2.2 and 2.8 MW to build the bins. We use a 3.33 percentage points discretization step. Controls include the 2012 payroll share of workers earning less than 1.5 MW interacted with a full set of year dummies. Robust standard errors clusters at the firm level in parentheses. *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

sample, although the baseline coefficient is well within the confidence interval. Very large firms that have a small fraction of workers earning a wage close to 2.5 MW might have slightly different behaviors. Overall, our estimates are very stable when weighing observations by firm size and our results are unlikely to be exclusively driven by small firms.

Level regressions. To improve comparability with previous estimates in the literature, we also use a linear specification with measures of wage and policy surplus per worker in euros. The idea is to see whether such a linear specification will also imply that an extra euro for the firm translates in roughly 0.5 euros of wage for workers.

The dependent variable is average wage per worker, and the treatment intensity is the firm policy surplus divided by the number of workers, both expressed in euros.[34] This measure is directly comparable to other works using surplus per worker as a treatment intensity. Accordingly, the specification we estimate writes as follows:

$$Y_{i,t} = \alpha_i + \alpha_{c,t} + \beta \cdot SPW_i \cdot \mathbb{1}\{t \geq 2013\} + \sum_d X_i' \mathbb{1}\{t = d\} \cdot \gamma_d + \varepsilon_{i,t} \tag{4}$$

[34] In both cases, we compute the total number of workers by weighting each workers based on her hours worked and expressing overall employment in "full time equivalent". For instance, a half-time worker counts as 0.5 and a full time worker will count as 1.

where $Y_{i,t}$ is the mean hourly wage in euros, and SPW_i captures the surplus per full time equivalent in euros induced by the policy predicted with the 2012 wage distribution.[35]

We also adapt the definition of cells accordingly. In the standard version, cells are based on discretizing the wage bill share of workers earning less than 2.2 and 2.8 times the minimum wage and interacting these measures. It is consistent with a treatment intensity defined using the share of eligible wage bill. In the linear specification, the treatment intensity is the surplus per worker, so we redefine the cells to best match this measure. We build alternative measures of surplus per worker as if the eligibility thresholds were 2.2 and 2.8, and discretize them in the same number of categories as before.[36]

Table 3 reports estimates of the baseline specification in Columns (1)-(3) and estimates of the level specification in Columns (4)-(6). In Columns (1) and (4), the sample includes all firms that have a non-zero number of employees paid between 2.2 and 2.8 MW. In Columns (2) and (5), firms have at least 30% of their wage bill paid to workers earning between 2.2 and 2.8 MW, 50% in Columns (3) and (6).

Panel A presents unweighted estimates, as in the baseline specification. The coefficients of the baseline specification and that of the level specification are very close, all are statistically significant and implies an extra euro of tax credit for the firm translates roughly into 0.5 extra euros for employees through higher wages. Weighing observations based on 2012-employment lead to somewhat more nuanced conclusions. In Panel B and C, we see that coefficients are very stable for the 30% and 50% samples. Only the 0% sample, estimates of weighted specifications are close to zero and not significant. It could be that the level specification estimates are more sensitive to outliers or specific observations. The estimates are also less precise for the 0% sample as, for instance, the standard error of the linear specification weighted by employment is more than two times that of its unweighted counterpart.

[35]The surplus is defined as follows:

$$S_i = \tau \cdot \sum_{j \in i} w_{j,t_0} h_{j,t_0} \cdot \mathbb{1}(w_{j,t_0} < 2.5MW_{t_0})$$

where w_{j,t_0} and h_{j,t_0} respectively denote the gross hourly wage and hours worked, in full time equivalent, of employee j in firm i during the last pre-reform year denoted t_0. τ is time-invariant and is set to reflect the average subsidy rate over the period. The surplus per worker for firm i is:

$$SPW_i = \frac{S_i}{FTE_i}$$

where FTE_i is the number of full time equivalents in firm i.

[36]Interacting these categories with sector, size and year indicator variables yields the new version of the cells. In the baseline specification, we use 31 categories, that is 30 categories, plus an accumulation point for firms whose workers are all earning less than 2.2 MW.

Table 3: Magnitude of effects: surplus-per-worker specification and weighting by firm size

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline specification			Surplus-per-worker specification		
Panel A.	Baseline–unweighted			Linear–unweighted		
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.359***	0.466***	0.587***	0.213***	0.431***	0.638***
	(0.0639)	(0.0790)	(0.117)	(0.0794)	(0.0937)	(0.154)
Panel B.	Baseline–weighted by employment			Linear–weighted by employment		
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.258***	0.411***	0.595***	-0.162	0.356***	0.694***
	(0.0918)	(0.0861)	(0.126)	(0.176)	(0.119)	(0.184)
Panel C.	Baseline–weighted by $\sqrt{\text{employment}}$			Linear–weighted by $\sqrt{\text{employment}}$		
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.297***	0.443***	0.587***	0.119	0.390***	0.646***
	(0.0618)	(0.0776)	(0.116)	(0.0911)	(0.0945)	(0.156)
Window	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
# cells	31	31	31	31	31	31

NOTES: Columns (1) to (3) report estimates analoguous to those presented in Table 2 (see also equation 2) except that Panel B. and B. weight each firm-level observation by its 2012 employment and its square-root respectively. No controls are included. Columns (4) to (6) presents results from a linear specification where a measure of wage in level (euros per worker per year) is regressed on a measure of surplus per worker (euros per worker per year). See paragraph **Level regressions.** from section 5.1 for more details on the constructions of the cells.

Magnitude and comparison with earlier works. The estimates for the strictest estimation samples – samples including firms with at least 30% and respectively 50% of their wage bill paid to worker earning a wage close to the eligibility cutoff – range between 0.4 and 0.6, implying a pass-through between 40 and 60%. This magnitude is close to wage incidence results documented in several recent works in the corporate income tax literature (Arulampalam *et al.*, 2012; Liu and Altshuler, 2013; Fuest *et al.*, 2018), which imply that employees bear about 50% of the corporate tax burden. Studying the US, Suárez Serrato and Zidar (2016) find the corporate income tax wage incidence to be a little smaller, around 30%.

Our work also relates to the rent-sharing literature that studies how changes in firm surplus translates into wages. In this literature, some papers estimate money metric measures of pass through, as in Kline *et al.* (2019). They find that employee earnings increase by about 29 cents of every dollar of patent allowance-induced surplus (by 61 cents for firm stayers). We also estimate euro-for-euro measures of pass-through (3). Our estimates are somewhat larger, between 0.43 and 0.64 for more restrictive samples. Other papers estimate elasticities, with the dependent variable being the log of wages, and the independent variable the log of a measure of firm surplus per worker. These elasticities a generally smaller, between 0 and 0.3 (see Card *et al.*, 2018, in particular their Table A1). Due to differences in specification—our independent variable is a tax rate—our estimates are not directly comparable.

A potential explanation for why our money metric pass-through estimates tend to be larger than Kline *et al.* (2019) is that the surplus shock we study is net-of-tax, while Kline *et al.* (2019) use a pre-tax measure of surplus.[37] This observation echoes Card *et al.* (2018) who highlight that rent-sharing estimates are sensitive to the measure of surplus-per-worker. Our estimates may be closer to those found in the corporate tax incidence literature because they also consider changes in after-corporate tax surplus.

Finally, it is also possible that the reason why our estimates are larger is because of the specific features of the policy studied. For instance, its schedule explicitly linked the tax credit to wages, and social partners were encouraged to monitor the use of the tax credit money (Carré and Blein, 2014). These two features might have prompted employees and their representative to ask for raises.

We now turn to assessing the robustness of our results to different potential threats.

5.2 Robustness

Rationale for within cell estimation. In our empirical strategy, we use cell-year fixed effects to compare firms that have a similar distribution of wages, except immediately around the 2.5 minimum wage cutoff.[38] Including cell fixed effects, we exploit variation in treatment within groups of firms with a similar wage distribution. The resulting variation is more likely to be exogenous, as it is influenced by the arbitrary policy cutoff rather than by the firm's underlying production function. Without cells, our specification is akin to a standard gap approach. Differences in policy exposure are larger, but firms are also less comparable. Identification rests on comparing low-wage firms to high-wage firms. It is not an issue if these firms differ in levels, a difference taken into account by firm fixed effects, as long as they follow parallel trends. Several reasons, including pre-existing policies whose schedule is discontinuous at 1.6 MW, the propagation of minimum wage increases in the bottom of the wage distributions, and more generally differing growth trends between higher and lower wage firms, cast doubt on the plausibility of the parallel trend assumption. Yet, ultimately, it is an empirical question, which we take to the data.

[37]In our case, any policy-induced surplus shock directly increases after tax profits. By contrast, a one dollar patent-induced surplus (or labor cost decrease) is associated with a $1-\tau_{\text{CIT}}$ dollar increase in after tax profit. Rescaling our estimates by $1-\tau_{\text{CIT}}$ and assuming an effective marginal corporate tax rate of roughly 30% in France, we obtain estimates hovering around 35% rather than 50%. We multiply our point estimates by $1-\tau_{\text{CIT}}$ because, in order for the after-tax surplus to increase by 1 euro, the pre-tax surplus must go up by $\frac{1}{1-\tau_{\text{CIT}}} > 1$ euros.

[38]Cells are defined in Section 4.

Figure A4 illustrates how the inclusion of cell-year fixed effects affects estimates. Coefficients from the event study specification without cell fixed effects are reported in black, while coefficients obtained without cell fixed effects are in gray. Figures (a), (b) and (c) correspond to the 0%, 30% and 50% samples respectively. Dashed vertical lines visually represent the point estimates of the difference-in-differences specification. They are overall similar with and without cell fixed effects. Yet, when cell fixed effects are not included, the parallel trend assumption is rejected based on pre-trends. It is not surprising as the identifying variation stems from comparing very dissimilar firms. Wages in low-wage firms (very treated) were increasing more slowly than in high-wage firms (less treated) in pre-reform years. Yet, a clear rebound can be observed in 2013, suggesting that the policy had a positive impact on wages. Including cell fixed effects, the parallel trend assumption seems to hold for all the samples, and a clear increase is visible starting in 2013. Overall, these estimates lend support to the idea that, in this setting, it is not possible to causally interpret difference in differences estimates when failing to compare firms with similar wage distributions. Including cell fixed effects is an effective way to compare similar firms that are differently exposed to the policy.

Alternative definitions of bins. We first show that our results are robust to the size of bin we choose. Figure A5a plots event study estimates of Equation 3 as in Figure 5, except that bin fixed effects are defined using alternative bin widths. The baseline bin width is 3.33 percentage points. Grey estimates correspond to a bin width of 2 percentage points. Black estimates correspond to a bin width of 5 percentage points. Corresponding difference in differences estimates are reported in Table A3. Our results do not change. Coefficients remain non-significant pre-reform and become positive and significant after 2013 in all specifications. The magnitude of the effect is also very similar.

Next, we show that our results are robust to the wage thresholds we use to define bins. We have defined bins using the payroll share of workers paid less than 2.2 and 2.8 MW. We now compare our results with that obtained when using payroll share of workers paid less than 2.3 and 2.7 MW to define our bins. Figure A5b presents event study estimates using this alternative definition of bins. Table A4 reports the corresponding difference in differences estimates. Results are similar. Pre-reform coefficients are not significantly different from zero while post reform ones gradually increase over time.

Alternative base year of the instrument. We use the pre-reform wage structure of firms to predict their exposure to the policy. We have use the year 2012, the year before the policy was implemented, to compute this instrument. Figure A6 plots event study estimates using other reference years to define our instrument. Again, our results are unchanged. Pre-reform coefficients are all close to zero and statistically non significant. Post reform coefficients gradually increase from 2013. The magnitude of the estimated coefficients is very close to that of the baseline in Figure 5. Table A5 reports corresponding difference in differences estimates and present coefficients very close to those presented in Table 2. Robustness to the year used to define the instruments and bins suggest that serial correlation is not a concern here. Indeed, some papers have documented that instruments *à la* Auten and Carroll (1999) could be affected by mean reversion (see Weber, 2014, for an application to the elasticity of taxable income literature).

Placebo. To further test the robustness of our empirical design, we implement a placebo test using a fictional policy schedule. Namely, we conduct our empirical analysis as if the tax credit amounted to a share of the payroll of workers paid less than 3.1 MW, instead of 2.5 MW in reality. We use 2.9 and 3.3 MW as wage thresholds to define bins. We set the new cut-off to 3.1 MW such that it is low enough to have sufficient local variation in the distribution of wages around it, but not too low so the widow used to define the bins does not overlap with our previous bounds. Figure A7 presents event study estimation results. Both pre- and post-reform coefficients are not statistically significant.

External validity. Our approach uses variation in the distribution of wages near the eligibility cutoff and relies on firms that have a sensible share of their wage bill share paid to workers earning a wage close to 2.5 MW. This design may come at the expense of external validity, especially regarding our preferred estimation sample: firms that have at least 50% of their wage bill paid to workers earning between 2.2 and 2.8 MW. Table A1 shows that these firms, when compared to the population of firms, are smaller, pay higher wages, and more likely operate in services.

To gauge the sensitivity of our estimates to the industrial and size composition of our sample, we reweight observations of our three samples such that they display the same characteristics as the full sample of firms taking up the policy on these dimensions.[39] We use Hainmueller (2012)'s entropy balancing method. Entropy weights yield the closest possible balance between two samples regarding the first moments of designated variables. We use employment and indicator variables for industries. Table A6 reports the weighted statistics for these variables. The first moments match almost exactly matched those of the full sample of firms taking up the policy.

Table 4 reports estimates of our baseline specification when weighting observations using entropy weights. To the extent that responses are heterogeneous across sectors,

[39]The target sample is the "All firms" sample in Table A1.

point estimates should be affected by our reweighting procedure (Solon *et al.*, 2015).[40] We find that the coefficients are very similar and remain significant. Overall, these results indicate that our results are not driven by the industry and size composition of our samples.

Table 4: Weighted estimation to match size and industry mix of population of firms

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.326***	0.326***	0.400***	0.400***	0.514***	0.513***
	(0.0679)	(0.0679)	(0.0935)	(0.0936)	(0.142)	(0.143)
Observations	797699	797699	155925	155925	41811	41811
R^2	0.931	0.931	0.852	0.852	0.765	0.765
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	113957	113957	22275	22275	5973	5973

NOTES: This table reports estimates of Equation 2. Observations are weighted such that the industry mix and employment of firms in the samples match their counterpart in the population of firms taking up the policy. Other notes as in Table 2.

Firm profitability. Typically, only firms liable to a tax benefit from tax credits. The CICE is slightly different, as even firms that are not profitable can benefit. First, even if a firm is not liable to the corporate income tax, it will get the amount of tax credit it is entitled to after three years. Second, firms meeting some legal requirements (SMEs or firms in financial distress for instance) do not have to wait three years, and can claim the tax credit after one year, whether or not they are profitable. Despite these features, on average, profitable firms will tend to receive the tax credit more rapidly. Accordingly, the present value of the tax credit will be higher for profitable than for non-profitable firms.

Therefore, both profitable and nonprofitable firms are likely to respond to the policy but profitable firms' response is expected to be stronger. To test this hypothesis, we interact the treatment with indicator variables of profitability status. A firm is considered profitable if it was liable to the corporate income tax in 2012, the last pre-reform year, not profitable otherwise.[41]

Table 5 reports wage incidence estimates based on firms' 2012 profitability status. The bottom row of the table presents the p-value of a test of equal coefficients for

[40] Note that the weighted and unweighted estimates could be similar for two reasons: either there is indeed limited heterogeneity along the dimension for which we are re-weighting or the weights are fairly uniformly close to 1 as there is very limited unbalance between the estimating sample and the targeted sample among the population studied.

[41] The specification writes:

profitable and non profitable firms. We find that both profitable and not profitable firms responded to the policy by increasing the wage of their employees. The wage pass-through is substantially larger in profitable firms however. For all specifications, we can reject that the two coefficients are equal with a significance level below 1%. The wage effects are about two times larger in profitable firms than in non profitable ones.

Table 5: Impact on wages based on pre-reform firm profitability status

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\} \times \mathbb{1}\{\text{Non Profitable}_i\}$	0.210***	0.210***	0.229***	0.234***	0.352***	0.358***
	(0.0644)	(0.0644)	(0.0830)	(0.0831)	(0.128)	(0.129)
$Z_i \times \mathbb{1}\{t \geq 2013\} \times \mathbb{1}\{\text{Profitable}_i\}$	0.439***	0.439***	0.589***	0.590***	0.705***	0.706***
	(0.0639)	(0.0639)	(0.0801)	(0.0801)	(0.121)	(0.121)
Observations	830144	830144	159551	159551	42854	42854
R^2	0.935	0.935	0.863	0.863	0.773	0.773
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	118592	118592	22793	22793	6122	6122
p-value $\beta_p = \beta_{np}$	0.00	0.00	0.00	0.00	0.00	0.00

NOTES: This table reports the estimated impact of the CICE tax credit on wages depending on the profitability of firms. A firm is considered profitable if it paid a strictly positive amount of corporate income tax in 2012, not profitable otherwise. Estimated coefficients correspond to Equation 5. All regressions include cell × year FE, as defined in Section 4.2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Other notes as in Table 2.

County-year fixed effects. Our empirical approach uses local variation in wage distribution around the cutoff to isolate quasi-random random variation in policy exposure. To the extent that our approach is successful, the variation we exploit should not systematically be related to the features of the local labor markets in which firms operate. To assess whether spatial heterogeneity drives our results, we add county (*département* in French) × year fixed effects in our main specification. We first associate all firms with a county based on their headquarters (HQ) and include HQ county × year fixed effects. Next, we estimate this specification only for a sample of firms operating in a single county and include county × year fixed effects. Estimates are presented in A7.

$$\begin{aligned}\ln(Y_{i,t}) &= \alpha_i + \alpha_{c,t} \\ &+ \beta_1 \cdot Z_i \cdot \mathbb{1}\{t \geq 2013\} \cdot \mathbb{1}\{\text{Non Profitable}_i\} \\ &+ \beta_2 \cdot Z_i \cdot \mathbb{1}\{t \geq 2013\} \cdot \mathbb{1}\{\text{Profitable}_i\} \\ &+ X'_{i,t}\gamma + \varepsilon_{i,t}\end{aligned} \tag{5}$$

Overall, coefficients are very similar to our baseline estimates in Table 2. It implies that our empirical strategy is robust to the presence of time-varying spatial shocks.

5.3 Heterogeneity

Results so far show that, on average, workers benefited from the policy through substantial wage gains. In this section, we explore whether all workers benefit uniformly or, rather, if incidence varies across skills, incumbency and gender.

