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Wage Incidence of a Large Corporate Tax Credit: Contrasting Employee- and Firm-Level Evidence *

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February 20, 2019

Abstract

The present paper sheds new light on the incidence of firm taxation by exploiting the design of a large-scale corporate income tax credit in France. The tax credit is proportional to the wage bill of workers paid below a hourly wage threshold, which induces a discontinuity in mandatory levies at the employee level. We use discontinuities at the employee level in order to estimate firm-level incidence. This turns out to be the relevant level for the effects of the policy, which would be undetectable with an estimation focused on the employee level impact of the shock. Relying on exhaustive matched employer-employee data, we find a discrepancy between the absence of incidence at the employee level and a substantial incidence on wages at the firm level, around 50 %. We find moreover that the policy in question has stark (anti)-redistributive effects. The tax cut is targeting the lowest part of wage earners, but the benefits accrue to other employees inside the firm, who earn substantially higher wages on average.

Keywords: tax credit, incidence, rent sharing; **JEL codes:** D22, H25, H32

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1 Introduction

In the US, the 2017 Tax Cuts and Jobs Act reduced the statutory rate of the corporate income tax (CIT) from 35 % to 21 %, continuing the decline in the effective tax rate that occurred over the past 25 years (Dyreng *et al.*, 2017). Similar downward trends of CIT are under way in the EU (DG Taxation and Custom Union, 2017). In a context of growing international competition, most advanced economies attempt to reduce mandatory levies on firms in order to support activity and competitiveness, either by decreasing CIT effective rates or by implementing payroll tax cuts targeting subsets of the workforce. These changes can have wide implications for domestic and foreign investment, for employment, as well as for the income distribution (Harris *et al.*, 2009). Assessing the distributional consequences necessitates not only estimating the magnitude of wage incidence in order to determine the share of the benefits from lower firm taxation captured by wage earners – as opposed to capital owners –, but also measuring how these wage gains are distributed among workers – high- versus low-skill/wage (Auerbach, 2018).[1]

This paper sheds new light on the wage incidence of firm taxation by taking advantage of a French reform which provides a quasi-natural experiment inducing variation in the burden of firm taxation. The Competitiveness and Employment Tax Credit (CICE hereafter)[2], introduced in 2013, is attributed for employees with wages up to 2.5 times the minimum wages (MW). The schedule therefore introduces a sharp discontinuity in mandatory levies at the 2.5 minimum wage (MW) eligibility threshold. It generates treatment intensity differences between otherwise similar employees and firms. Leveraging this discontinuity and using a rich matched employer-employee database, we are able to estimate and contrast the incidence of the tax credit from an employee- and a firm-level perspective. We develop a firm-level methodology based on employee-level discontinuity allowing us to estimate a collective incidence that cannot be detected by employee-level methodologies. Our results point to a discrepancy between the absence of incidence at the employee level and a substantial incidence on wages at the firm level. Moreover, the actual distributive impact of the tax credit is disconnected from the targeting of the policy as wage increases mostly benefit high-skill workers.

Our empirical analysis is first carried out at the employee level. It aims at detecting a possible deformation of the wage structure around the 2.5 MW eligibility threshold,

[1] Auerbach (2018) concludes his analysis of the recent US CIT reform by writing: " [...] *One should not think about wage and salary earners as a monolithic group. Whether through differences in rent sharing across the income distribution, or differences in capital-labor complementarity that lead to differences in gains, the effects of corporate taxation on different groups of wage earners is another direction in which distributional analysis needs to develop.*".

[2] CICE is the acronym in French: Crédit d'impôt pour la compétitivité et l'emploi.

based on the methodologies developed by McCrary (2008) and Chetty *et al.* (2011). However, we do not find any bunching in the distribution of new hires' wages nor any deformation of wage growth distribution with respect to the initial wage. These results are consistent with a growing literature concluding the absence of payroll tax net wage incidence at the employee-level (Bosch and Micevska-Scharf, 2017; Bozio *et al.*, 2017a).[3] They can have two alternative explanations: (*i.*) economic and statutory incidence coincide and employers bear the full burden of corporate taxes (Alvaredo *et al.*, 2017); (*ii.*) incidence on wages is not aligned with the policy targeting and benefits to a wider range of employees (Saez *et al.*, 2018).

To determine which mechanism dominates, we develop a firm-level analysis. We isolate the variation in firm-level treatment intensity that stems from the discontinuity of the schedule at the employee level. We construct cells of firms with similar pre-reform characteristics thanks to a grid based on the wage bill cumulative density function slightly below and above the 2.5 MW threshold (actually at 2.2 MW and 2.8 MW) interacted with sector and size categories. We confirm the validity of the within cell difference in difference estimations: within cells (*i.*) the common trend assumption holds; (*ii.*) the variation of treatment intensity remains large.

Our results point to a substantial wage incidence of CICE at the firm level: a €1 increase in CICE translates into a €0.5 increase in overall wage bill. The magnitude of the wage incidence of the tax credit meets a recent body of work estimating that labor bears between 30 % and 50 % of the CIT burden, relying on different sources of variation: CIT rates differences across industries (Liu and Altshuler, 2013), CIT rates changes at the country level in Europe (Arulampalam *et al.*, 2012) or in the OECD (Azémar and Hubbard, 2015), at the state level in the US (Suárez Serrato and Zidar, 2016) and at the municipal level in Germany (Fuest *et al.*, 2018).

Our analysis highlights that the absence of wage adjustment at the employee level hides a substantial incidence at the firm level. This pattern is also found in a recent study by Saez *et al.* (2018) of a program of payroll tax cut (rather than CIT cut) targeting young workers in Sweden (rather than workers below a given wage in France). They conclude that 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.

The existence of incidence at the firm-level but not at the employee-level does not mean however that wage incidence is homogenous. We demonstrate that only upper

[3] 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.

socioprofessional categories benefit from wage increases, although they are less likely to contribute to the firms' CICE eligibility. The heterogenity in wage incidence between skill groups is also documented by Fuest *et al.* (2018).[4] Overall, heterogeneity in wage incidence across socioprofessional categories suggests that reforms of firm taxation may have substantial effects on the income distribution, which is consistent with an emerging literature highlighting the role of firms in shaping labor market inequalities (Card *et al.*, 2016).

The rest of the paper is organized as follows: the CICE scheme is explained in Section 2 and the databases are detailed in Section 3. In Section 4, we present evidence on employee-level incidence. The identification strategy at firm-level is documented in Section 5, in addition to central results, robustness checks and tests for heterogenous impacts. Section 6 concludes and puts into perspective the impacts of the CICE we highlight in this paper.

2 The Competitiveness and Employment Tax Credit

This section describes the policy we are leveraging to uncover the mechanisms driving the wage incidence of a corporate income tax credit. Labeled CICE, the implementation of the Competitiveness and Employment Tax Credit was decided in late 2012 by a newly elected government facing a challenging economic context (high unemployment, low corporate profitability[5]). Introduced January 1^{st} 2013, its amount is equal to a proportion of the wage bill accruing to workers paid below 2.5 MW: employees paid above 2.5 times the minimum hourly wage trigger no corporate income tax credit while others allow the firm to benefit from a corporate income tax credit equal to 4% of their wage bill in 2013 and 6% of their wage bill in 2014 and 2015.[6]. In addition to the large amount it represents (0.56 point of GDP in 2013 and respectively 0.83 and 0.82 point of GDP in 2014 and 2015) the CICE has two main characteristics useful for identification of its effect on wages: the design of the schedule introduces a discontinuity at 2.5 minimum wage (MW) and could hardly be anticipated.

→ **The CICE could not be anticipated firms** The idea behind the CICE originates from a report by Louis Gallois (2012) – French businessman and former CEO –, on the behalf of the government. He emphasized the loss of French market shares worldwide and the need to restore firm competitiveness. He therefore advocated

[4]Studying opposite CIT changes – tax hikes instead of tax cuts –, they show that low-skill workers were the most penalized by wage decreases due to CIT increases in Germany.

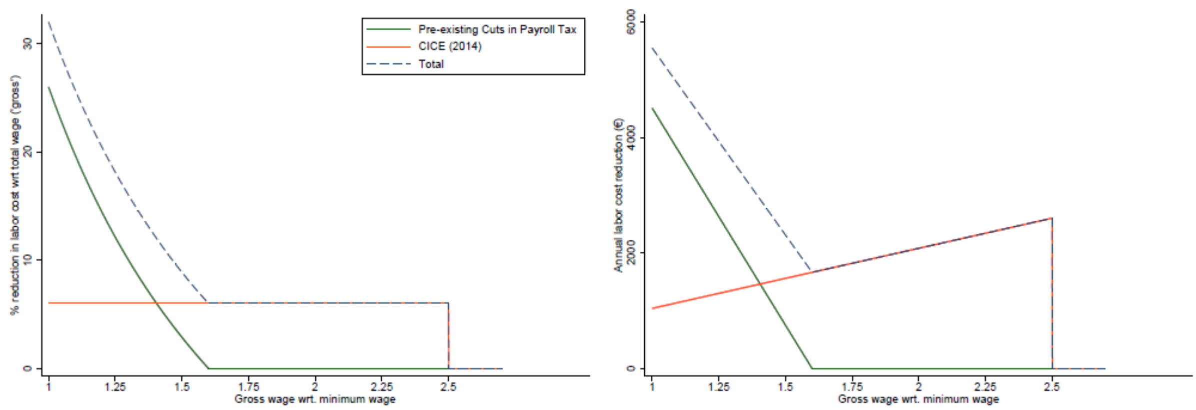
[5]The profit rate – ratio of gross operating surplus to value added – was at 30.25 % in 2012, a figure lower than in previous years (31.70% over the years 2008-2011 and 32.63% for the years 2000-2010. Source: INSEE, Comptes nationaux).