5.3.1 By skill level

A recent body of work has stressed the importance of occupations (Caliendo *et al.*, 2015) to understand within-firm wage dynamics. Kline *et al.* (2019) posit employees in occupations where incumbents are least substitutable with new hires—due to training costs for instance—enjoy higher wage premia.

Motivated by this body of work, we split workers into two occupational groups based on the level of skills required. The first group includes senior staff, professionals, associate professionals, technicians, and employees at the supervisor level, corresponding to classes 3 and 4 of the French occupational classification system.[42] The second category encompasses clerical employees and blue-collar workers, i.e. classes 5 and 6. We refer to these groups as *high-skill workers* and *low-skill workers* respectively.

Figure 1 plots the distribution of gross hourly wage for these two groups in 2012. Low-skill workers are almost all paid less than 2.5 MW, implying they all open rights to the tax credit. High-skill workers are paid higher wages and 44% of them are paid more than 2.5 MW.

Table 6 reports difference in differences regression estimates by skill groups. The outcome variable is the skill-specific mean gross hourly wages. The positive wage effects of the policy only benefit high-skill workers. A one percentage point increase in the tax credit rate increases the wages of high-skill workers by 0.6%. By contrast, we find no effect of the policy on low-skill workers. Coefficients are all close to zero and precisely estimated.

Figure 6 plots the corresponding event study regression results. For both skill groups, pre-reform coefficients are close to zero, lending support to the common trend

[42] We group these two classes although they have been shown to be different (Caliendo *et al.*, 2015) to have enough firms with at least one worker of this group every year over the period studied.

Table 6: Impact on hourly wage earnings (log), by skill group

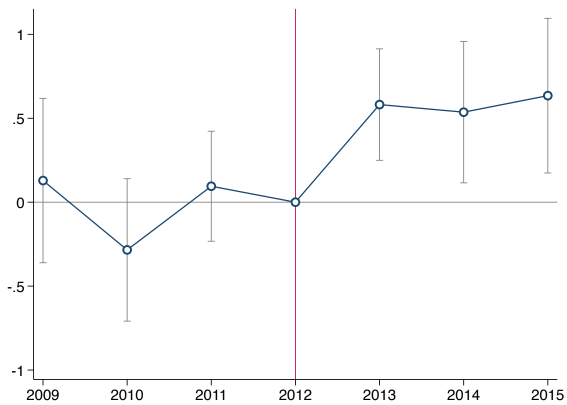
	Mean hourly wage (log)					
	High skill workers			Low skill workers (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.384***	0.465***	0.602***	-0.0422	-0.0144	-0.0485
	(0.113)	(0.132)	(0.180)	(0.0762)	(0.0988)	(0.159)
Observations	691992	121282	30559	793648	141315	35245
R^2	0.812	0.760	0.700	0.853	0.824	0.813
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	106597	19228	4910	116137	21437	5513

NOTES: This table reports the estimated impact of the CICE tax credit on wages of workers by skill groups. The unit of observation is the firm. The dependent variable is the mean gross hourly wage of continuing workers of a given skill level, weighted by hours worked. The independent variable is the 2012 payroll share of workers earning less than 2.5 MW multiplied by the tax credit rate and interacted with an indicator variable equal to one after 2013. Other notes as in Table 2.

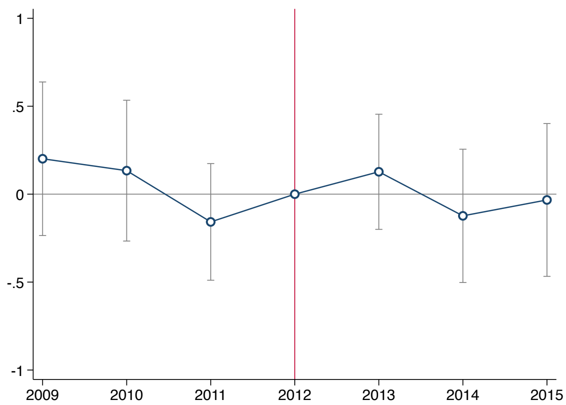
assumption. For high-skill workers, the effects of the policy on wages gradually unfolds. By contrast, the policy has no effect on wages for low-skill workers.[43]

Figure 6: Impact on mean hourly wage (log), by skill group

(a) High skill employees



(b) Low skill employees



NOTES: This figure plots the estimated impact of the CICE tax credit on the wage earnings of workers, based on their skill level. Other notes as in Figure 5.

[43]Figure A8 plots the evolution of the weighted gross hourly wage (base 1 in 2012) in two groups of firms, separately for the high and low-skill workers. The two groups of firms—according to whether their policy exposure is above or below the within-cell median—have parallel trends prior to the reform. We find that the group the most exposed to the policy experiences faster growth in mean hourly wages for high-skill workers but not for low-skill workers.

We check the sensitivity of our results on wages by skill groups. Figures A9 and A10 plot event study regression coefficients corresponding to Equation 3 with alternative definitions of cells, either using larger or smaller steps to define them, or a smaller window around the cutoff. Tables A9 and A10 report the corresponding difference in difference coefficients, estimated from Equation 2. Tables A11 and A12 present results from a specification in which other pre-reform years are used to define policy exposure and cells. Figure A11 present placebo event study estimates, assuming a policy threshold at 3.1 MW. Finally, Table A13 gauges the sensitivity of our results to the inclusion of region-year fixed effects. Overall, results are very stable across specifications.

Our results have two implications. First, while the policy has a sizable effect on wages, this effect varies dramatically across groups of workers. It suggests that within-firm mechanisms are crucial to understand how taxes impact labor income inequalities. Further, although tax incentives target mid- and low-wage workers, the surplus generated by the policy does not "trickle down" to low-wage workers. Compared to tax cuts, tax credits allow policymakers to create a link between tax benefits and desired outcomes. Yet, as demonstrated in this setting, this link can be muted as statutory incidence does not necessarily coincide with economic incidence: the tax credit does not benefit low-wage workers but instead spills over to workers less likely to be eligible.

5.3.2 By incumbency status

Several works highlight that incumbent workers benefit most from rent-sharing, especially if they are less substitutable with new entrants (Kline *et al.*, 2019; Howell and Brown, 2019). To test whether this hypothesis could apply in our setting, we divide workers into two groups, one of "incumbents" and one of "entrants". We define "incumbents" as employees who were working in the same firm in the past year. Other workers are labeled "entrants."

Table 7 reports difference in differences estimates of the impact of the tax credit on gross hourly wages of incumbents and entrants. The positive effects of the policy load on incumbent employees. An increase in the predicted effective tax credit rate by 1 percentage point translates into a 0.65% increase in gross hourly wage of incumbent workers. Coefficients reported in Table A16a show that among incumbents, high-skill workers only benefit from wage gains.

Overall, the profit windfalls generated by the CICE have distributive effects similar to that of profit windfalls studied in Kline *et al.* (2019)[44] and Howell and Brown (2019). This set of results suggests that the mechanisms underlying the wage pass-

[44]They find that incumbent workers captured 61 cents of every dollar of patent-induced surplus.

Table 7: Impact on hourly wage earnings (log), by incumbency status updated

	Mean hourly wage (log)					
	Incumbents			Entrants		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.386***	0.459***	0.612***	-0.0120	0.284	0.0952
	(0.0722)	(0.0892)	(0.128)	(0.189)	(0.269)	(0.492)
Observations	792288	150532	40402	589030	73029	13633
R^2	0.915	0.824	0.723	0.667	0.652	0.652
Window	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	118410	22766	6119	108423	17233	3703

NOTES: This table reports the estimated impact of the CICE tax credit on wages of incumbents and entrants. An incumbent is an employee who was already working for the firm in the past year. An entrant is an employee who was not working in the firm in the past year. Other notes as in Table 6.

through might not be specific to the population of firms studied, or the type of policy generating the surplus.

5.3.3 By gender

We finally consider the heterogeneity by gender. Table A15 reports difference in differences estimates of the impact of the tax credit on gross hourly wages of men and women. Overall, we find that women benefit from smaller wage gains than men. Incidence estimates are far less contrasted than for skill groups, and the differences hold focusing on incumbents (Table A16b).

6 Mechanisms

We find that a substantial share of the tax credit was passed onto employees but that, among them, only high-skill and incumbents get higher wages. In this section, we explore the potential mechanisms driving our results. We first analyze and rule out individual-level responses to the policy and classical production adjustments. We then study why only high-skill incumbents are able to get higher wage.

6.1 Individual-level analysis

The CICE sharp eligibility cutoff entails a notch in the net-of-tax credit labor cost. We investigate through a bunching analysis whether firms respond to this notch by substituting ineligible employees for eligible ones. We frame our analysis in a simple wage posting framework.

Standard wage posting model. In a standard wage posting model (e.g. Manning, 2003), firms with given levels of productivity p face a trade-off when posting wage w. To maximize the expected profit associated with a vacancy, firms strike a balance between increasing the probability that a vacancy is filled by offering a higher wage w on the one hand, or the vacancy surplus in case it is filled $(p - w)$ by offering a lower wage on the other hand. As a result, the expected profit is concave in w for a given p.

In a pre-reform equilibrium where reservation wages and productivity types are continuously distributed, optimal posted wages $w^*(p)$ are continuous. The policy introduces a notch in the net-of-tax credit profitability of a vacancy $p - w \cdot (1 - \tau \cdot \mathbb{1}_{(w<2.5MW)})$, with no direct impact on the probability that it is filled. A firm whose optimal posted wage is just below the eligibility cut-off benefits from a profit windfall and has no incentive to change its posted wage. A firm whose productivity is just slightly higher and whose optimal posted wage is just above the eligibility cut-off has an incentive to post a wage just below the eligibility cut-off as doing so only marginally decreases the probability the vacancy is filled while generating first order gains in profit conditional on filling the vacancy. As a result, the distribution of wages should exhibit an excess mass to the left of the cut-off and a missing mass to its right (Kleven and Waseem, 2013).[45]

Effects on wages. A graphical and transparent way to test whether firms change their wage setting behaviors after the reform is to study the distribution of wages around the eligibility cut-off. An excess bunching and hole pattern would indicate that firms respond to the tax incentive created by the discontinuity in individual eligibility of workers. Conversely, an absence of deformation in the distribution of gross wage earnings would indicate that the payoff of each employer-employee match is discontinuous. The wage incidence documented at the firm-level would not be driven by worker-level responses, but would instead reflect a collective incidence that cannot be evidenced at the individual level.

Figure 7 depicts the distribution of gross hourly wage earnings around the eligibility cut-off for different time periods. We consider two periods: 2009-2012 is the

[45]We provide a formal description of the model and a characterization of the bunching region in Appendix OA3.

pre-reform period and 2013-2015 is the period after the reform. The solid black vertical line depicts the wage eligibility threshold. Figure 7a plots the distribution of wages for all employees of firms included in the estimation sample. By construction, all employees work in firms that claimed the tax credit. Figure 7b includes only new hires, defined as workers starting a new job in a new firm during the year. Figure 7a shows that the distribution of wages is very smooth around the eligibility threshold both before and after the reform. No excess bunching and hole pattern is visible. We test for discontinuity in the distribution at the cut-off after the reform using a McCrary test (McCrary, 2008) and find no statistically significant discontinuity (coefficient = 0.0044, s.e. = 0.0046).

A potential explanation for this absence of behavioral response is that firms might face constraints when setting wages. For instance, downward nominal wage rigidities may prevent employers from decreasing workers' wage as desired. To lower the potential influence of wage-setting constraints, we restrict our sample to new hires.[46] Figure 7b plots the distribution of gross hourly wage earnings of newly-hired workers, and even this subsample displays no excess mass and hole pattern. A McCrary test yields no significant discontinuity estimate (coefficient = -0.0021, s.e. = 0.012). In Appendix Section A.5.1 we formally test for the presence of bunching using methods developed by Saez (2010); Chetty *et al.* (2011) and Kleven and Waseem (2013). We find no evidence of bunching in the distribution of wages.

Effects on wage growth. We explore another margin of response: wage growth. Employers might be reluctant to increase the wages of workers just below the eligibility cut-off to keep benefiting from the tax credit. They may also not want to increase wages just above the threshold as its nominal level increases annually along with the nominal minimum wage. As a result, wage growth of employees close to the cut-off might be locally lower.

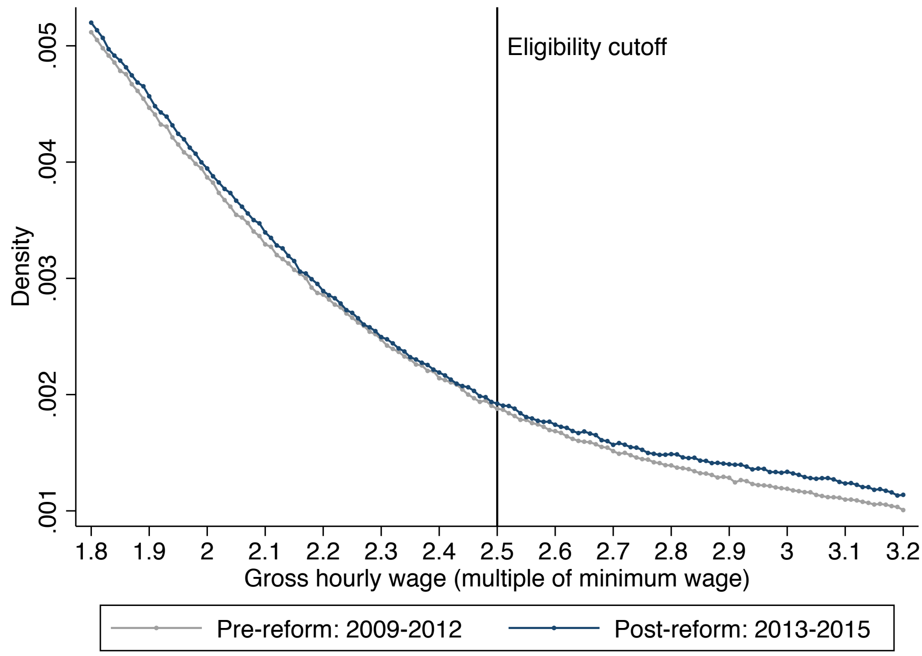
Figure 8 depicts the mean year-on-year wage growth of continuing workers for bins of past year's wage for two time periods: before the reform (2009-2012) and after the reform (2013-2015). Wage growth rates are centered on the period mean growth rate in the considered interval. Continuing workers are workers with a permanent contract who had the same job in the same firm the year before.[47] Wage growth does not appear slower for workers close to the eligibility cut-off. The shape of the two curves are very similar for both periods, especially around the threshold.

[46] Although new hires' wage setting is not exempt from frictions, there is evidence that their wages tend to be more flexible than that of incumbent employees (e.g. Haefke *et al.*, 2013). In particular, they are not subject to within-contract downward wage rigidities. To the extent that firms do respond to the notch, we expect bunching in the wage distribution of new hires to be more visible than in the wage distribution of all employees.

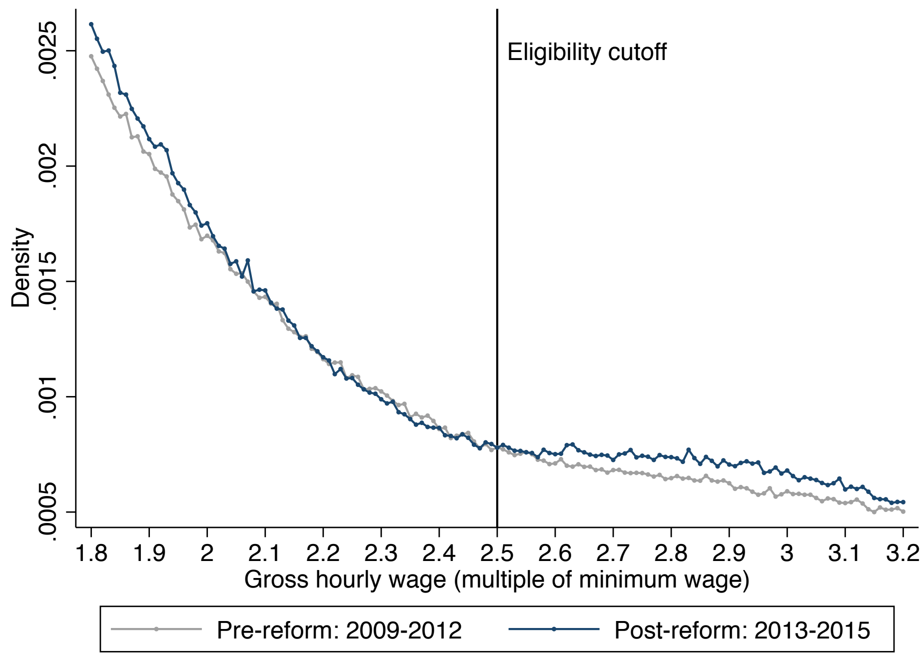
[47] We exclude fixed term contract because by law employers are required to give them an end-of-term bonus that amounts to 10% of the total gross wage earned during the time of that contract.

Figure 7: The Effect of Individual Eligibility on Gross Wages

(a) All Employees

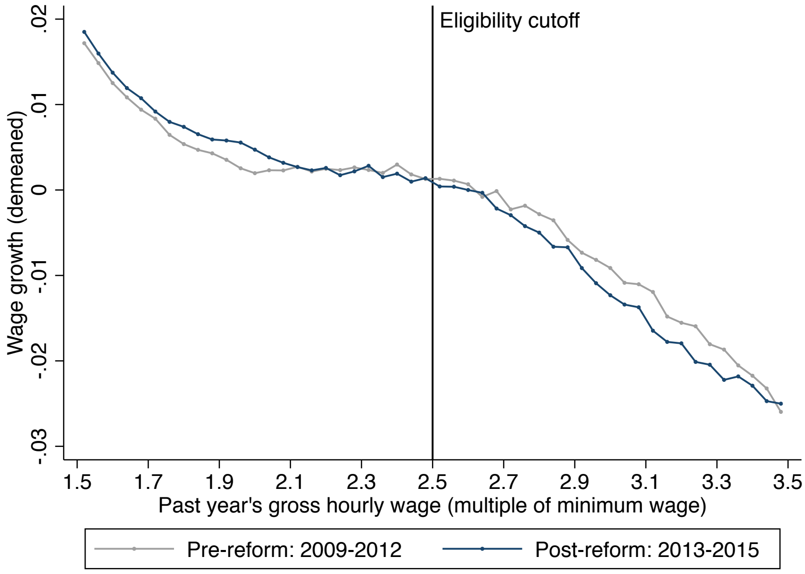


(b) New hires



NOTES: This figure depicts the distribution of gross hourly wages expressed as multiples of the minimum wage in France for years before the reform (2009-2012) and years after the reform (2013-2015). The vertical solid line depicts the wage threshold under which workers open rights to the tax credit. The sample includes all employees (Panel a) in firms included in the firm-level estimation sample. The sample is restricted to new hires in Panel b. New hires are workers starting a new job in a new firm during the year.