[6]The CICE is still ongoing, but our analysis ends in 2015 as additional payroll tax cuts were implemented in 2016, with the potential of interfering with our estimations.

for a payroll tax cut up to 3.5 MW in the manufacturing sector (in the form of CIT and of a cut in social security contributions). This cutoff was meant to ensure that exporting firms, which tend to pay higher wages, would largely benefit from the measure. However, the estimated cost of such a measure was very high and the 3.5 MW was deemed too high by labor economists on the ground that the elasticity of labor demand to labor cost is higher for the lower part of the wage distribution.[7] Due to this debate, the actual CICE differs substantially from the recommended policy: it consists in a CIT credit instead of social contribution cuts and was set up to 2.5 MW instead of 3.5 MW. Furthermore, little time elapsed between the release of the report – November 5^{th} 2012 – and the implementation of the CICE beginning January 1^{st} 2013 (from Finance Act of December 29^{th} 2012), making any possibility of anticipation quite unlikely.

→ **The CICE is discontinuous.** If a firm has one employee paid 2.5 MW, it gets the maximum per capita amount of tax credit (4% of her wage in 2013, 6% in 2014 and 2015), while if the one employee is paid 2.51 MW, the firm will get no tax credit. This discontinuity is a new feature in the French policies using mandatory levy cuts to decrease labor costs. Figure 1 presents a comparison of the CICE (in red) with pre-existing and still on-going payroll tax cuts. Prior to 2013, large payroll tax cuts targeted low wages, they consist in about 26% of the gross wage at the minimum wage and continuously decrease to reach 0 at 1.6 MW. In contrast, the CICE, which does not replace but supplements these pre-existing cuts, is flat up to 2.5 MW and falls down to 0 above this threshold. The CICE generates large differences between very similar employees as in 2015, 6% of 2.5 MW corresponded to 2,624 euros per employee.

Figure 1: The CICE schedule in comparison with pre-existing cuts on payroll tax

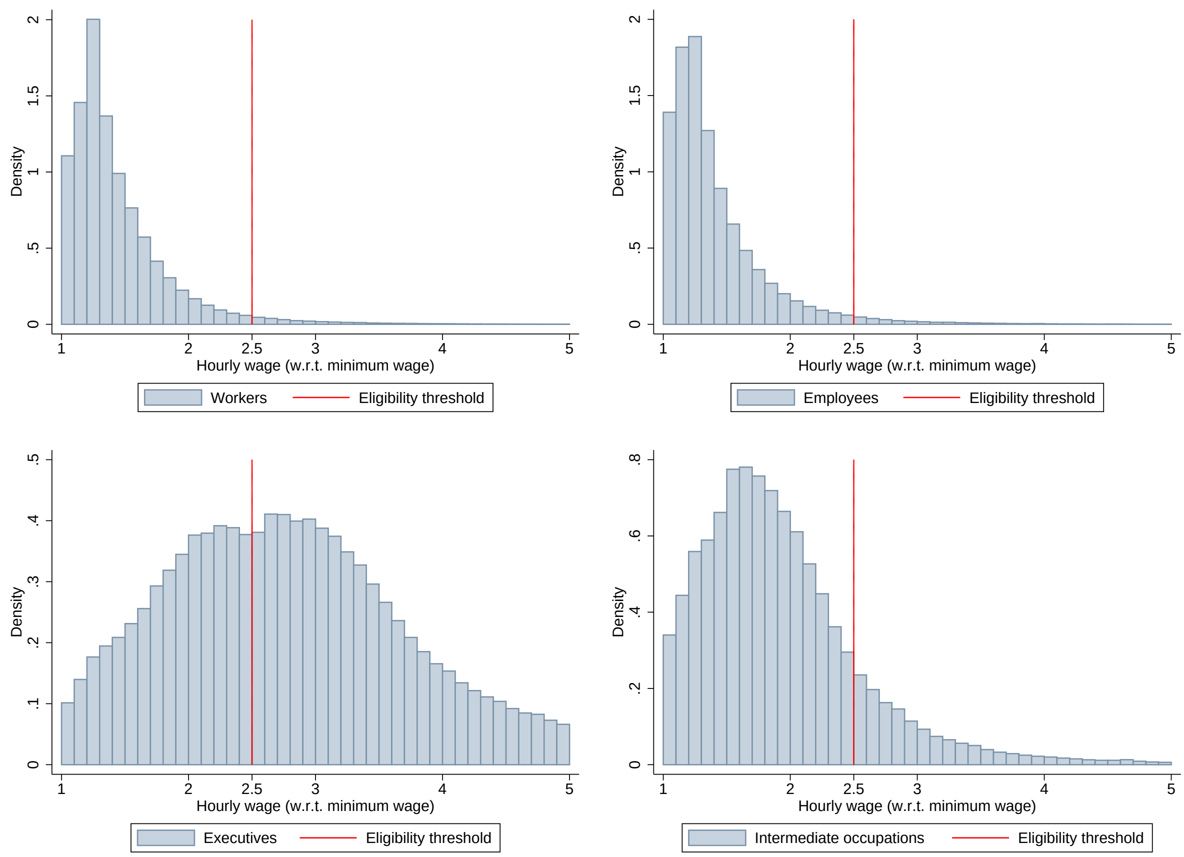


CICE as share of gross wage   CICE amount for a full time job

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

Beyond these features, particularly valuable for estimation purposes, the level of the threshold also implies that a very high share of workers are eligible. Figure 2 presents the share of wage bill eligible to the CICE by socio-professional category. Regarding blue collar workers and other employees[8] (on the top row, respectively on the left- and right-hand side), nearly all the salaried workers are eligible. Moreover, a substantial share of executives, intellectual and intermediate occupations[9] (bottom left and right respectively) is also eligible to the CICE. The threshold therefore generates an employee-level discontinuity at a wage level where density of high skill workers is large.

Figure 2: Distribution of wages by occupation



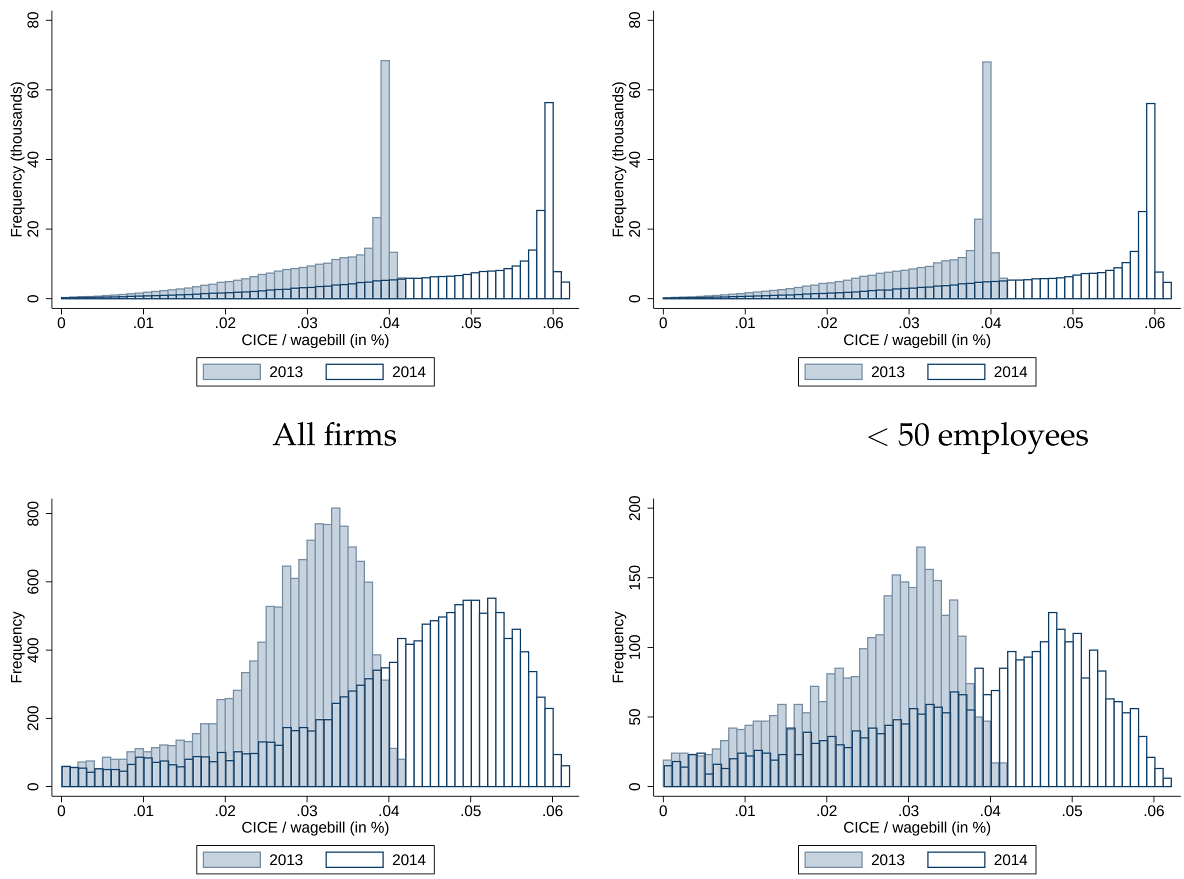
Note: Source: DADS 2012.

[8]We consider here the socioprofessional categories of the French statistics classification as it is the actual classification in the databases: blue collar workers correspond mainly to plant and machine operators and assemblers (8) in the ISCO-08 classification of the International labor organisation (ILO); other employees correspond to clerical support workers (4), services and sales workers (5) and elementary occupations (9). We group these categories and named them low-skill occupations as they correspond to the skill levels 1 and 2 of the ILO classification.

[9]Executives and intellectual occupations correspond to managers (1) and professionals (2) in the ISCO-08 classification; intermediate occupations correspond to technicians and associate professionals (3). We group these categories and named them high-skill occupations as they correspond to the skill levels 3 and 4 of the ILO classification.

Finally, although there is little variation in treatment status (extensive margin) given that most firms are eligible to the CICE, firms are differently exposed in treatment intensity (intensive margin) depending on their wage structure. Notably, as the threshold is central in the wage distribution of high skill workers, even firms with a similar socioprofessional composition of their workforce may be differently treated in the intensive margin. This is reflected in Figure 3 which depicts variation in treatment intensity according to firm size. The top left histogram is based on the full sample of firms. Many firms have all their employees eligible, they get the maximum of CICE relative to their wage bill (4% in 2013 and 6% in 2014 and 2015). The other graphs present the same distribution by firm size category. Interestingly, there is not only variation between firms of different size but also between firms belonging to similar size categories. This is especially true for medium-sized firms (bottom left) and for larger firms (bottom right). Our identification strategy – presented in section 5 – aims at isolating the exogenous part of this treatment intensity differences.