Figure 8: Distribution of wage growth of continuing employees



NOTES: This figure depicts the relationship between the year-on-year wage growth rate and past year's wage for two time periods, before and after the reform. Wages are expressed in multiples of the minimum wage. Wage growth is centered on the period's mean wage growth in the [1.5 MW, 3.5 MW] wage range.

We formally test for local distortions in wage growth. Excluding a range of observations around the notch, we estimate a counterfactual relationship between wage growth and past wage. We then compare the counterfactual to the observed wage growth. We provide a detailed description of our estimation procedure in Section A12. Table A18 reports estimation results.Point estimates captures the difference between the observed and the counterfactual wage growthsbefore and after the reform. All coefficients are close to zero, none is statistically significant.

As wage growth is almost always positive, this absence of reaction is unlikely to be driven by downward wage rigidity. The implication of our findings is that the net-of-tax credit labor cost of otherwise similar employees is persistently different after the policy is implemented.

Discussion. We find no evidence that firms responded to the individual eligibility status of employees, either by setting wages just below the threshold or by slowing down wage growth. Bunching estimates can be attenuated by frictions (Kleven, 2016), and we consider below two sources of bunching attenuation. As a consequence, bunching estimates cannot be directly translated into structural elasticities (Kleven, 2016), and no bunching does not necessarily imply a zero average structural elasticity of labor demand.[48]

First, the threshold we study is not expressed in nominal terms, but in multiples of the national minimum wage, which increases over time. For this reason, firms might find it difficult to determine eligibility and make optimization errors. However, it is implemented on January 1st,[49] meaning that when hiring, employers can easily compute the eligibility threshold for the current civil year.[50] Employers might also have been unaware of the threshold, but it seems unlikely given that the notch is large, and we study wages up to three years after the policy implementation.

Second, there might be frictions (collective agreements, occupational wage floors, etc.) preventing employers from setting wages as desired. Collectively bargained wage floors are a source of wage rigidity in France (Gautier *et al.*, 2019), but very few are binding for wages around 2.5 MW.[51] Nominal wage rigidity may also apply, but these frictions should not affect newly hired employees as much. Overall, although the policy induces a large notch in implied labor costs, we find no bunching, even for the wage distribution of new hires. It suggests that the absence of bunching is not entirely driven by frictions

The findings of our individual-level analysis imply that the net-of-tax credit labor cost and the profit derived from these employer-employee matches remain discontinuous at the cutoff, even several years after the policy is implemented. Workers on both sides of the threshold are likely to be perfect substitutes, and yet entail different costs for firms.[52] Taken together, our results support the idea that firms did not respond to the large notch induced by the policy by substituting ineligible workers for eligible ones.

6.2 Employment

The heterogeneous wage incidence we observe may be related to employment effects. First, because the policy decreases the marginal cost of production, firms might in-

[48]Telling frictions apart from structural elasticities is in principle doable applying the techniques in the spirit of Kleven and Waseem (2013); Kleven (2016) but is beyond the scope of this paper.

[49]This is the case from 2013. Increases are generally announced in advance, not on January 1st, such that firms have time to anticipate.

[50]Naturally, employers might be a bit cautious and set wage at 2.40 or 2.45 instead of 2.5^{-}. This implies not necessarily a sharp bunching, but at least some deformation of the distribution of wage offers which we fail to detect. Note moreover, that the national minimum wage is very salient in France as other policy thresholds are expressed in multiples of the minimum wage. For instance, payroll tax cuts in the 1990s targeted employees paid up to 1.6 MW. It has been growing fairly slowly. Between 2009 and 2012, it increased by an average of 1.4% per year.

[51]In 2012, wage floors above 2.4 MW applied to only 3.2% of employees, while 20% of employees earned at least 2.4 MW (Gautier, 2017).

[52]The net-of-tax credit labor cost difference is equal to 1,040 euros, as computed using using the 6% tax credit rate applicable in 2014 and 2015.

crease their scale of production and, to do so, hire more. To the extent that skill premium varies with firm size (Mueller *et al.*, 2017)[53], scale effects might be increase the relative wage of high-skill workers. Next, if employment effects are different across skill groups, the marginal productivity of workers in the two groups will also adjust. Assuming complementary between the two skill groups, an increase in the number of low-skill workers will raise the productivity of high-skill workers. As a result, the wage of non-eligible workers will be impacted by the tax reform. We explore these potential mechanisms below.

Overall employment and sales. Table 8 reports the results of the estimation of equation (2) using employment as dependent variable. We measure employment as the number of employees in each firm, irrespective of their contract type. Figure 9 shows the result of the estimation of equation 3 with employment as the dependent variable. We find no employment effect. Standard errors are large, especially for stricter samples (30% and 50%) and we cannot rule out that the policy had a positive effect on firm-level employment with a great degree of confidence. Using the standard error in Column 4 (Column 6), the 95% confidence interval centered on zero excludes elasticities above 0.44 (0.62).[54] While fairly large, these upper bounds are smaller than elasticities often found in the literature. We cannot rule out that the policy had positive employment effects, but if so, the implied labor demand elasticities were small compared to those associated with other policies.[55] Table A19 and Figure A13 present similar estimates with sales as dependent variables. Coefficients are all close to zero and non significant. Although the policy lowers production costs, firms did not seem to expand.[56]

While other papers studying comparable policies find significant employment effects (in particular Saez *et al.*, 2019, studying payroll tax cuts targeted at young employees), there are several potential explanations for why firms did not expand. Due to fixed hiring costs,[57] firms could choose not to adjust their labor force even when labor

[53]This could happen if production function are non-homothetic for instance and the relative productivity of high skill workers increases with scale of production.

[54]Our explanatory variable is the effective tax credit rate, which is approximately equal to the predicted percent change in labor cost. Point estimates of Table 8 can be interpreted as estimated firm-level average labor demand elasticities.

[55]In France, using minimum wage and payroll tax reforms in the 1990s, Kramarz and Philippon (2001) find a 1.5 elasticity of labor of demand. Exploiting the same type of reforms, Crépon and Desplatz (2001) estimate firm-level labor demand elasticities of hovering around 1.7. Analyzing a temporary hiring tax credits implemented in 2009 in France, Cahuc *et al.* (2019)'s results imply a labor elasticity in excess of 2. In the German context, Lichter *et al.* (2017) find firm-level elasticities ranging between 0.6 and 0.7. In the US, Monras (2019) find local (state-level) labor demand elasticities above 1. Using a more structural approach, Suárez Serrato and Zidar (2016) find local demand elasticities above 1.5.

[56]In line with our result, Fuest *et al.* (2018) find that higher corporate income tax rates do not lead to higher unemployment.

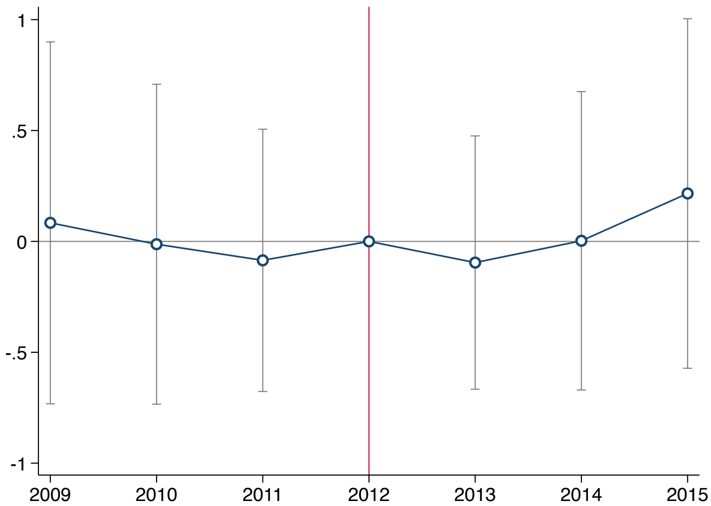
[57]Kline *et al.* (2019) find that the cost of replacing an employees amounts to about a new hire's annual earnings.

Table 8: Impact on employment (log) updated

	Number of employees (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	-0.150	-0.149	-0.180	-0.174	0.0325	0.0447
	(0.187)	(0.187)	(0.226)	(0.226)	(0.319)	(0.319)
Observations	830144	830144	159551	159551	42854	42854
R^2	0.969	0.969	0.923	0.923	0.878	0.878
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	118592	118592	22793	22793	6122	6122

NOTES: This table reports the estimated impact of the CICE tax credit on employment. The unit of observation is the firm. The dependent variable is the number of full time equivalents at the firm level. The independent variable is the 2012 payroll share of workers earning less than 2.5 MW multiplied by the tax credit rate and interacted with an indicator variable equal to one after 2013. Estimated coefficients correspond to Equation 2. Regressions include cell $\times$ year FE, as defined in Section 4. We use the payroll share of workers paid less than 2.2 and 2.8 MW to build bins. We use a 3.33 percentage points discretization step. Controls include the 2012 payroll share of workers earning less than 1.5 MW interacted with a full set of year dummies. Robust standard errors clusters at the firm level in parentheses. *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Figure 9: Impact on employment (log), event study



Notes: This figure plots the point estimates and 95-percent confidence intervals from the event study regression defined in Equation 3. The unit of observation is the firm. The dependent variable is the log of the number of full time equivalents. The independent variable is the 2012 payroll share of workers earning less than 2.5 MW multiplied by the tax credit rate and interacted with year dummies. 2012 is the reference year. The sample includes firms with at least 50% of their payroll paid to workers earning between 2.2 and 2.8 MW. Bins are defined as in Table 2. Lagged controls are included. Robust standard errors are clustered at the firm level.

costs decrease (Bentolila and Saint-Paul, 1994). Policy uncertainty (France Stratégie, 2015) regarding the continuation of the tax credit might have expanded their zone of inaction, in particular given the high firing costs in France (Kramarz and Michaud, 2010). The macroeconomic environment was also unfavorable, which might have discouraged firms from hiring in spite of lower labor costs. The policy design may also have muted the employment effects of the policy. As suggested by qualitative research (Carbonnier *et al.*, 2016), a tax credit, handled by accounting services, may be less salient than a payroll tax cut when it comes to hiring. Moreover, the cash effects take longer to materialize, especially for less profitable firms which might be more cash constrained. These are the firms which may have increased employment the most in response to lower labor costs, as suggested by Saez *et al.* (2019).

Employment by skill level. Table 9 reports the results of the estimation of equation (2) using employment by skill group as separate dependent variables. Estimated coefficients are not statistically significant for both groups. Yet, standard errors remain quite large. In order to gain in precision, we also use the share of high-skill workers as a dependent variable. Estimates presented in columns (1) to (3) of Table 10 imply that the policy had a small positive and significant effect on the share of high skill workers. A 1% decrease in labor costs translates in an increase in the share of skilled workers of +0.5pp. For reference, the mean share of high skill workers in the most restrictive sample is 52%. A 4% decrease in labor costs would have therefore increased the share of skilled workers to 54%. The tax policy had moderate effects on firms' skill mix, in favor of skilled workers. Tax incidence only benefiting high skill workers cannot be explained by productivity gains due to an increase in low skill employment.[58] Columns (4) to (6) of Table 10 display results using the ratio of newly hired high skill workers to overall employment. We find no evidence of a positive impact on the share of high skill new hires. Accordingly, the overall impact documented on the overall share of high-skill among employees is probably not driven by changes in hiring behaviors but instead by differential patterns of retention / separation across skill groups. We consider this issue directly in the section below.

Overall, results from sections 6.1 and 6.2 suggest that the policy targeting does not seem relevant to explain how the gains are distributed as the policy surplus spills over

[58] As previously noted in the literature (see in particular Saez *et al.*, 2019, page 1738), a well identified limitation of empirical designs similar to ours is that it is generally not possible to study substitution between eligible and ineligible employees. The reason is that our firm-level policy exposure measure is a function of the share of eligible workers in 2012. This share exhibits mean reversion, which masks substitution patterns. Note, however that, although the amount of policy-induced surplus is firm specific as it is a direct function of firm wage structure, the incentive to hire eligible workers generated by the discontinuity in the schedule does not vary across firms. As such, we view our individual-level analysis of the distribution of wage, especially among new hires, as most fitted to study potential substitution patterns. Results presented in Section 6.1 find no evidence of substitution between eligible and ineligible employees.

Table 9: Impact on employment (log), by skill level

	Number of employees (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.0952	0.186	0.139	-0.432*	-0.351	-0.226
	(0.268)	(0.315)	(0.431)	(0.244)	(0.298)	(0.432)
Observations	695166	121994	30746	797433	143025	35782
R^2	0.942	0.925	0.889	0.958	0.880	0.829
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	107161	19368	4952	116571	21644	5575

NOTES: This table reports the estimated impact of the CICE tax credit on employment. The unit of observation is the firm. The dependent variable is the number of full time equivalents at the firm level. The independent variable is the 2012 payroll share of workers earning less than 2.5 MW multiplied by the tax credit rate and interacted with an indicator variable equal to one after 2013. Estimated coefficients correspond to Equation 2. Regressions include cell × year FE, as defined in Section 4. We use the payroll share of workers paid less than 2.2 and 2.8 MW to build bins. We use a 3.33 percentage points discretization step. Controls include the 2012 payroll share of workers earning less than 1.5 MW interacted with a full set of year dummies. Robust standard errors clusters at the firm level in parentheses. *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

onto ineligible workers. Standard labor market mechanisms operating through the production function such as scale effects or complementarity between skill groups do not seem to be driving these effects.

6.3 Retention of workers

Our results suggest that the surplus generated by the tax reform is shared at the firm level between employers and employees. It raises the question of why some groups of employees successfully bargain over this surplus while others do not. Kline *et al.* (2019) propose that firms use the surplus to increase the wage of workers who would be most costly to replace in order to lower their quit rate. They show that the surplus generated by patents translates in higher wages for workers in the top of the wage distribution, thus increasing within-firm earnings inequality.

To test the retention mechanism, we look at the impact of the policy on the retention rate of employees by skill group. The retention rate is defined as the share of continuing employees with a permanent contract who are still working in the same firm in December. Table 11 reports estimation results corresponding to Equation 2. Figures A14 and A15 depict corresponding event studies. We find a statistically significant

Table 10: Effect on the share of high skill employees

	Share of high skill among employees					
	All high-skill workers			Newly hired high-skill workers		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.315***	0.329***	0.553***	-0.00127	-0.0403	-0.0226
	(0.0832)	(0.106)	(0.163)	(0.0403)	(0.0507)	(0.0760)
Observations	830034	159479	42806	830034	159479	42806
R^2	0.929	0.900	0.876	0.621	0.543	0.501
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033

NOTES: This table reports the estimated impact of the CICE tax credit on the proportion of high-skill among the workforce. In columns (1) to (3), the dependent variable is the share of all high skill workers among employees, i.e. the number of highskill workers divided by overall employment. Columns (4) to (6), the dependent variable is the ratio is the ratio of the number of newly hired high skill workers over total employment. An individual is considered as a hire if she was not employed in her current employer in the previous year. The unit of observation is the firm. The independent variable is the 2012 payroll share of workers earning less than 2.5 MW multiplied by the tax credit rate and interacted with an indicator variable equal to one after 2013. Other notes as in Table 8.

effect of the tax credit on the retention rate of high-skill workers. An increase in the predicted effective tax credit rate by 1 percentage point translates into a .55 percentage point increase in the retention rate of high-skill workers. The policy had no effect on the retention of low-skill employees. This pattern parallels the heterogeneity of wage gains across skill groups, suggesting employers increase the wages to retain workers costly to replace.

We compute the implied "retention elasticity" of high-skill workers with respect to wage using retention estimates of Table 11 and high skill wage incidence estimates of Table 6. The implied elasticity is 1.06 (1.01) for the 30% (50%) sample.[59] It is larger than recent estimates in Bassier *et al.* (2020)[60] but close to estimates in Kline *et al.* (2019).[61]

[59]The retention elasticity is computed as follows. Let us denote R the retention rate and w the wage offered to the relevant set of workers. Finally, let us β^x the marginal effect of the policy on x. The retention elasticity is defined as: $e^R \equiv \frac{dR/R}{dw/w}$; and can be approximated as : $\hat{e}^R = \frac{\beta^R/\bar{R}}{\beta^{\log(w)}}$ where $\bar{R}$ is the mean retention rate as of 2012 (see last row Table 11).

[60]Figure 4 in Bassier *et al.* (2020) implies that a value for the semi-elasticity of R with respect to wage, i.e. the equivalent of $\beta^R/\beta^{\log(w)}$ of about 0.1.

[61]See the last row their Table IX. The elasticity implied by our results may be large because we focus on a group of high skill, potentially more mobile employees whose arrival rate of outside options is higher.

Table 11: Impact on employee retention rate, by skill group

	Retention rate					
	(1)	(2)	(3)	(4)	(5)	(6)
	Low-skill workers			High-skill workers		
$Z_i \times \mathbb{1}\{t \geq 2013\}$	-0.0857	0.0478	-0.168	0.435***	0.459***	0.571***
	(0.106)	(0.138)	(0.206)	(0.116)	(0.139)	(0.191)
Observations	739422	125834	30565	640137	109965	27683
R^2	0.457	0.386	0.389	0.420	0.410	0.406
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033

NOTES: This table reports the estimated impact of the CICE tax credit on the retention rate of employees, by skill group. The retention rate is the share of employees who were working in the firm the previous year and are working in the same firm in December of the current year. Other notes as in Table 6.

Differential employee retention and high-skill share. Estimates in Table 10 imply that the share of high skill employees increase, but not because of more high skill employees being hired. Rather, the share of high skills may have increased because incumbent employees are more likely to stay in the firm, that is through differential retention across skills.

We use estimates in Table 11 to measure the extent to which retention could explain changes in high skill shares. Denoting s^H the share of high-skill in total employment, the impact of the policy on s^H, noted ds^H, can be approximated as a function of the policy's impact on the hiring dn^H and retention rates of high-skill workers dr^H. It writes $ds^H = \bar{s}^H \times (dr^H + dn^H)$,, where $\bar{s}^H$ is the mean high-skill share prior the policy (see second to last line of Table 11).[62] We find that the contribution of differential retention to the change in the high-skill share is $dr^H \times \bar{s}^H / ds^H$. It accounts for the large majority of the effect, 80 and 66% for the 30 and 50% samples respectively.