Figure 3: Distribution of firm-level treatment intensity size in 2013 and 2014



All firms

< 50 employees

50-249 employees

⩾ 250 employees

Note: Treatment intensity is defined as the amount of tax credit with respect to the firm's wage bill.
Sources: DADS, MVC & FARE, 2013 2014.

3 Data

The empirical analysis relies on the exploitation of employer-employee data over the period 2009-2015. The main features of these databases are presented here while additional details may be found in appendix A.1.

3.1 Data at the individual level

The annual social data declaration files (DADS) contain, for each salaried contract, information on gross wages, working time, socioprofessional categories, type of contract, gender, etc. The exhaustive DADS cannot be turned into a worker-level panel as the employee identifier is not recognizable from one vintage to the other. For each vintage however, the value of the variables are given for the current and the previous year. This enables us to compute the yearly growth rate of wages. Moreover the administrative identifiers of French firms (code *SIREN*) are available over time which allows to constitute a panel of employer employee data.

Information on wages, our main outcome variable, is central in our analysis. Gross compensation includes "all remuneration received by the employee under her contract of employment, before deducting compulsory contributions." These pay data present the advantage to be accurate and exhaustive. However they do not allow to decompose the overall compensation into different components. For instance, it includes bonuses for end of fixed-term contracts (corresponding to 10% of the amount received during the contract) which could lead to biases in the measurement of hourly wage growth since these bonuses inflate the total gross earnings but not the number of hours worked the year of the contract term. To overcome this potential issue, we focus in the empirical analysis on the mean wage of permanent contracts. Moreover, as it will be made clear below, we are able to predict very precisely the tax credit firms can officially claim on the basis of their pre-reform wage structure, meaning that these data are very reliable for the purpose of our study.

3.2 Data at the firm-level

General information on the production structure of firms, income statement and balance-sheet are presented in the FARE database of the ESANE system (annual business statistics). It is built by Insee[10] on the basis of the tax data, social declarations and a survey. This database covers all firms (including firms without employees) with the exception of the financial sector and agriculture sector. We use it to control for firm characteris-

[10] National Institute of Statistics and Economic Studies

tics, in terms of productivity (value added divided by average workforce) and capital stock (tangible and intangible assets). [11]

We also obtained access to the MVC tax files which document firms' CICE rights. This database, whose first vintage is 2013, gathers information for all firms benefiting from the CICE. For each firm we know the *initialization*, i.e. the amount of tax credit to which the firm is entitled for a given year. In contains around 800,000 observations per year but in the analysis we will exclude unincorporated firms for which information from other sources is lacking. A large number of firms benefits from a relatively small amount of CICE, with an average of €2,756 for micro-enterprises and €24,492 for SMEs, whereas the amounts received by large firms are ten to one hundred times larger: the 288 largest firms present in the database have in 2013 a tax credit approximately equal to that of the 496,750 micro-enterprises. However, in the empirical analysis, treatment intensity will be computed, not in absolute terms, but in relative terms as a share of firm wagebill, in order to measure how much labor cost is reduced for firms.

In order to carry out our difference in difference strategy and to avoid endogeneity issues, it is necessary to be able to measure the CICE to which a firm or an employee would have been entitled to, before the reform implementation, based on its wage structure. Such information was not directly collected by tax authorities prior to 2013. However, the DADS database allows us to approximate these values thanks to the accuracy on wage structure of firms. It is indeed possible to calculate the share of wage bill below 2.5 MW, and to compute a potential CICE.[12] This calculation for the years 2013 to 2015 of predicted CICE (given past wage structure) is very close to the amounts of CICE actually initialized with the tax agency and presented in the MVC database (see table 3 below).

3.3 Building the final database

The three previously presented databases are merged to build the final dataset. We first match the employee level data DADS with the MVC file on tax credit, which implies that we keep in the analysis only the firms benefiting from the CICE. We calculate the ratio of the CICE imputed on the basis of the DADS database over the actual CICE initiated in the MVC database. We exclude from the sample the firms whose ratio belongs to the upper percentile in 2013, in 2014 or in 2015. We then match it with the

[11]Note that information on profits is also available but is not sufficiently reliable to be used as a dependent variable because firms can account differently for the CICE (as a deduction of labor costs, as operating subsidies, other operating income or CIT deduction). Thus, the various measures of profit (EBIT, EBITDA, operating income) may or may not take into account the CICE depending on the firm.

[12]Similar wage structure indicators can also be built at other thresholds (1.5 MW in particular) in order to better approximate the distribution of wage in the firms.

FARE database. Only the firms present in the three datasets (DADS-FARE-MVC) are kept in the analysis We then make two selections. First, we keep only firms that have at least one full-time equivalent job over the years and exclude firms in the extreme percentiles of profit margin, capital intensity and wage growth. Second, we construct a balanced panel over the period 2009-2015 to avoid composition effects which could potentially bias estimations. The resulting dataset contains 325,329 firms, that amounts for 64.6% of total CICE in 2013 (64.3% in 2014 and 63.8% in 2015).