[62]The derivation is a follows. Denote s_t^H the share of high-skill in a given firm at time t. We focus on the two-period case where $t = 0, 1$. Let T_t and H_t be total and high-skill employment (in levels, i.e. number of workers) at time t. The law of motion of high-skill employment is : $H_1 = H_0 + N - S$ where N and S stand for the total number of new hires and of separations respectively. The retention and hiring rate of high-skill workers are defined respectively as $r^H = (H_1 - N)/H_0$ and $n^H = N/H_0$. We can express, the share of high-skill in period 1 as: $s_1^H = (H_1/H_0) \times (T_0/T_1) \times (H_0/T_0) = (H_1/H_0)(T_0/T_1) \times s_0^H$. We can substitute the definition of the hiring and retention rates in the previous equation and express the high-skill share in period 1 as : $s_1^H = (r^H + n^H) \times s_0^H \times (T_1/T_0) \approx (r^H + n^H) \times s_0^H$ where the last approximation is obtained by assuming that growth in firm-level total employment is approximately zero. We can finally express the contribution of the retention channel as: $r^H \times s_0^H$.

6.4 Labor market institutions

An other potential reason why only some employees benefit from wage gains has to do with labor market institutions. These institutions may influence how much rent sharing is taking place, and ultimately the wage incidence of tax policies on labor (Kim *et al.*, 2021). In particular, Daruich *et al.* (2020) document that fixed term contract employees benefit less from firm surplus than permanent contract workers. In a French labor market characterized by its duality, incumbents and high skill employees may be over-represented among permanent contracts workers. Their hold-up power may come from this institutional protection, rather than from firm-specific skills.

Table A20 reports estimates from Equation (2) of the effect of the policy on the wages of permanent and temporary contract employees, and of high and low skill permanent contract employees. Fixed term contract workers benefit from no wage increase, only permanent workers do. Yet, among permanent contract employees, only high skill ones get higher wages.

Next, permanent contract workers may be getting higher wages not because of their contract type, but rather because they are incumbents and have already been trained and developed firm-specific skills. In the data, there are very few temporary contracts workers among incumbents[63]. Accordingly, in order to test whether the effect is driven by contract type or incumbency status, we study the wage effects on newly hired permanent contract employees. Table A21 reports estimates of Equation (2). We detect no wage gains among newly hired permanent contract employees. It suggests that incumbency and skill, rather than contract type, shape incidence.

Overall, while we cannot completely rule out the influence of labor contracts, our results are most consistent with wage gains being primarily driven by firm-specific skills (as measured by occupation $\times$ incumbency) rather than by the employment protection that permanent contracts provide.[64]

[63] This is due to legal limitations on the cumulative length workers can spend on temporary contracts with the same employer in France—typically 18 months at most. This feature of the French legislation might explain why we find no effect on temporary workers while for Daruich *et al.* (2020) find significant albeit lower pass-through in Italy and Garin and Silvério (2019) find very similar effects across contract types in Portugal.

[64] We also explore heterogeneity along industry level proxies of employee hold up power. Patterns of retention mirror the heterogeneity of wage gains across skill groups, which suggests that employers increase wages to retain workers who are costly to replace. The idea here is to assess whether heterogeneity across sectors supports this view, by estimating whether our passthrough estimates larger in sectors where proxies signal a higher hold-up power for workers. Following Garin and Silvério (2019), we measure employee hold up power using average employee tenure and separation rate by industries. Table A22 reports estimates. Although incidence coefficients tend to be larger in higher tenure industries, it is not the case in low separation industries, and differences are not significant. A potential limit is that these measures proxy for hold-up power too coarsely.

7 Conclusion

In this paper, we show that firms play a crucial role in shaping labor market inequalities. We find that a profit windfall generated by a large corporate income tax credit translated in substantial wage gains for workers. It supports the idea that idiosyncratic shocks to firms' profits can be an important determinant wage dispersion.

We also show that profit windfall not only increase wage inequalities across firms, but also within firms. The wage pass through of the tax credit is markedly heterogeneous across workers. We find that only incumbent high-skill workers benefit from higher wages. The benefits generated by the reform do not trickle down to low wage workers or entrants. Instead, employers seem to raise the wages of workers who are the most costly to replace. We document that the policy increased the retention rate of high-skill workers, not of low-skill workers.

We find that fiscal incentives strongly affect the wage setting behavior of employers; yet irrespective of the policy targeting. We find no bunching in the wage distribution of employees near the eligibility wage cut-off and low-skill workers, who are almost all individually eligible, get no wage gains. Compared to tax cuts, tax credits allow policymakers to link a tax expenditure to a given behavior, potentially generating efficiency gains. However, employers seem to distribute the surplus generated by the tax credit as they would with other profit surpluses, regardless of the employee-level incentive embedded in the design of the tax credit.

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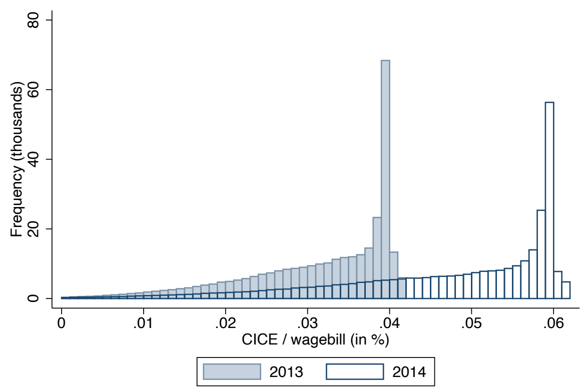
APPENDIX

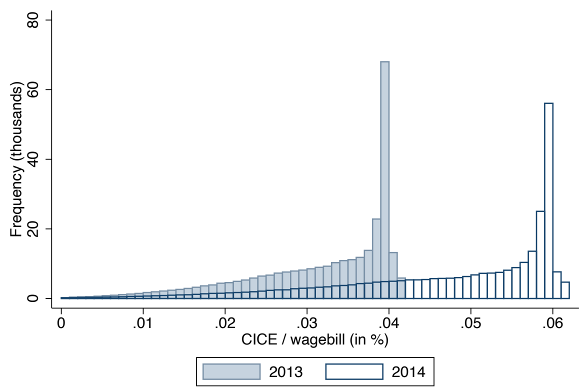
A Empirical appendix: robustness tests and additional results

A.1 Descriptive statistics

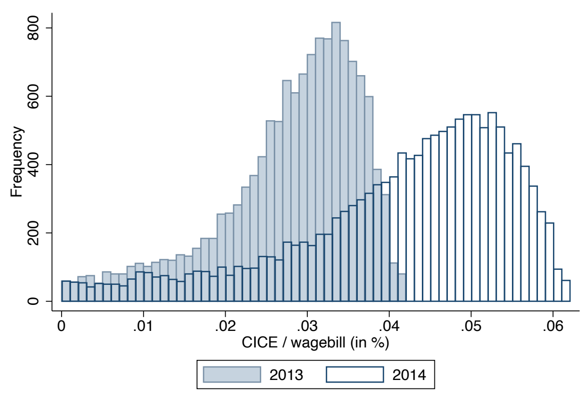
Figure A1: Distribution of firm-level treatment intensity size in 2013 and 2014



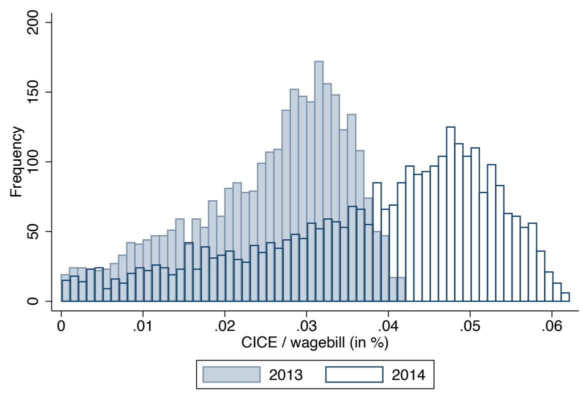
All firms



$<$ 50 employees



50-249 employees



$\geqslant$ 250 employees

NOTES: Treatment intensity is defined as the amount of tax credit with respect to the firm's wage bill.

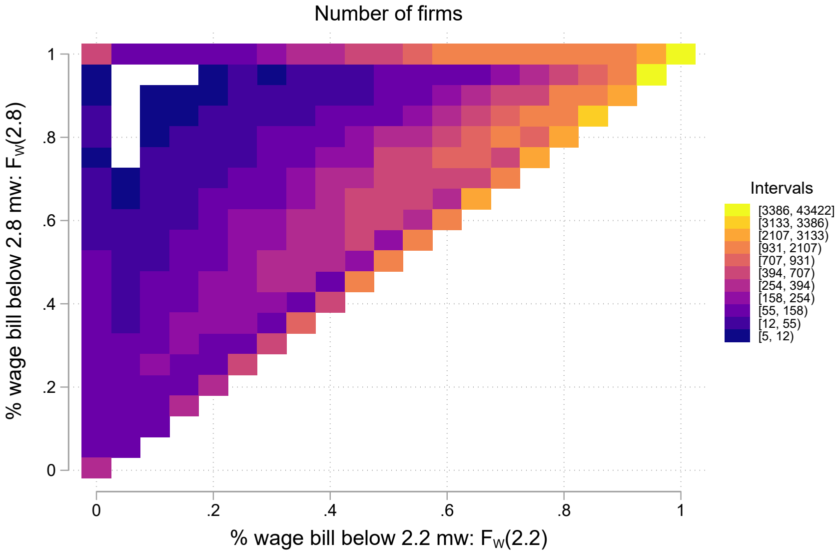
Table A1: Descriptive statistics of subsamples

	All	> 0%	> 30%	> 50%
Employment				
# of employees	40.42	73.70	20.72	8.38
# of high skill	12.15	24.64	11.38	4.23
# of low skill	27.99	48.60	9.09	3.95
Employee retention rate	0.89	0.89	0.91	0.93
High skill employee retention rate	0.90	0.90	0.92	0.94
Low skill employee retention rate	0.89	0.89	0.91	0.91
Wage earnings				
Mean yearly wage (FTE)	28790.40	32886.67	34884.10	35769.00
Mean hourly wage	15.82	18.07	19.17	19.65
Mean hourly wage, high skill	22.22	23.62	22.62	22.30
Mean hourly wage, low skill	13.60	14.73	15.70	15.99
Wage structure				
Wage bill (in k €)	868	1674	607	217
Share of <1.6MW in WB	0.55	0.37	0.24	0.18
Share of <2.5MW in WB	0.83	0.73	0.69	0.67
Firm performance				
Mean sales (in k €)	6565	12869	3923	1222
Mean EBIT (in k €)	240	470	253	65
Mean VA per worker (in k €)	60	67	76	85
Mean assets (in k €)	3428	6946	2972	482
Mean profitability (EBITDA/sales, in %)	5.46	5.24	5.77	6.13
Predicted exposure to policy				
Share of eligible wage bill	0.83	0.73	0.69	0.67
Policy surplus per hour worked (in €)	0.65	0.65	0.67	0.68
Policy surplus per worker (in €)	1182.38	1184.78	1218.13	1245.71
Industries				
Share in manufacturing	0.15	0.17	0.11	0.09
Share in construction	0.17	0.18	0.18	0.18
Share in retail	0.42	0.35	0.32	0.31
Share in services	0.18	0.23	0.33	0.37
Share in other	0.04	0.03	0.03	0.02
Observations	311284	143738	29114	7840

NOTES: This table presents descriptive statistics for the balanced estimation samples depending on restrictions regarding the 2012 share of the wage bill accruing to workers whose hourly wage lays between 2.2 and 2.8 MW. Statistics are displayed for all firms in Column (1), for firms whose payroll share of eligible workers exceeds 30% in Column (2), for firms whose payroll share of eligible workers exceeds 50% in Column (3).

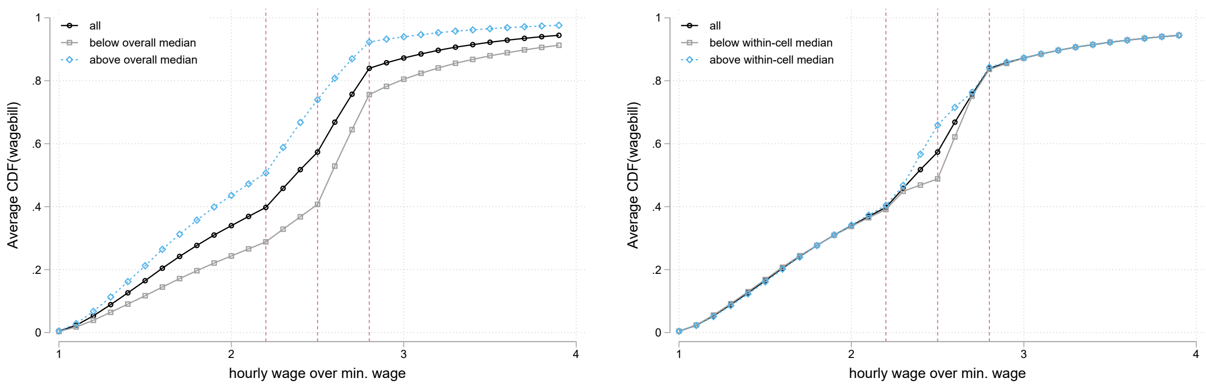
A.2 Identification Strategy

Figure A2: Number of firms per bin



NOTES: This figure reports the number of firms within each bin. The x-axis reports the share of wage bill paid to employees earning less than 2.2 MW. It is discretized in 30 categories, plus an accumulation point with all firms whose entire wage bill is paid to employees earning less than 2.2 MW. The y-axis reports the share of wage bill paid to employees earning less than 2.8 MW. Again, it is discretized in 30 categories, plus an accumulation point with all firms whose entire wage bill is paid to employees earning less than 2.8 MW. In the bin at the top right corner, all employees are paid less than 2.5 MW, all of them are eligible.

Figure A3: Distribution of wages by treatment intensity



a. Median defined globally

b. Median within bin

NOTES: This figure displays the cumulative density function of wages depending on whether they lie above or below the median of the treatment intensity. For the left panel figure, the median is defined for the entire sample. For the left panel figure, the median is now defined within each bin. The sample is here restricted to firms for which more than 30% of the wage bill accrue to workers paid between 2.2 and 2.8 MW. Red vertical lines refers to 2.2 and 2.8 MW.

Table A2: Correlation between treatment intensity and covariates

(1) Statistic	(2) Sample	(3) # firms	(4) Uncondit.	(5) Sector $\times$ Size FEs	(6) Sector $\times$ Size $\times$ Bins FEs
$\rho(Z_i, \ln Assets_i)$	all	311,284	-0.164	-0.099	-0.004
$\rho(Z_i, \ln(VA_i/L_i))$	all	311,284	-0.365	-0.289	-0.008
$\rho(Z_i, Sh1.5MW_i)$	all	311,284	0.604	0.505	0.001
$\rho(Z_i, \ln Assets_i)$	% WB $\geq$ 30%	29,114	-0.058	-0.025	-0.009
$\rho(Z_i, \ln(VA_i/L_i))$	% WB $\geq$ 30%	29,114	-0.191	-0.187	-0.028
$\rho(Z_i, Sh1.5MW_i)$	% WB $\geq$ 30%	29,114	0.355	0.317	0.012
$\rho(Z_i, \ln Assets_i)$	% WB $\geq$ 50%	7,840	-0.018	-0.034	-0.028
$\rho(Z_i, \ln(VA_i/L_i))$	% WB $\geq$ 50%	7,840	-0.085	-0.104	-0.041
$\rho(Z_i, Sh1.5MW_i)$	% WB $\geq$ 50%	7,840	0.178	0.225	0.018

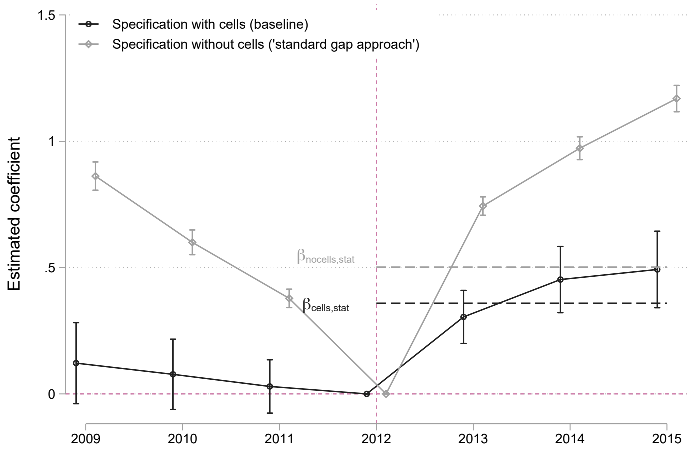
NOTES: A bin is defined as a unique value of the proportion of wage bill accruing to workers making less than 2.2 and less than 2.8 MW (both variables are discretized through truncation into 31 values). $\rho(Z_i, X_i)$ is the coefficient of correlation between the instrument for treatment intensity Z_i and the firm's characteristics X_i in 2012. "% WB $> x$" refers to a sample restriction to firms whose share of the wage bill constituted of wages between 2.2 and 2.8MW is above x. Column (4) show unconditional correlations. Column (5) shows the correlation between the two variables after absorbing 3-digit sector $\times$ size FEs. Column (6) shows the correlation between the same variables after absorbing sector $\times$ size $\times$ bins FEs.