4 Absence of incidence at the employee-level

The sharp discontinuity (notch) at 2.5 MW could likely influence firms' wage setting behavior: the CICE rate is constant (4% in 2013 and 6% starting in 2014) and suddenly dies down at 2.5 MW. Thus, a full-time employee paid two and a half times the minimum wage opened in 2014 an annual tax credit of €2,624 for his employer. If she had been paid even a few euros more, her employer could not have received any tax credit. With perfect control over the assignment variable - namely the position of the employee's hourly wage with respect to the threshold - such a large notch should lead to discontinuity of the employees density function, with sharp bunching prior the threshold (Kleven and Waseem, 2013). With imperfect control over the assignment variable, it may lead to smoothed bunching before the threshold or to deformation of the distribution of outcomes (Carbonnier *et al.*, 2014): in the present case a slowing down of wage increases around the threshold. We test for such behaviors graphically and econometrically: we first analyze the wage distribution of employees, focusing on new hires for which the control over the assignment variable is maximum; we then turn to the distribution of wage growth for incumbent employees.

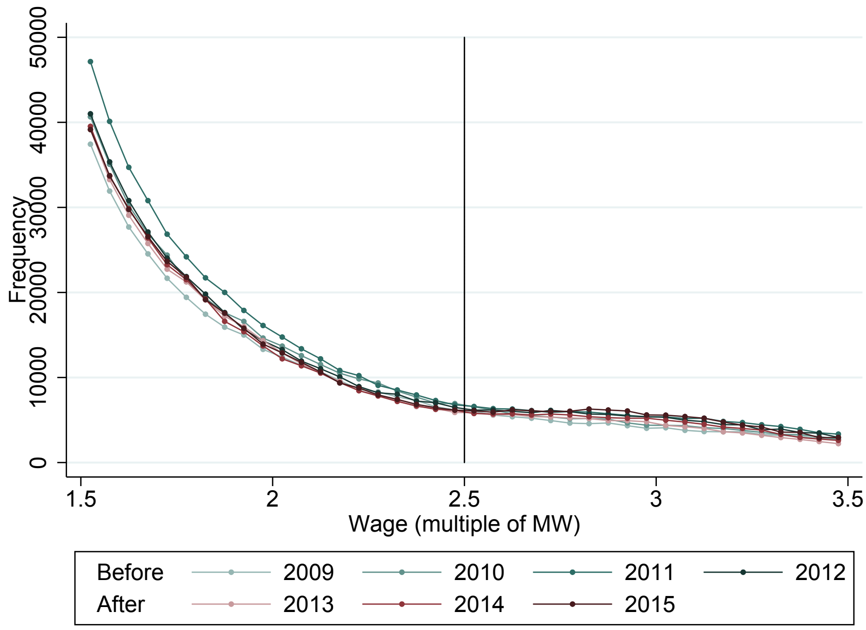
4.1 Distribution of new hires wages

As discussed above, the policy creates a direct incentive for the firm to set their employees' wages below the threshold. This incentive is however contradicted by the strong downward rigidity of nominal wages, giving firms little power to decrease wages. For this reason, we first focus on new hires, whose wage suffers no path dependency, and should reveal more evidently firms' behaviors, if any. In this analysis, new hires include permanent and fixed term contracts.

Testing for a discontinuity at the notch

A simple plot of the distribution of new hires' wage in a large window around the cutoff for years ranging between 2009 and 2015 suggests no major deformation of the distribution as the curves profiles are very similar (Figure 4).

Figure 4: Distribution of wage of new hires, 2009-2015

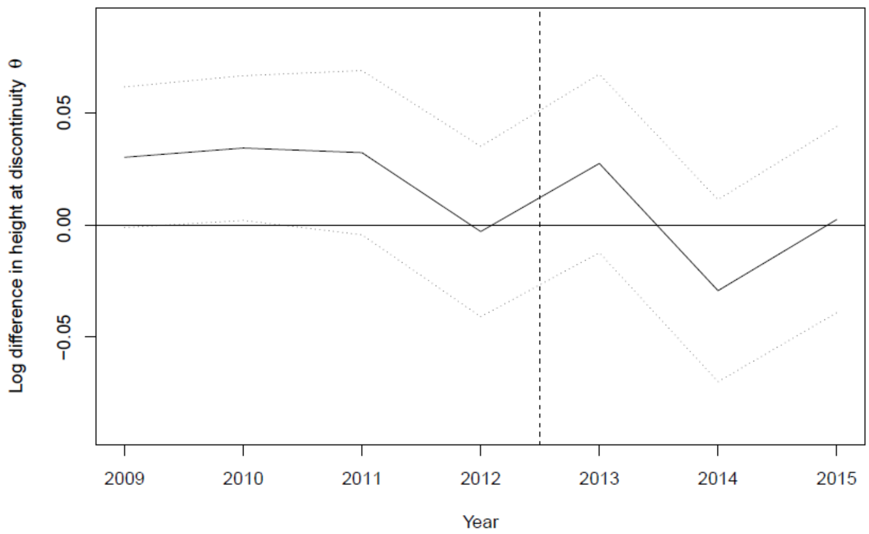


Note: The y-axis refers to number of hires in thousands. Hires are defined as jobs starting in February or later at year t that did not exist in year $t-1$ taken up by workers not employed in the same firm at $t-1$. Firms with no employment at year $t-1$ are excluded. We restrict the sample to hires by employers included in the firm-level estimation sample. Source: DADS 2009-2015.