A.3 Main results: supplementary material and robustness

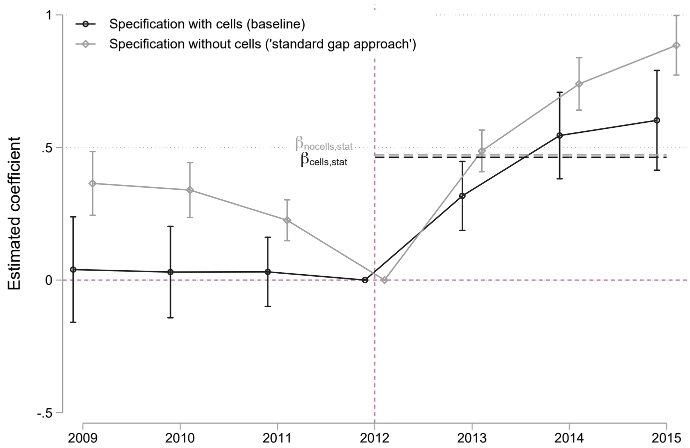
A.3.1 Rationale for including cell-year fixed effects

Figure A4: Estimates comparison with and without cell-year fixed effects

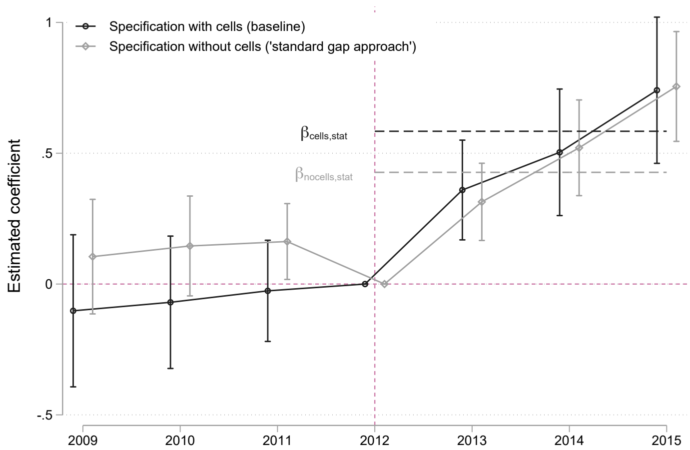
(a) Sample 0% or more



(b) Sample 30% or more



(c) Sample 50% or more

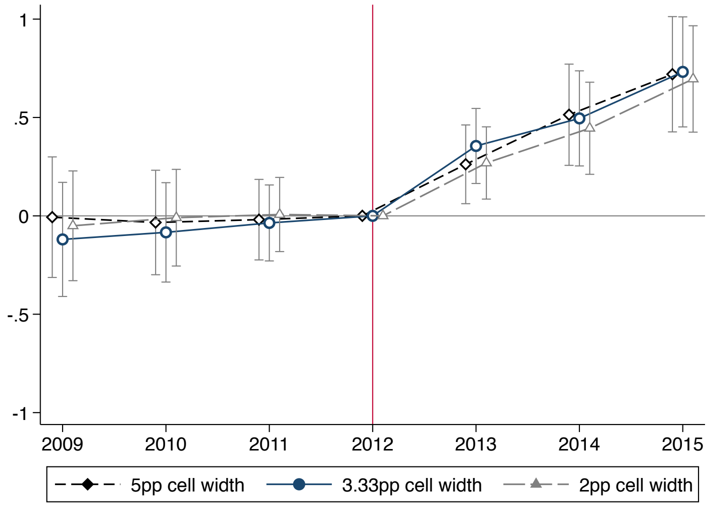


NOTES: The figure plots the point estimates form Equation (3) with several samples, depending of the share of firm wage bill paid to workers earning between 2.2 and 2.8 MW. Black coefficients correspond to the baseline specification, which includes cells-year fixed effects. Grey coefficients correspond to the same specification, without the cell-year fixed effects. We also refer to this latter specification as the standard gap approach. Vertical bar correspond to 95% confidence intervals. The reference year is 2012, the last pre-reform year, and the point estimate is equal to 0 by construction. The dashed black and grey horizontal lines plot the difference-in-differences estimates—see equation (2). Cells are described in Section 4.2.

A.3.2 Robustness to changes in cell size

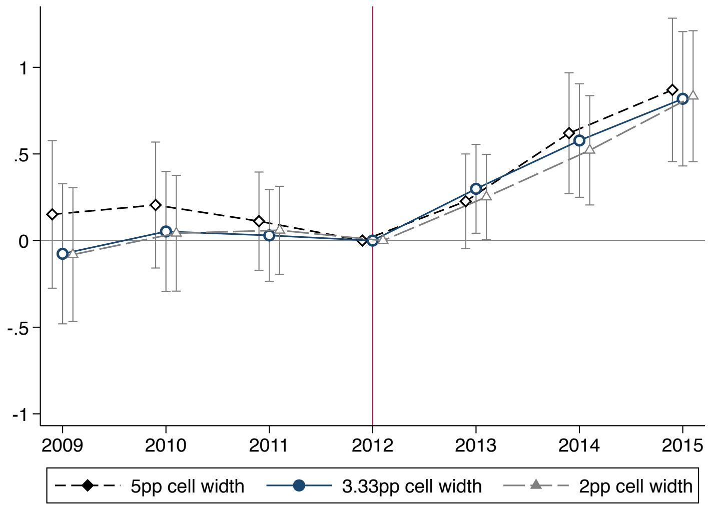
Figure A5: Impact on wages (log), robustness to alternative definitions of cells

(a) Varying cell width for (2.2,2.8) window



NOTES: This figure reports point estimates and 95-percent confidence intervals from the event study regression of Equation 3 with alternative cell widths for a (2.2,2.8) window. More on cells in Section 4.2. Other notes as in Figure 5.

(b) Varying cell width for (2.3,2.7) window



NOTES: This figure reports point estimates and 95-percent confidence intervals from the event study regression of Equation 3 with alternative cell widths for a (2.3,2.7) window. More on cells in Section 4.2. Other notes as in Figure 5.

Table A3: Robustness to alternative cell size

(a) Larger grid (5pp), regular window (2.2,2.8)

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.289***	0.289***	0.402***	0.404***	0.480***	0.483***
	(0.0609)	(0.0609)	(0.0763)	(0.0763)	(0.113)	(0.113)
Observations	898912	898912	173201	173201	46102	46102
R^2	0.933	0.933	0.863	0.863	0.773	0.773
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.05	.05	.05	.05	.05	.05
Controls	No	Yes	No	Yes	No	Yes
# firms	128416	128416	24743	24743	6586	6586

(b) Finer grid (2pp), regular window (2.2,2.8)

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.326***	0.326***	0.398***	0.402***	0.511***	0.513***
	(0.0690)	(0.0690)	(0.0834)	(0.0835)	(0.124)	(0.124)
Observations	709492	709492	139279	139279	38808	38808
R^2	0.936	0.936	0.857	0.858	0.765	0.766
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.02	.02	.02	.02	.02	.02
Controls	No	Yes	No	Yes	No	Yes
# firms	101356	101356	19897	19897	5544	5544

NOTES: This table reports estimates of Equation 2. The dependent variable is mean hourly wages, at the firm level. Compared to the baseline specification, the steps used to discretize wage bill shares below 2.2 MW and 2.8 MW is larger (5pp) or smaller (2pp) in panels (a) and (b) respectively. Other notes as in 2.

Table A4: Robustness to alternative cell size, continued

(a) Larger grid (5pp), smaller window (2.3,2.7)

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.273***	0.273***	0.356***	0.358***	0.523***	0.530***
	(0.0756)	(0.0756)	(0.100)	(0.100)	(0.158)	(0.158)
Observations	717752	717752	90832	90832	22526	22526
R^2	0.937	0.937	0.851	0.851	0.759	0.760
Window defining cells	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.05	.05	.05	.05	.05	.05
Controls	No	Yes	No	Yes	No	Yes
# firms	102536	102536	12976	12976	3218	3218

(b) Regular grid (3.3pp), smaller window (2.3,2.7)

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.282***	0.282***	0.337***	0.339***	0.556***	0.563***
	(0.0794)	(0.0794)	(0.103)	(0.103)	(0.162)	(0.162)
Observations	658819	658819	83636	83636	20993	20993
R^2	0.939	0.940	0.851	0.851	0.762	0.763
Window defining cells	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	94117	94117	11948	11948	2999	2999

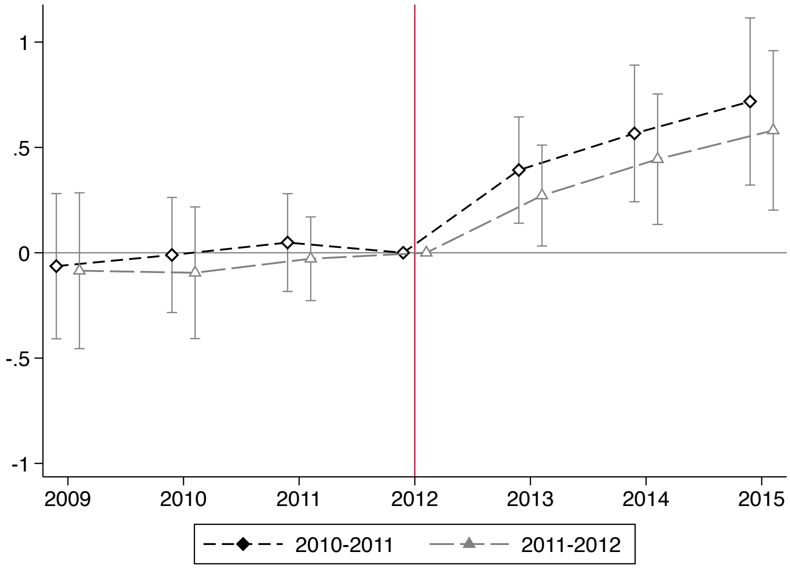
(c) Finer grid (2pp), smaller window (2.3,2.7)

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.279***	0.279***	0.286***	0.288***	0.448***	0.455***
	(0.0865)	(0.0865)	(0.109)	(0.109)	(0.171)	(0.171)
Observations	556458	556458	73297	73297	18970	18970
R^2	0.941	0.942	0.845	0.846	0.768	0.769
Window defining cells	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.02	.02	.02	.02	.02	.02
Controls	No	Yes	No	Yes	No	Yes
# firms	79494	79494	10471	10471	2710	2710

NOTES: This table reports estimates of Equation 2. The dependent variable is mean hourly wages, at the firm level. Compared to the baseline specification, the steps used to discretize wage bill shares below 2.3 MW and 2.7 MW is larger (5pp), similar (3.3pp) or smaller (2pp) in panels (a), (b) and (c) respectively. Other notes as in 2.

A.3.3 Treatment intensity based on other years

Figure A6: Impact on wages (log), alternative definitions of the instrument



NOTES: This figure reports robustness of point estimates and 95-percent confidence intervals from the event study regression defined in Equation 3 with different years (2010 and 2011, 2011, or 2011 and 2012 as opposed to 2012 in the baseline analysis) used to build the instrument and cells. Other notes as in Figure 5.

Table A5: Robustness for treatment intensity based on other years

(a) 3.3pp grid

	Mean hourly wage (log)					
	2010-2011			2011-2012		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.151**	0.371***	0.565***	0.198***	0.318***	0.484***
	(0.0729)	(0.0977)	(0.148)	(0.0748)	(0.0982)	(0.148)
Observations	1029140	136304	31577	1020033	136906	30835
R^2	0.937	0.867	0.779	0.934	0.863	0.776
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	147020	19472	4511	145719	19558	4405

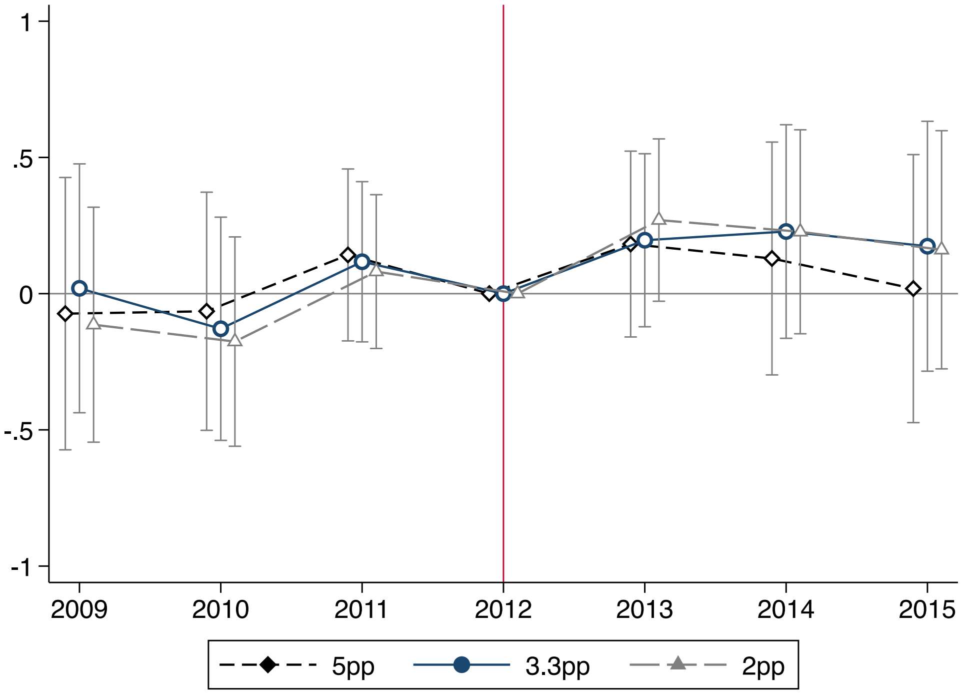
(b) 5pp grid

	Mean hourly wage (log)					
	2010-2011			2011-2012		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.163**	0.330***	0.505***	0.214***	0.352***	0.538***
	(0.0686)	(0.0920)	(0.141)	(0.0704)	(0.0932)	(0.141)
Observations	1103872	150913	34608	1094975	151298	34265
R^2	0.934	0.864	0.775	0.932	0.859	0.773
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.05	.05	.05	.05	.05	.05
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	157696	21559	4944	156425	21614	4895

NOTES: This table reports the estimates of Equation 2. the dependent variable is the mean hourly wage of high skill employees. Treatment intensity is defined using firm wage distribution in years other than 2012. Other notes as in Table 2.

A.3.4 Placebo tests

Figure A7: Placebo policy threshold at 3.1 MW



NOTES: This figure reports placebo test of the effects of the tax credit on mean hourly wages, in log. We estimate the effect of the policy with a placebo eligibility cutoff at 3.1 MW instead of 2.5 MW. We define cells accordingly, using 2.9 and 3.3 MW thresholds. No policy features an eligibility threshold at 3.1, we therefore expect no effect. The sample includes firms with at least 50% of their wage bill paid to workers earning between 2.9 and 3.3 MW. Other notes as in Figure 5.

A.3.5 External validity

Table A6: Re-weighting the estimation samples to match the overall distribution of firms

	All (1)	$> 0\%$ (2)	$> 30\%$ (3)	$> 50\%$ (4)
Variables used in matching				
Employment				
# of employees	40.42	40.53	40.43	40.46
Industries				
Share in manufacturing	0.15	0.15	0.15	0.15
Share in construction	0.17	0.18	0.18	0.18
Share in retail	0.42	0.43	0.43	0.43
Share in services	0.18	0.19	0.19	0.19
Share in other	0.04	0.04	0.04	0.04
Other variables				
Profitability (EBITDA/sales, in %)	5.46	5.13	5.50	5.86
Mean annual wage (FTE)	28790	31304	33153	34878
Share of eligible wage bill	0.83	0.77	0.73	0.70
Policy surplus per worker (in €)	1182	1202	1244	1262
Observations	311284	138059	28322	7613

NOTES: This table presents descriptive statistics for the reweighted estimation samples depending on restrictions regarding the 2012 share of the wage bill accruing to workers whose hourly wage lays between 2.2 and 2.8 MW. Statistics are displayed for all firms in Column (1), for firms whose payroll share in the estimation window is strictly positive in Column (2), exceeds 30% in Column (3), exceeds 50% in Column (4). The weights are obtained using Hainmueller (2012)'s entropy balancing. Entropy balancing relies produces a set of unit weights so that the re-weighted sample satistics a large set of pre-specified balance conditions based on known sample moments. Here, the first moments have been selected for binary variables for industies and two continuous variables (hourly wage and overall employment).

A.3.6 Robustness to county-year fixed effects

Table A7: Robustness to including county-year fixed effect

(a) County-year fixed effects, all firms

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.356***	0.356***	0.473***	0.475***	0.559***	0.562***
	(0.0638)	(0.0638)	(0.0791)	(0.0792)	(0.118)	(0.118)
Observations	830144	830144	159551	159551	42854	42854
R^2	0.936	0.936	0.864	0.865	0.778	0.778
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	118592	118592	22793	22793	6122	6122

(b) County-year fixed effects, only firms operating in a single county

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.366***	0.366***	0.478***	0.481***	0.585***	0.589***
	(0.0650)	(0.0650)	(0.0799)	(0.0800)	(0.119)	(0.119)
Observations	729484	729484	151886	151886	41930	41930
R^2	0.933	0.933	0.862	0.862	0.777	0.778
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	104212	104212	21698	21698	5990	5990

NOTES: This table plots estimates from Equation 2. The dependent variable is the mean hourly wage in log. Compared to the baseline specification, we added county (*département* in French) $\times$ year fixed effects. For Panel A7a, the sample includes all firms for which we have data on the location of their headquarters, the specification includes HQ county $\times$ year fixed effects, independently of whether they have establishments in several counties. Panel A7b reports estimates of the same specification when restricting the estimating sample to the the subset of firms with establishments in a single county?i.e. single county firms. Other note as in Table 2.

A.3.7 Robustness to sample trimming

Table A8: Robustness to sample trimming

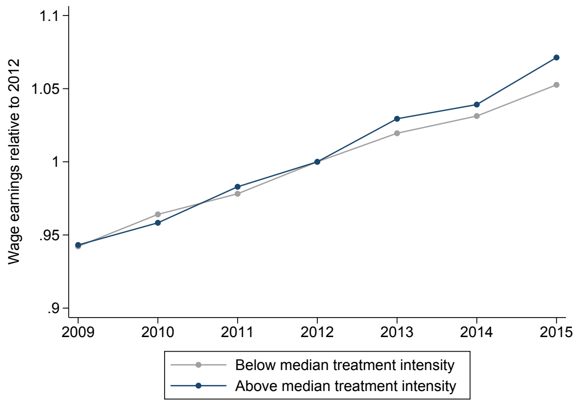
	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.264***	0.264***	0.338***	0.340***	0.444***	0.446***
	(0.0675)	(0.0675)	(0.0820)	(0.0821)	(0.120)	(0.120)
Observations	952742	952742	193921	193921	55496	55496
R^2	0.933	0.933	0.836	0.836	0.729	0.729
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	136106	136106	27703	27703	7928	7928

NOTES: This table reports the estimates of Equation 2. the dependent variable is the mean hourly wage of employees. The sample include all firms, including outliers that were dropped. Other notes as in Table 2.

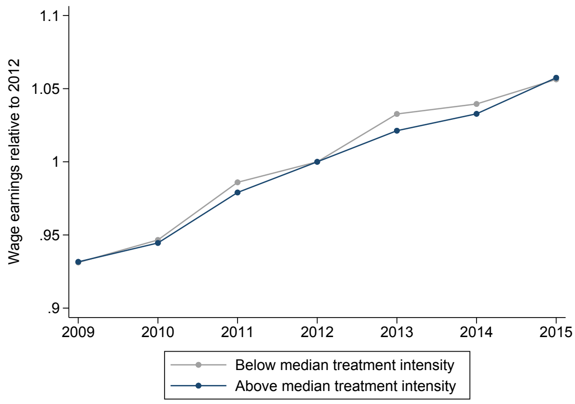
A.4 Heterogeneity: supplementary material and robustness

A.4.1 Heterogeneity by skill level

Figure A8: Firm-level impact on gross hourly wages, by skill group



(a) High-skill workers



(b) Low-skill workers

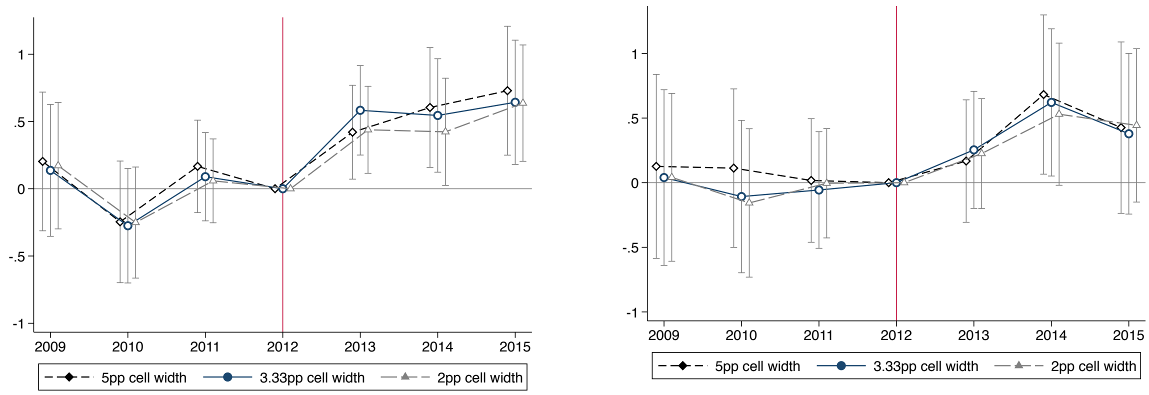
NOTES: This figure plots the mean gross hourly wage of firms (relative to 2012) across years for high-skill and low-skill workers, depending on firms' treatment intensity. Other notes as in Figure 4.

A.4.2 Robustness to changes in cell size

Figure A9: Impact on wages of high skill employees (log), robustness to alternative definitions of bins

(a) Varying cell width for (2.2,2.8) window

(b) Varying cell width for (2.3,2.7) window

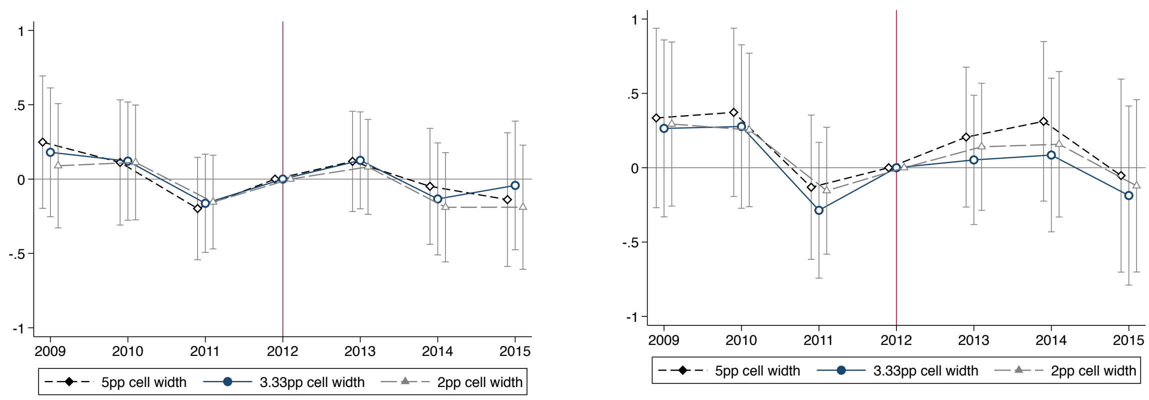


NOTES: This figure reports point estimates and 95-percent confidence intervals from the event study regression of Equation 3 with alternative cell widths for a (2.2,2.8) window in Panel a and (2.3,2.7) window in Panel b. More on cells in Section 4.2. Other notes as in Figure 5.

Figure A10: Impact on wages of low skill employees (log), robustness to alternative definitions of bins

(a) Varying cell width for (2.2,2.8) window

(b) Varying cell width for (2.3,2.7) window



NOTES: This figure reports point estimates and 95-percent confidence intervals from the event study regression of Equation 3 with alternative cell widths for a (2.2,2.8) window in Panel a and (2.3,2.7) window in Panel b. More on cells in Section 4.2. Other notes as in Figure 5.

Table A9: Robustness to alternative cell size

(a) Larger grid (5pp), regular window (2.2,2.8)

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.292***	0.385***	0.504***	-0.0250	-0.00730	-0.108
	(0.106)	(0.125)	(0.172)	(0.0732)	(0.0962)	(0.155)
Observations	758482	133962	33428	860632	153998	37932
R^2	0.804	0.756	0.697	0.844	0.816	0.810
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.05	.05	.05	.05	.05	.05
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	116341	21140	5359	125851	23315	5924

(b) Finer grid (2pp), regular window (2.2,2.8)

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.430***	0.464***	0.552***	-0.0556	-0.0512	-0.0601
	(0.124)	(0.141)	(0.191)	(0.0815)	(0.103)	(0.166)
Observations	575523	102716	27141	677088	123128	31968
R^2	0.819	0.756	0.705	0.865	0.828	0.818
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.02	.02	.02	.02	.02	.02
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	89572	16435	4388	99086	18650	4972

NOTES: This table reports estimates of Equation 2. The dependent variable is mean hourly wages, at the firm level, by skill group. Compared to the baseline specification, the steps used to discretize wage bill shares below 2.2 MW and 2.8 MW is larger (5pp) or smaller (2pp) in panels (a) and (b) respectively. Other notes as in 6.

Table A10: Robustness to alternative cell size, continued

(a) Larger grid (5pp), smaller window (2.3,2.7)

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.367***	0.390**	0.428*	-0.0457	-0.0709	-0.0346
	(0.130)	(0.163)	(0.238)	(0.0901)	(0.123)	(0.209)
Observations	621462	67838	15686	687448	79273	18234
R^2	0.811	0.747	0.700	0.845	0.820	0.820
Window defining cells	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.05	.05	.05	.05	.05	.05
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	94388	10848	2537	100513	12072	2864

(b) Regular grid (3.3pp), smaller window (2.3,2.7)

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.396***	0.402**	0.449*	-0.124	-0.153	-0.0753
	(0.140)	(0.170)	(0.249)	(0.0939)	(0.126)	(0.215)
Observations	564272	61221	14416	629912	72749	16977
R^2	0.819	0.753	0.709	0.856	0.823	0.824
Window defining cells	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	86024	9822	2330	92150	11088	2670

(c) Finer grid (2pp), smaller window (2.3,2.7)

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.503***	0.396**	0.363	-0.107	-0.185	0.0155
	(0.155)	(0.183)	(0.265)	(0.103)	(0.134)	(0.233)
Observations	466058	52300	12714	531207	64084	15446
R^2	0.829	0.749	0.720	0.869	0.829	0.833
Window defining cells	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.02	.02	.02	.02	.02	.02
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	71664	8478	2079	77718	9723	2412

NOTES: This table reports estimates of Equation 2. The dependent variable is mean hourly wages, at the firm level, by skill group. Compared to the baseline specification, the steps used to discretize wage bill shares below 2.3 MW and 2.7 MW is larger (5pp), similar (3.3pp) or smaller (2pp) in panels (a), (b) and (c) respectively. Other notes as in 6.

A.4.3 Robustness to alternative reference years

Table A11: Robustness for treatment intensity based on other years (3.33pp width)

(a) High skill employees

	Mean hourly wage of high skill workers (log)					
	2010-2011			2011-2012		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.653***	0.702***	0.747***	0.343***	0.480***	0.890***
	(0.131)	(0.159)	(0.223)	(0.132)	(0.157)	(0.223)
Observations	823727	103395	22195	817571	104854	21752
R^2	0.812	0.784	0.734	0.810	0.777	0.707
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	128865	16396	3582	127738	16568	3483

(b) Low skill employees

	Mean hourly wage of low skill workers (log)					
	2010-2011			2011-2012		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	-0.251***	0.000705	0.214	-0.0821	-0.0233	-0.0830
	(0.0901)	(0.129)	(0.217)	(0.0910)	(0.130)	(0.220)
Observations	986698	119634	25507	977385	119714	24542
R^2	0.853	0.823	0.818	0.852	0.827	0.824
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	144209	18216	3994	142865	18258	3864

NOTES: This table reports the estimates of Equation 2. The dependent variable is the mean hourly wage of employees, by skill group. Treatment intensity is defined using firm wage distribution in years other than 2012. Other notes as in Table 6.

Table A12: Robustness for treatment intensity based on other years (5pp width)

(a) High skill employees

	Mean hourly wage of high skill workers (log) 2010-2011			2011-2012		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.623***	0.606***	0.622***	0.329***	0.466***	0.869***
	(0.122)	(0.149)	(0.214)	(0.122)	(0.147)	(0.209)
Observations	896460	117120	24828	890278	118075	24484
R^2	0.802	0.773	0.722	0.800	0.766	0.703
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.05	.05	.05	.05	.05	.05
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	139471	18428	3982	138338	18533	3909

(b) Low skill employees

	Mean hourly wage of low skill workers (log) 2010-2011			2011-2012		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	-0.268***	-0.0372	0.183	-0.135	-0.0438	-0.0751
	(0.0850)	(0.122)	(0.203)	(0.0861)	(0.124)	(0.210)
Observations	1060104	133308	28035	1050562	132850	27341
R^2	0.843	0.812	0.811	0.842	0.818	0.819
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.05	.05	.05	.05	.05	.05
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	154841	20257	4385	153471	20222	4288

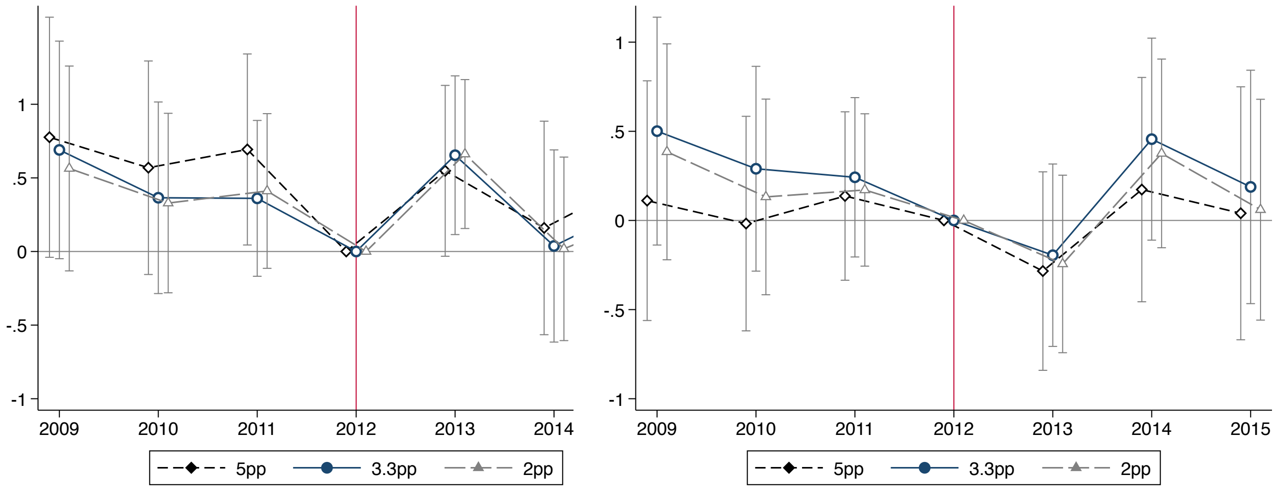
NOTES: This table reports the estimates of Equation 2. The dependent variable is the mean hourly wage of employees, by skill group. Treatment intensity is defined using firm wage distribution in years other than 2012. Cells are defined by using larger steps to discretize wage bill share at 2.2 MW and 2.8 MW with respect to the baseline specification. Other notes as in Table 6.

A.4.4 Placebo tests

Figure A11: Placebo policy threshold at 3.1 MW

(a) Mean hourly wage of high skill employees (log)

(b) Mean hourly wage of low skill employees (log)



NOTES: This figure reports placebo test of the effects of the tax credit on mean hourly wages, in log. We estimate the effect of the policy with a placebo eligibility cutoff at 3.1 MW instead of 2.5 MW. We define cells accordingly, using 2.9 and 3.3 MW thresholds. No policy features an eligibility threshold at 3.1, we therefore expect no effect. The sample includes firms with at least 50% of their wage bill paid to workers earning between 2.9 and 3.3 MW. Other notes as in Figure 5.

A.4.5 Robustness to county-year fixed effects

Table A13: Robustness to including region-year fixed effect

(a) Region-year fixed effects, all firms

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.380***	0.486***	0.620***	-0.0426	-0.00607	-0.0766
	(0.113)	(0.131)	(0.184)	(0.0762)	(0.0991)	(0.161)
Observations	691992	121281	30555	793648	141315	35245
R^2	0.812	0.763	0.709	0.853	0.825	0.818
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	106597	19228	4910	116137	21437	5513

(b) Region-year fixed effects, only firms operating in a single county

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.399***	0.481***	0.611***	-0.0315	-0.00606	-0.0365
	(0.115)	(0.133)	(0.185)	(0.0777)	(0.0999)	(0.162)
Observations	595899	114201	29699	694873	134355	34501
R^2	0.809	0.761	0.709	0.853	0.826	0.819
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	92531	18179	4784	101846	20382	5389

NOTES: This table plots estimates from Equation 2. The dependent variable is the mean hourly wage in log. Compared to the baseline specification, we added county (*département* in French) × year fixed effects. For Panel A13a, the sample includes all firms for which we have data on the location of their headquarters, the specification includes HQ county × year fixed effects, independently of whether they have establishments in several counties. Panel A13b reports estimates of the same specification when restricting the estimating sample to the the subset of firms with establishments in a single county, i.e. single county firms. Other note as in Table 6.

A.4.6 Robustness to sample trimming

Table A14: Robustness to sample trimming

	Mean hourly wage (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.399***	0.481***	0.611***	-0.0315	-0.00606	-0.0365
	(0.115)	(0.133)	(0.185)	(0.0777)	(0.0999)	(0.162)
Observations	595899	114201	29699	694873	134355	34501
R^2	0.809	0.761	0.709	0.853	0.826	0.819
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	92531	18179	4784	101846	20382	5389

NOTES: This table plots estimates from Equation 2. The dependent variable is the mean hourly wage in log, by skill group. The sample includes firms with extreme values of wages. Other notes as in 6.

A.4.7 Heterogeneity by gender

Table A15: Impact on hourly wages (log), by employee gender

	Mean hourly wage (log)					
	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.382***	0.499***	0.522***	0.216**	0.309***	0.334*
	(0.0937)	(0.118)	(0.171)	(0.0945)	(0.116)	(0.171)
Observations	798330	143233	36683	767146	133580	33098
R^2	0.910	0.831	0.773	0.876	0.845	0.842
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	116048	21331	5510	112655	20078	5055

NOTES: This table reports the estimated impact of the CICE tax credit on wages of workers by gender. Other notes as in Table 6.

A.4.8 Heterogeneity among incumbents

Table A16: Heterogeneity among incumbents

(a) Across skill levels

	Mean hourly wage of incumbents (log) High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.385***	0.483***	0.649***	-0.0414	-0.00848	0.0191
	(0.114)	(0.133)	(0.176)	(0.0810)	(0.106)	(0.172)
Observations	630474	108371	27276	734772	123946	29975
R^2	0.830	0.784	0.719	0.862	0.842	0.835
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	103147	18565	4743	113519	20315	5079

(b) Across genders

	Mean hourly wage of incumbents (log) Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.274***	0.377***	0.468**	0.439***	0.499***	0.604***
	(0.101)	(0.124)	(0.185)	(0.0954)	(0.119)	(0.167)
Observations	699296	117089	28501	737990	126696	32191
R^2	0.879	0.853	0.850	0.906	0.840	0.777
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	109364	19029	4705	112899	20126	5165

NOTES: This table reports the estimated impact of the CICE on the mean hourly wage of incumbent workers by skill groups and gender. Other notes as in Table 2.

A.5 Employee-level analysis

A.5.1 Distribution of employees' wage

Estimation. We formally test for the presence of bunching using methods developed by Saez (2010); Chetty *et al.* (2011) and Kleven and Waseem (2013). To detect the excess or missing masses in the distribution of wages around the eligibility threshold, we first estimate a counterfactual wage distribution. The counterfactual distribution is fitted using a seventh degree polynomial excluding observations in a range $[w_L, w^N]$ while taking into account that any excess mass in the excluding range to the left of the notch has to be compensated for to the right of the notch. We test the robustness of our results to several exclusion ranges. Excess bunching is estimated as the difference between the counterfactual and observed distributions in the excluding range.

More formally, we estimate the sum of total extra bunching $\hat{B}_N$ in the range $[w_L, w^N]$ as follows:

$$\hat{B}_N = \sum_{i=w_L}^{w^N} C_i - \hat{C}_i \tag{6}$$

where C_i is the number of observations in wage bin i and $\hat{C}_i$ is the counterfactual number of observations in the same bin. Standard errors are calculated using a bootstrap procedure.

We then estimate $\hat{b}$, the rate of extra bunching in the range $[w_L, w^N]$.

$$\hat{b} = \frac{\hat{B}_N}{\frac{1}{w^N - w_L + 1} \sum_{i=w_L}^{w^N} \hat{C}_i} \tag{7}$$

A significantly positive $\hat{b}$ would indicate an excess mass of employees paid just below 2.5 MW.

Results. Table A17 reports the estimated rate of excess bunching in employees' wage distribution for different estimation ranges. Estimates are all close to zero and not statistically significant, both in the pre- and post-reform years.

A.5.2 Distribution of continuing employees' wage growth

We formally test for a potential deformation of the distribution around the 2.5 MW threshold for years after the reform. As for new hires, we test for a local deformation in the relationship between wage growth and initial wage of incumbents.[65] We model employee wage growth g_i as a n^{th} order polynomial of initial wage $w_{0,i}$. We fit this polynomial excluding a range of observations around the cut-off $[w_L, w_U]$ and get a

[65]Here, however, we do not need to impose that the distribution deformation below the threshold is compensated for above it since we are analyzing a conditional expected value, not a distribution.