We then turn to an econometric analysis. Following McCrary (2008), we implement linear regressions of the number of hiring in bins of the distribution of hiring wages (defined such that no bin contains wages both below and above the cutoff), with separate regressors on each side of the threshold. The parameter of interest is the log difference θ of the estimated number of hires at the notch. A positive θ indicates an excess mass to the right of the cutoff, a negative θ an excess mass to the left of the cutoff. It could be that 2.5 MW is special point in the distribution, generating "natural" discontinuity: to alleviate concerns about such possibly of pre-existing bunching at the notch confounding our analysis, we plot the estimated θ as well as their confidence intervals for each year of the 2009-2015 time frame (Figure 5).

Coefficients are very close to zero before and after the introduction of the CICE in 2013 and, overall, not statistically different from zero. The only exception is 2010: θ nearly becomes significant, with a small standard error. However, the magnitude of the coefficient remains very low and this coefficient is very close to those of other

Figure 5: Discontinuity estimates θ, 2009-2015



Note: The y-axis refers to the log difference in height at the cutoff in the wage distributions of new hires. Hires are defined as jobs starting in February or later at year t that did not exist in year $t-1$ taken up by workers not employed in the same firm at $t-1$. Firms with no employment at year $t-1$ are excluded. We restrict the sample to hires by employers included in the firm-level estimation sample. Source: DADS 2009-2015.

years. Hence, we can conclude at the absence of discontinuity of hires at the CICE threshold.

Estimating bunching to the left of the notch

The absence of discontinuity does not rule out the possibility of smoothed bunching before the threshold because of imperfect control over the assignment variable. To detect bunching of new hires to the left of 2.5 MW, we use a method developed in the case of kinks by Chetty *et al.* (2011) based on Saez (2010). The goal is to detect a local excess mass relative to the density near the threshold, while taking into account the fact that this local excess mass has to be compensated for elsewhere.

We first choose a window in which we believe bunching may occur. Our preferred window ranges between 2.3 and 2.5 MW, but alternatives are tested, with windows ranging from 2.0-2.5 to 2.4-2.5 MW, with 0.1 MW increments (see online complementary materials, results of all specifications are consistent). We then interpolate the density function of new hires outside the window. To do so, hires are grouped in bins of 0.75 MW width and wages are rescaled such that the discontinuity occurs in bin $i = 0$. The number of hires C_i in each bin i is regressed on a polynome of wages (Z_i) and on bin dummies for bins inside the bunching window $[-R, 0]$ (with $R = 0.2$ in central specification and $R = 0.1 - 0.5$ for robustness test in table A2). Polynomial coefficients – excluding bin dummies – are used to compute a counterfactual distribution of hires $\hat{C}_i$. Importantly, the counterfactual distribution accounts for the fact that the excess mass expected to the left of the notch has to be compensated for to the right of the notch. This adjustment is computed by successive iterations of the regression until a fixed point is reached. Finally, we compute the difference between actual and counterfactual hires within the bunching window (Equation 1).

$$\hat{B}_N = \sum_{i=-R}^{0} C_i - \hat{C}_i \tag{1}$$

The actual bunching estimator $\hat{b}$ is then given by equation 2, with standard error estimated by bootstrap method.

$$\hat{b} = \frac{\hat{B}_N}{\frac{1}{R+1}\sum_{i=-R}^{0} \hat{C}_i} \tag{2}$$

It represents the excess number of hires in the bunching zone expressed relative to the counterfactual density at the threshold. A significantly positive $\hat{b}$ would indicate an accumulation of hires in the bunching zone, a significantly negative $\hat{b}$ would imply a lack of hires in the bunching zone. Results are reported in Table 1.

Table 1: Detection of bunching of new hires before the threshold of CICE, [2.3, 2.5]

	Before the CICE				After the CICE		
	2009	2010	2011	2012	2013	2014	2015
$\hat{b}$	0.39	-0.16	-0.03	0.48	0.10	0.01	0.02
	(0.30)	(0.26)	(0.68)	(0.30)	(0.27)	(0.32)	(0.28)