Table A17: Bunching estimation

$[\mathbf{w_L}, \mathbf{w^N}]$	All		New Hires	
	Pre-reform	Post-reform	Pre-reform	Post-reform
$[2.45, 2.50]$	-0.037	-0.0075	0.0088	0.0167
	(0.0191)	(0.0195)	(0.0810)	(0.1473)
$[2.40, 2.50]$	-0.0194	-0.0029	-0.0895	-0.0358
	(0.0254)	(0.0287)	(0.1174)	(0.2146)
$[2.35, 2.50]$	-0.0053	-0.0047	-0.1622	-0.2454
	(0.0327)	(0.0342)	(0.1485)	(0.2637)
$[2.30, 2.50]$	-0.0193	-0.0127	-0.2205	-0.4631
	(0.0385)	(0.0406)	(0.1741)	(0.3055)

Notes: New hires are defined as workers starting a new contract in a new firm during the year. The sample only includes employees working in firms that claimed the tax credit and that are in our firm-level estimation sample. *: p<0.05; **: p<0.01; ***: p<0.001.

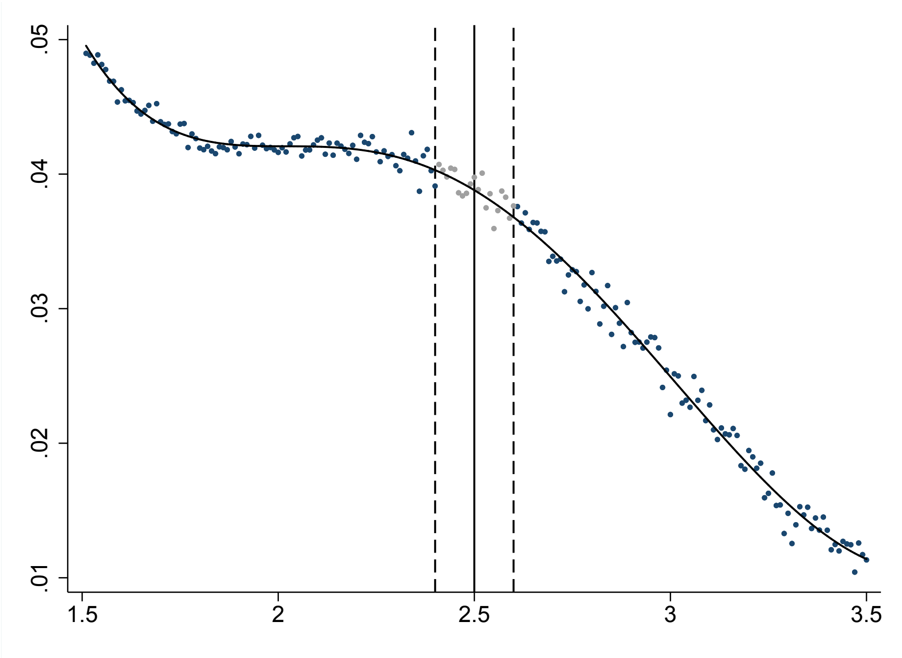
vector of polynomial coefficients denoted β. We then predict the average wage growth within the exclusion range using the mean wage in the range. We note $\bar{X}$ the vector of mean wage in the range to the power $p = 0, ..., n$.$\bar{X} = [1, \overline{w_0^1}, \overline{w_0^2}, ..., \overline{w_0^n}]$, where $\overline{w_0^p}$. We compare the observed mean wage growth $\bar{g}$ with the mean counterfactual wage growth $\hat{g} = \bar{X}'\hat{\beta}^n$ in the exclusion range. We test the significance of $\hat{G} = \bar{g} - \hat{g}$ under the assumption that the two estimators are independent.

Table A18: Growth distortion estimation

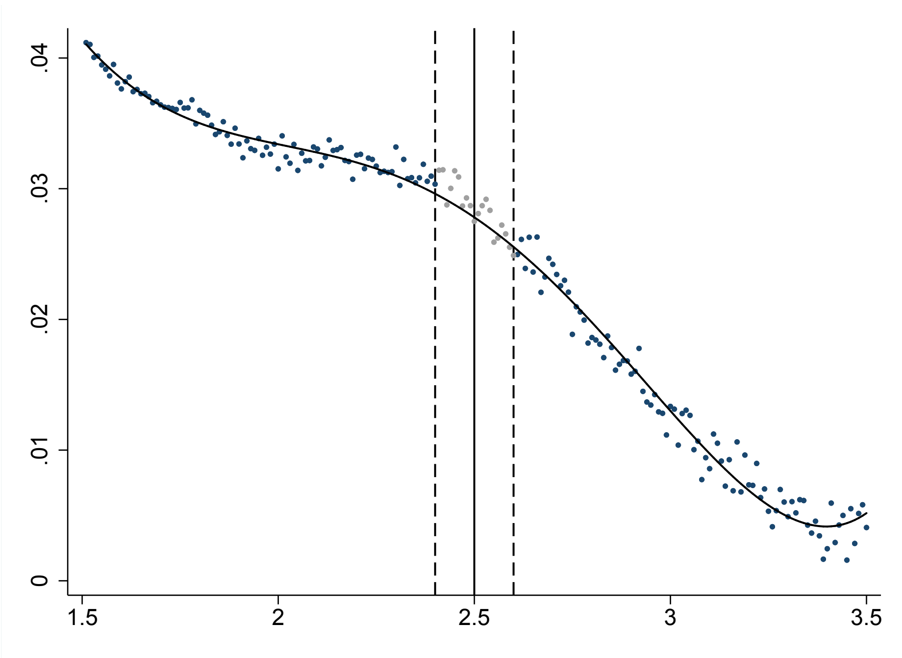
Excluded range	Pre-reform	Post-reform
$[2.45, 2.55]$	-0.00025	0.00069
	(0.00075)	(0.00082)
$[2.40, 2.60]$	0.00015	0.00084
	(0.00079)	(0.00086)
$[2.35, 2.65]$	0.00017	0.00097
	(0.00087)	(0.00095)
$[2.30, 2.70]$	0.00034	0.0011
	(0.00097)	(0.0011)

Notes: Coefficients are the difference between the mean observed and counterfactual wage growth in the exclusion range. The sample only includes employees working in firms that claimed the tax credit and that are in our firm-level estimation sample. Wage growth is computed only for continuing workers with a permanent contract. Standard errors in parentheses. *: p<0.05; **: p<0.01; ***: p<0.001.

Figure A12: Distribution of wage growth of incumbents



(a) Pre-reform



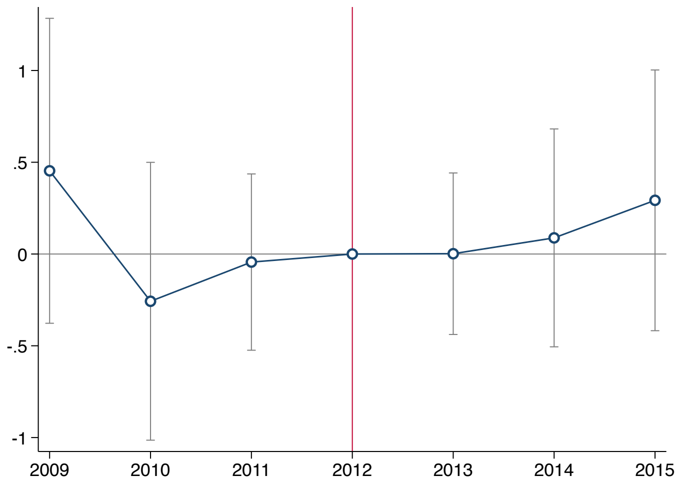
(b) Post-reform

Notes: This figure depicts the relationship between the year-on-year wage growth rate and past year's wage for two time periods, before and after the reform. Wages are expressed in multiples of the minimum wage. Dashed vertical lines illustrate the excluding range (w_L, w_U), which here corresponds to (2.4 MW, 2.6 MW).

A.6 Mechanisms: supplementary material and robustness

A.6.1 Impact on Sales

Figure A13: Impact on sales (in log)



NOTES: This figure reports the estimated impact of the CICE tax credit on wages of high-skill workers. Other notes as in Figure 5.

Table A19: Impact on sales (in log)

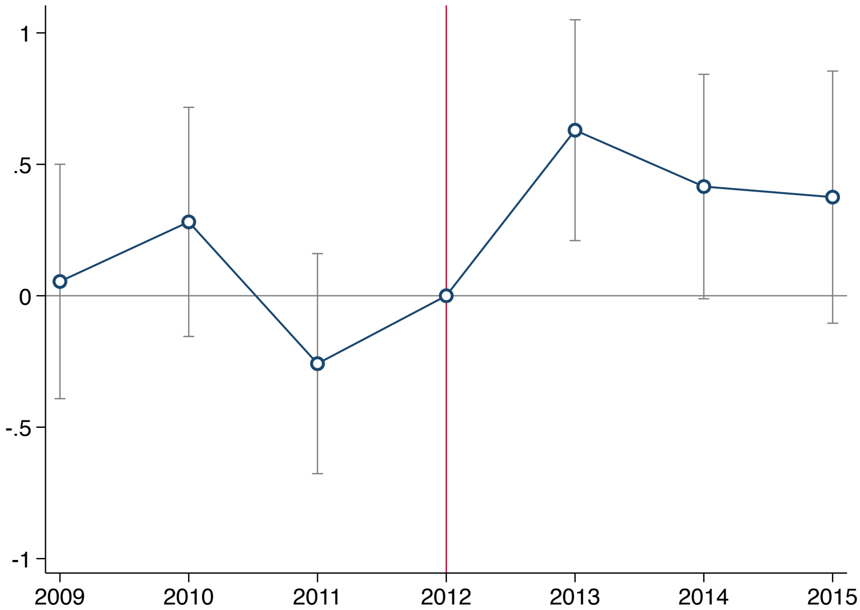
	Sales (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	-0.156	-0.156	0.000477	0.00347	0.0877	0.0915
	(0.182)	(0.182)	(0.214)	(0.214)	(0.298)	(0.298)
Observations	826559	826559	158727	158727	42654	42654
R^2	0.973	0.973	0.949	0.949	0.927	0.927
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	118569	118569	22787	22787	6122	6122

Notes: This table reports the coefficients from Equation 2 measuring impact of the CICE tax credit on sales, in log. Other notes as in Table 2. *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

A.6.2 Retention Rate

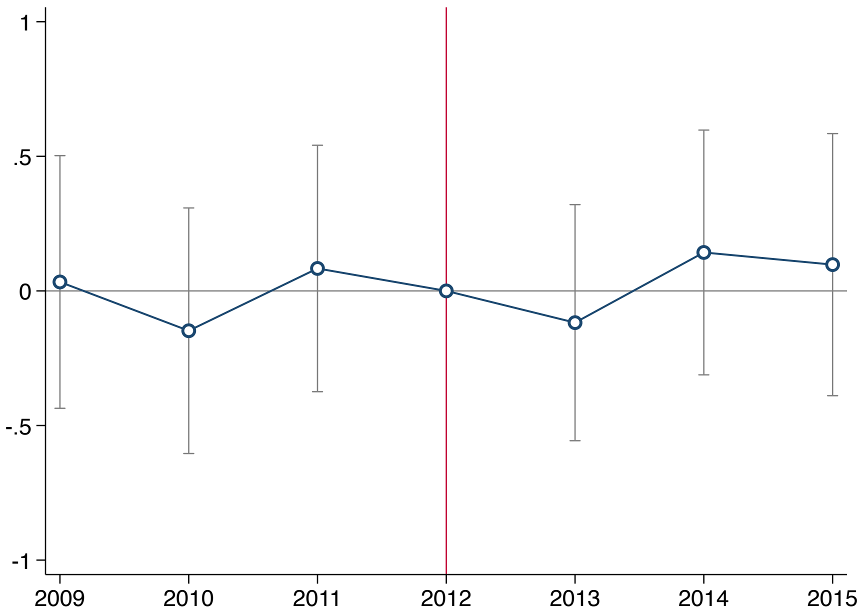
Figure A14: Impact on employee retention rate by skill group (30% sample)

(a) High-skill workers



NOTES: This figure reports the estimated impact of the CICE tax credit on wages of high-skill workers. Other notes as in Figure 5.

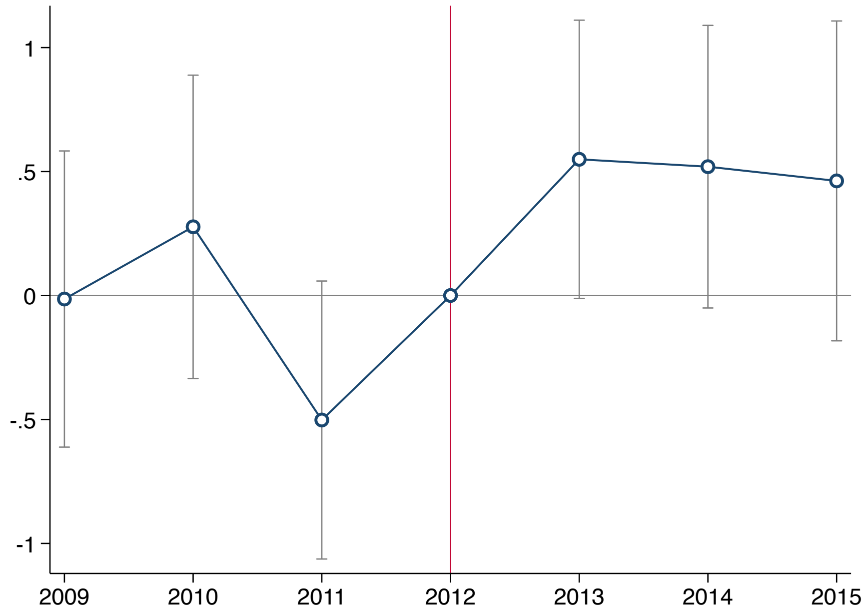
(b) Low-skill workers



NOTES: This figure reports the estimated impact of the CICE tax credit on wages of low-skill workers. Other notes as in Figure 5.

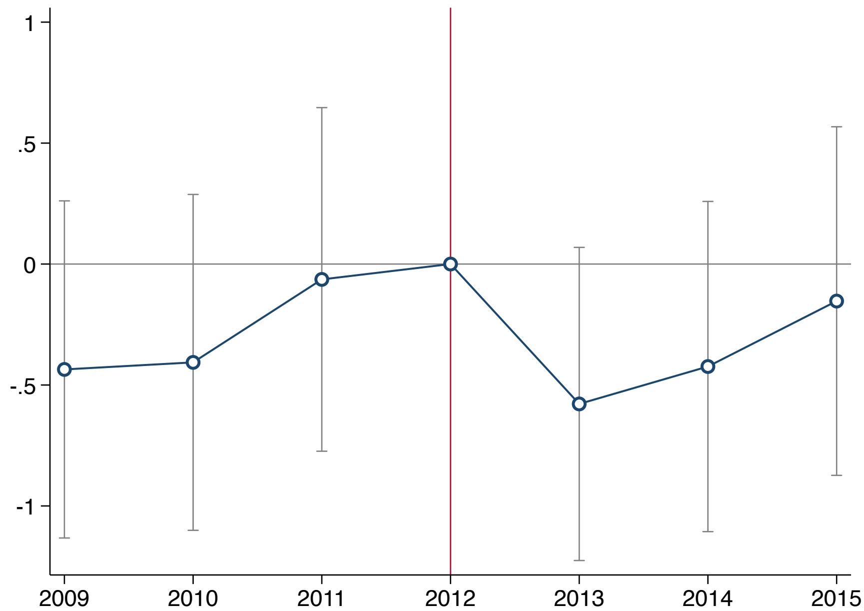
Figure A15: Impact on employee retention rate by skill group (50% sample)

(a) High-skill workers



NOTES: This figure reports the estimated impact of the CICE tax credit on wages of high-skill workers. Other notes as in Figure 5.

(b) Low-skill workers



NOTES: This figure reports the estimated impact of the CICE tax credit on wages of low-skill workers. Other notes as in Figure 5.

A.6.3 Contract types

Table A20: Impact on hourly wage earnings (log), by contract type

(a) By contract type

	Mean hourly wage (log)					
	Permanent contract			Fixed term contract		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.390***	0.493***	0.644***	-0.0927	-0.0511	0.0726
	(0.0681)	(0.0844)	(0.124)	(0.209)	(0.282)	(0.486)
Observations	824203	158348	42558	558965	71448	13871
R^2	0.927	0.850	0.757	0.692	0.680	0.714
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	118342	22747	6115	102170	15783	3417

(b) By skill level among permanent contract employees

	Mean hourly wage of permanent contract employees (log)					
	High skill employees			Low skill employees		
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.427***	0.529***	0.688***	-0.0407	-0.00867	0.0358
	(0.112)	(0.131)	(0.176)	(0.0784)	(0.102)	(0.165)
Observations	678019	118527	29901	777535	136174	33457
R^2	0.816	0.767	0.705	0.860	0.833	0.821
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	Yes	Yes	Yes	Yes	Yes	Yes
# firms	105245	18957	4842	114798	20922	5304

NOTES: This table reports the estimated impact of the CICE tax credit on wages of employees by contract type. A permanent contract is, in the French context, a *contrat à durée indéterminée*. A fixed term contract is any other type of contract, the most common being a *contrat à durée déterminée*. Other notes as in Table 2.

Table A21: Impact on hourly wage earnings of newly hired permanent contract employees (log)

	Mean hourly wage (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.117	0.126	0.246	0.241	-0.182	-0.199
	(0.233)	(0.233)	(0.344)	(0.345)	(0.656)	(0.655)
Observations	488952	488952	53568	53568	9254	9254
R^2	0.679	0.679	0.686	0.686	0.689	0.690
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0.3	0.3	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033
Controls	No	Yes	No	Yes	No	Yes
# firms	99205	99205	14205	14205	2785	2785

NOTES: This table reports the estimated impact of the CICE tax credit on wages of permanent contract employees who are entrants, i.e. who were not working in the firm in the past year. Other notes as in Table 6.

A.6.4 Hold up power

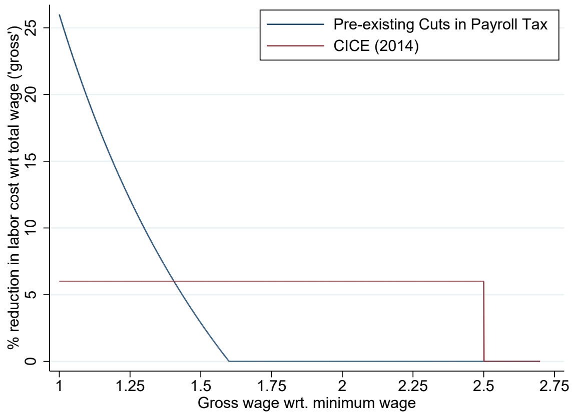
Table A22: Heterogeneity by sector based on sectoral proxies for frictions

	Mean hourly wage (log)					
	Separation rate			Average tenure		
	(1)	(2)	(3)	(4)	(5)	(6)
Below median	0.345***	0.401***	0.649***	0.433***	0.581***	0.665***
	(0.0952)	(0.114)	(0.170)	(0.114)	(0.134)	(0.195)
Above median	0.371***	0.526***	0.532***	0.322***	0.397***	0.540***
	(0.0861)	(0.110)	(0.160)	(0.0770)	(0.0976)	(0.145)
Observations	830144	159551	42854	830144	159551	42854
p-value equality	0.84	0.43	0.62	0.42	0.26	0.61
Window defining cells	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0.3	0.5	0	0.3	0.5
Width Cells	.033	.033	.033	.033	.033	.033

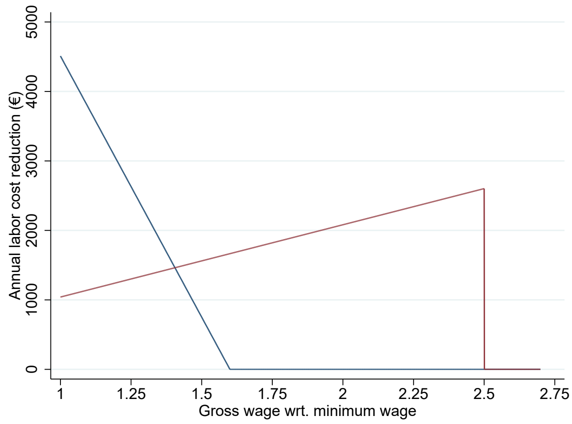
NOTES: The turnover rate is defined, following Garin and Silvério (2019), as the rate of separation among employees with a permanent contract (CDI). This rate is computed based on the DADS dataset over the 2009-2012 period. The tenure is defined based the date of arrival in the company reported in the Labor Force Survey over the same period (2009-2012). Both measures are averaged at the 2-digit industry level. The median is then determined on a firm-weighted basis within the estimation sample and firms are classified as above or below the median sector. The line "p-value equality" report the p-value of the test $\widehat{\beta}^{\text{above median}} = \widehat{\beta}^{\text{below median}}$. Each regression includes a set of bin $\times$ size (4 categories) $\times$ sector (3-digit) $\times$ year fixed-effects. Bins are defined as a unique combination of the proportion of wage bill accruing to workers making less than 2.2 and less than 2.8 times the minimum wage (both variables are discretized through truncation into 31 values).

Figure A16: The schedule of the CICE in comparison with pre-existing social security contribution cuts

(a) expressed in percent of gross wage



(b) expressed in euros per year



NOTES: The figures plot the surplus induced by the CICE in red and pre-existing payroll tax cuts in blue by wage levels (in multiples of the minimum wage), both in percent of gross wage earnings (panel a) and in euros (panel b).

ONLINE APPENDIX

Who benefits from tax incentives? The heterogeneous wage incidence of a tax credit

Clément Carbonnier   Clément Malgouyres

Loriane Py   Camille Urvoy

July 2021

OA1 Description of variables used in the analysis

Employee-level analysis:

1. **Hourly wage variable**: we define hourly wage as the ratio of gross earnings (*salaire brut*) over worked hours in a given job (*nombre d'heures*).

2. **New hire**: Hires are defined as jobs starting in February or later at year t taken up by workers not employed in the same firm at $t-1$. Firms with no employment at year $t-1$ are excluded.

3. **Incumbents**: Incumbents are defined as workers in permanent contract working full-time (32 hours per week or more) who kept the same occupation within the same firm between t and $t-1$.

Firm-level analysis:

1. **Occupation variable**: we define low-skill workers as those whose occupation (variable *CS*) is documented in DADS database as employee (*employé*) and blue collar (*ouvrier*) occupations, we define high-skill workers as those whose occupation is documented as intermediary occupations (*professions intermédiaires*) or executives and intellectual occupations (*cadres et professions intellectuelles*).

2. **Average hourly wage at the firm-level**: this variable is equal to the ratio of the sum of the gross wages accruing to workers in permanent contract (*contrat à durée indéterminée*) to the sum of worked hours for the same set of workers. We defined low and high-skill mean wage analogously (DADS).

3. **Actual treatment intensity**: the ratio of the amount of CICE claimed to the tax services and recorded in the MVC database over the sum of gross wages (DADS).

4. **Instrument for treatment intensity**: the instrument of exposure intensity is the CICE rate times the ratio of the sum of gross wages accruing to workers whose hourly wage is between .85 and 2.5 MW over the sum of overall gross wages (DADS).

5. **Grid variable**: We compute for each firm the share of the wage bill below 2.2 MW and below 2.8 MW in 2012 (alternative specification with 2.3 and 2.7MW points), the year prior the reform (DADS). We discretize these variables ranging from 0% to 100% with a step of 3.33 percentage points (alternative specification with steps of 2 and 5 percentage points). The interaction of these two variables yields the grid.

6. **Sectoral variable**: The sectoral variable is the variable APEN (DADS) documenting the main activity of the firm through a 3-digit classification.

7. **Size variable**: The size variable takes on 3 values and is defined based on the full-time equivalent employment variable (DADS). The three values are defined as follows: 1– less than 50, 2– between 50 and 250 ,3– 250 or more.

8. **Cell variables**: The cell variable is the interaction of grid, sectoral and size variables.

9. **Control variables**: We include 3 main lagged control variable (1) share of employees paid less than 1.5 MW (DADS) , (2) the log value of assets (*valeur des immobilisations* in FARE), and (3) the log productivity (value-added over average employment in FARE).

OA2 Empirical material

OA3 Conceptual framework for bunching analysis: a static wage posting model

We explore how bunching might arise from a discontinuity in the CICE schedule in a simple wage posting model. Consider a setting where each firm has a vacancy associated with an heterogeneous productivity p. Workers potentially accepting the offer have a reservation wage distributed according to a cumulative distribution function denoted $H(w)$. The expected profit of a p-type firm posting a wage w will therefore write as:

$$\pi(w,p) = H(w)(p-w)$$

When setting wages, the firm faces the standard trade off between the increasing the probability of filling a vacancy and increasing the per vacancy profit in case it is filled. We assume that $\pi(w,p)$ is strictly concave in p which holds under standard conditions on the cumulative distribution function $H()$.[66] Interpreted as a direct subsidy on labor paid below a given hourly threshold, the CICE introduces a notch in the labor cost schedule thus modifying the expected profit function as follows:

$$\pi(w,p,s) = H(w)(p - w(1 - s\mathbb{1}_{\{w \leq w_T\}}))$$

where s is the subsidy rate of the CICE and w_T is the wage threshold above which it does not apply. In this context, firms with values of p falling in a given range $S = [p_L, p_H]$ will have an incentive to bunch and propose wage at w_T. The lower bound of the set S is determined by the firm which was optimally choosing to set wage at w_T before the policy, i.e.

$$p_L = \{p : \pi'(w_T, p, 0) = 0\}$$

[66]The $\pi(p,w)$ to be concave in w it is sufficient that $\partial^2\pi(p,w)/\partial w^2 < 0$ which will be true if: $H''(w) < 0$ which holds for standard distribution such as the Pareto distribution.

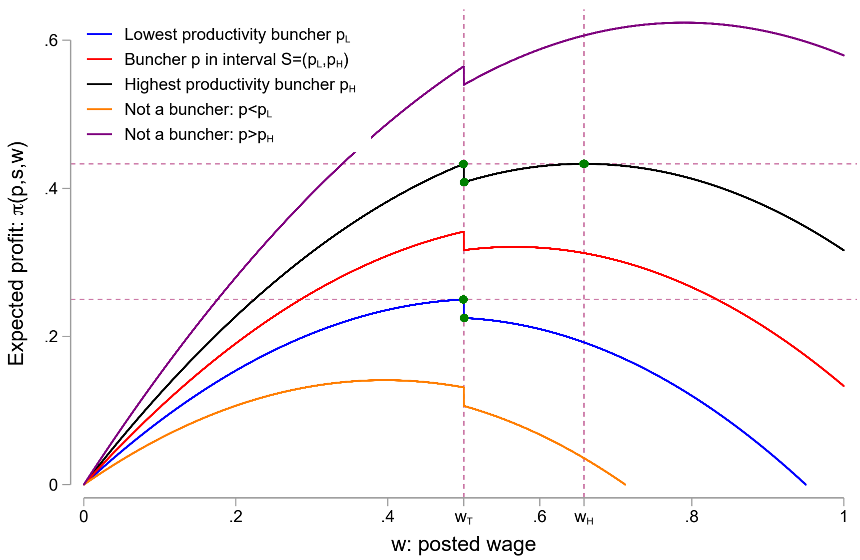
The upper bound corresponds to the firm that is indifferent between proposing wage exceeding w_T and therefore not benefiting from the tax credit and setting a wage equal to w_T and keep the eligibility of the program. That is p_H is defined as follows:

$$p_H = \{p : \max_w \pi(w, p, 0) = \pi(w_T, p, s)\}$$

Let us denoted w_H the profit maximizing wage that the firm whose productivity is equal to p_H will post in the absence of subsidy: $w_H \equiv \text{argmax}\pi(w, p_H, 0)$. In the case we assumed here, in which the heterogeneity of the wage of new hires is solely determined by the productivity of vacancy-posting firms (heterogeneity in p's), the model predicts that there should be a missing mass between w_T and w_H and an accumulation point at w_T.

We represent graphically the choice to bunch or not for firms of different productivity level in Figure OA1. The black and blue lines represent the expected profit as a function of posted wage w for the highest and lowest bunching firm respectively. The highest buncher is indifferent between posted w_H and w_T as it results in the same expected profit.

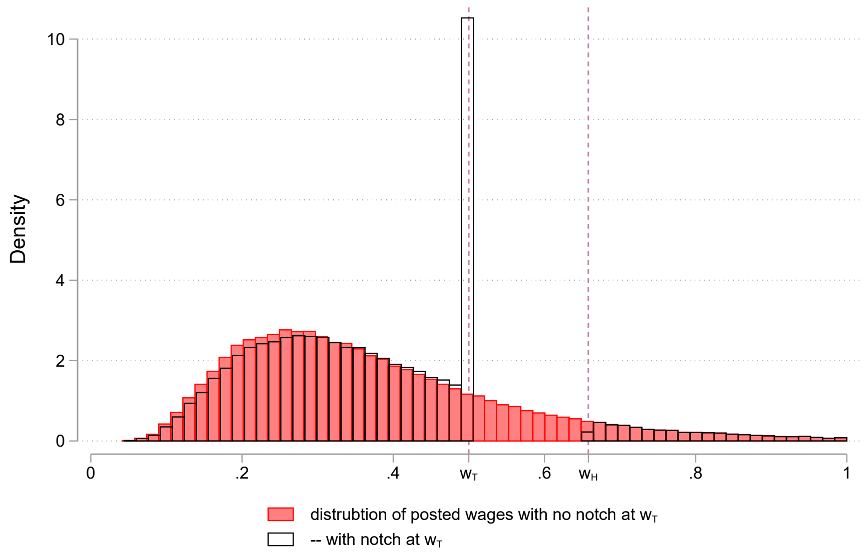
Figure OA1: Bunching in posted wages in a static model



Notes: The model predicts that firms with productivity $p \in (p_L, p_H)$ have an incentives to bunch their posted wages at w_T – i.e. the threshold above which the subsidy rate s ceases to apply. Accordingly, that there should be a missing mass at the right of the threshold w_T and bunching, i.e. an accumulation point at threshold w_T. Here the missing mass region would be $S_w = (w_T, w_H)$. The figure is drawn assuming $H(w) = w$, i.e. reservation wages follow a standard uniform distribution $U(0, 1)$, wage threshold is set at $w_T = 0.5$ and $s = 0.10$.

To further illustrate the potential bunching in that setting, we draw 100,000 random productivity realizations p from a rescaled lognormal distribution and plot the histogram of the resulting posted wage distribution without any notch and with the notch. Results are displayed in figure OA2. The bunching is apparent whenever the notch is introduced. Naturally, in practice there are other sources of heterogeneity underlying the distribution of new hires' wages, moreover optimization frictions might be at play which would lead to a less strict version of bunching Kleven (2016). Still we would expected an accumulation point in w_T but some missing mass in region (w_T, w_H) without a fully zero mass region.

Figure OA2: Bunching in posted wages in a static model



Notes: The figures display the posted wage distribution from a simulation of 100,000 productivity draws of random variable p. The red bars are represented the distribution without a notch at $w_T = .5$ and the black bars are representing the distribution with the notch. The figure is drawn assuming $H(w) = w$,i.e. reservation wages follow a standard uniform distribution $U(0, 1)$, wage threshold is set at $w_T = 0.5$ and $s = 0.10$. It further assumes that $p = x/1.5$ where x is a lognormal distribution with paramters $\mu = 0, \sigma = \sqrt{2}$.

OA4 Semi-elasticity as labor share of tax incidence

Our empirical analysis – see equation (2) – yields an estimate of the semi-elasticity of firm average wage with respect to the *effective tax credit rate*, where the effective tax credit rate is defined as the amount of tax credit granted per euro spent on the wage bill. Denoting the effective tax credit rate as s, we estimate : $\widehat{\beta} = \frac{dw/w}{ds}$. Here we build on standard tax incidence analysis – see e.g. section 3.3 of Fuest *et al.* (2018) – in order to relate this semi-elasticity estimate to the incidence of corporate tax credit on wages. In particular, we are interested in how this estimate relate the share of the total business tax credit falling onto workers.

We consider a simple economy where identical workers have quasi-linear preferences with indirect utility $V(w)$ where w is the wage rate which we measure as the hourly wage. Firms have a profit function which writes:

$$\Pi = pY - wL + swL$$

where L denotes the number of workers (in terms of effective labor supply), Y the total of output, p the price of output. For simplicity we abstract away from other factors of production such as capital as we do not expect the tax credit to affect their price. As above, the parameter s denotes the effective tax credit rate, that is the amount of tax credit given per euro spent on the wage bill:

$$s \equiv CICE/wL = \text{rate of the tax credit} \times \text{share wage bill eligible}$$

where $CICE$ refers to total amount of tax credit. We compute overall changes in social welfare W following a marginal change in s:

$$dW = LdV + d\Pi$$

The envelop theorem implies $dV = L\frac{dw}{ds}ds$ and $d\Pi = -\frac{dw}{ds}dsL + wL \times ds + swL$.

Overall change in welfare writes as: $dW = wL \times ds + swL$. Accordingly the share of the change in welfare falling on labor writes as:

$$\begin{aligned} I_L \equiv \frac{dV}{dV + d\Pi} &= \frac{Ldw}{dw \cdot sL + ds \cdot wL} \\ &= \left(\frac{dw \cdot sL + ds \cdot wL}{Ldw}\right)^{-1} = \left(s + \left(\frac{dw/w}{ds}\right)^{-1}\right)^{-1} \\ (\text{assuming } s \approx 0) &= \frac{dw/w}{ds} \end{aligned}$$

We see that the semi elasticity of the hourly wage with respect to s can be interpreted as an approximation labor share of tax incidence. As long as the semi elasticity $\frac{dw/w}{ds}$ is large relative to the effective subsidy rate s, the approximation is valid. For instance, for a semi elasticity equal to $\widehat{\beta} = 0.5$ and the upper bound for an effective tax credit rate of 0.053 – corresponding to a case where all workers are eligible to the tax credit –, the true labor share of incidence is $I_L = \left(0.053 + (0.5)^{-1}\right)^{-1} \approx 0.487$.

Our main specification – specified in equation (2) – regresses ln(average hourly wage) on the instrumented empirical counterpart of s – defined in equation (1). The coefficient associated denote β is an estimate of the elasticity of the average hourly wage with respect to s. The estimate $\widehat{\beta}$ can therefore be interpreted as a close approximation of the share of the tax incidence born by labor.

OA5 Reduced-form and structural coefficients

In the paper, we focus on reduced form estimates. To motivate this choice, consider the main dynamic specification (which is given by equation 3 in the body of the text):

$$\ln(Y_{i,t}) = \alpha_i + \alpha_{c,t} + \sum_{\substack{d=2009 \\ d\neq 2012}}^{2015} \beta_d \cdot Z_i \cdot \mathbb{1}\{d=t\} + X'_{i,t-1}\gamma + \varepsilon_{i,t} \tag{8}$$

The reduced-form estimates from equation (8) reflect the sum of first stage times structural coefficients for different lags. To see this, let us denote $\beta^{\mathbf{s}}_{t,l}$ the structural coefficient representing the marginal effect at time t of the endogenous variable D_{it} with a lag of l (so-called treatment effect on the treated). We further denote the first-stage relationship between the endogenous variable at time t and the instrument measured in 2012 as $\frac{dD_t}{dZ_{2012}}$. The reduced-form or intent-to-treat coefficient in 2013, 2014 and 2015 can then be expressed as:

$$\begin{aligned}
\beta_{2013} &= \beta^{\mathbf{s}}_{2013,0}\frac{dD_{2013}}{dZ_{2012}} \\
\beta_{2014} &= \beta^{\mathbf{s}}_{2014,1}\frac{dD_{2013}}{dZ_{2012}} + \beta^{\mathbf{s}}_{2014,0}\frac{dD_{2014}}{dZ_{2012}} \\
\beta_{2015} &= \beta^{\mathbf{s}}_{2015,2}\frac{dD_{2013}}{dZ_{2012}} + \beta^{\mathbf{s}}_{2015,1}\frac{dD_{2014}}{dZ_{2012}} + \beta^{\mathbf{s}}_{2015,0}\frac{dD_{2015}}{dZ_{2012}}
\end{aligned}$$

The main advantage of focusing of the reduced-form is that it allows us to remain agnostic as to the lag structure of the effect of the endogenous variable on wage. The structural coefficients cannot be readily identified without further assumptions – as there 3 equations and 6 structural parameters. For instance, Giupponi and Landais (2018) resort to a stationarity assumption (i.e. $\beta^{\mathbf{s}}_{t,l} = \beta^{\mathbf{s}}_{t',l}$ with $t' \neq t$ and $t', t > 2012$) to recursively identify the structural coefficients. Considering that the policy might take some time to unfold and gain in salience among firms, one might expect effects to vary with calendar time for a given lag, and therefore the assumption is not likely to fit our setting well. Accordingly, we prefer to focus on the reduced-form coefficients.

References:

Giupponi, G., & Landais, C. (2018). Subsidizing Labor Hoarding in Recessions: The Employment & Welfare Effects of Short Time Work. *CEP Discussion Paper* No 1585 December 2018