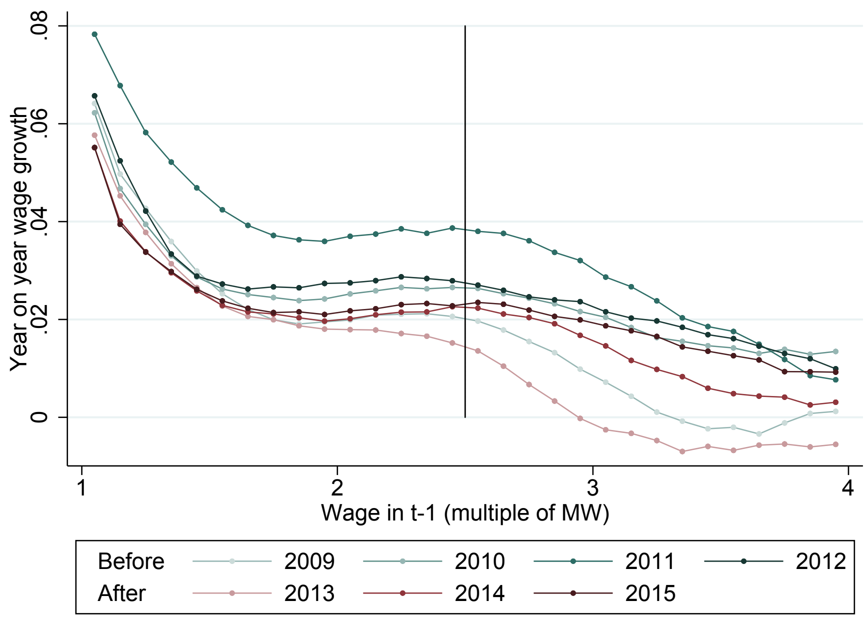
Note: New hires are defined as workers whose contract starts during the year and that were not previously employed in the same firm. *Sample restrictions*: The sample is restricted to employees whose contract starts during the year and that were not previously employed in the same firm. We further restrict the analysis to employers included in the firm-level estimation sample. Sources: DADS 2009-2015. *: p<0.05; **: p<0.01; ***: p<0.001.

The sample estimates are all close to zero and insignificant, both in the pre- and post-reform years, which confirms the previous results of absence of response in terms of hiring. Our analysis of the wage distribution of new hires demonstrates that firms did not change their wage-setting behaviors to take advantage of the discontinuity 2.5 MW created by the introduction of the CICE.

4.2 No deformation in the wage growth of employees

We now turn to incumbent employees, whose wages were already set prior to the reform, and analyze the distribution of wage growth according to initial wage. To do so, we keep only employees with permanent contract working full time in the same firm two consecutive years, and calculate the hourly wage growth in year n relative to year $n-1$. Figure 6 presents the evolution of wage growth for these " stayers" each year from 2009 to 2015. A brief visual inspection reveals no sharp deformation of the distribution in post-reform years: the curves' profiles for years before and after the reform are very similar, with no deformation of the wage growth distribution around the 2.5 MW threshold. The curves change from one year to another, but the overall pattern is stable over time: wage increases are larger for employees paid less than 1.5 MW, are stable at around 2% up to 2.5 MW, and then gradually decrease for higher wages. Since both pre- and post-reform curves exhibit this same pattern, this graphical analysis that the decline in wage growth after 2.5MW is not due to the CICE.

Figure 6: Distribution of wage growth of stayers, 2009-2015



Note: Stayers are defined as workers in permanent contract working full-time (32 hrs per week or more) who kept the same occupation within the same firm between t and $t-1$. *Sample restriction*: The sample is restricted employees in permanent contracts from year t to $t-1$ who stay within the same employer. We further restrict the analysis to employers included in the firm-level estimation sample. Bins are defined as intervals of equal length. The length is set so that there are 150 bins over the [150%, 350%] interval. Source: DADS 2009-2015.

We formally test for a potential deformation of the distribution around the 2.5 MW threshold for years after the reform. In the same spirit as for new hires, we seek to detect a deformation in the relationship between wage growth and initial wage of stayers.[13] We test for a deformation of the distribution within a window around the threshold. In the present setting, the window stretches both to the left and to the right

[13] 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.

of the cutoff as employers may reduce the growth rate of employees paid just above the threshold, waiting for potential minimum wage increase to make their wage eligible.

We model employee wage growth g_i as a n^{th} order polynomial of initial wage $w_{0,i}$. We estimate the model excluding the window [2.3,2.7]. We obtain a vector $\hat{\beta}^n$ of dimension $n+1$. Based on this vector, we predict the average wage growth within the window using the estimated parameter and the average values of the predictors $\bar{X} = [1, \overline{w_0^1}, \overline{w_0^2}, ..., \overline{w_0^n}]$, where $\overline{w_0^p}$ refers to the sample mean of the initial wage to the power p within the window around the threshold.[14] We then compare the average predicted wage growth based on the model estimated excluding observations in the window $\hat{g} = \bar{X}'\hat{\beta}^n$ and the observed wage growth $\bar{g}$. We test the significance of the gap $\hat{G} = \bar{g} - \hat{g}$ between actual and counterfactual growth wage, under the assumption that the two estimators are independent. Results of central specification are presented in Table 2. Within the excluded window, there seems to be no distance between the actual distribution and the interpolation although these bins were not used to fit the polynomial.

Table 2: Detection of deformation of the relationship between wage growth and initial wage – [2.3, 2.7] window

Year	$\bar{g}$	$\hat{g}$	$\hat{G} = \bar{g} - \hat{g}$	z-stat
2010	0.02708	0.02711	-0.00003	-0.10
2011	0.03786	0.03806	-0.00020	-0.95
2012	0.02733	0.02702	0.00031	1.13
2013	0.01387	0.01222	0.00164***	13.59
2014	0.02186	0.02161	0.00025	0.97
2015	0.02297	0.02267	0.00030	1.79

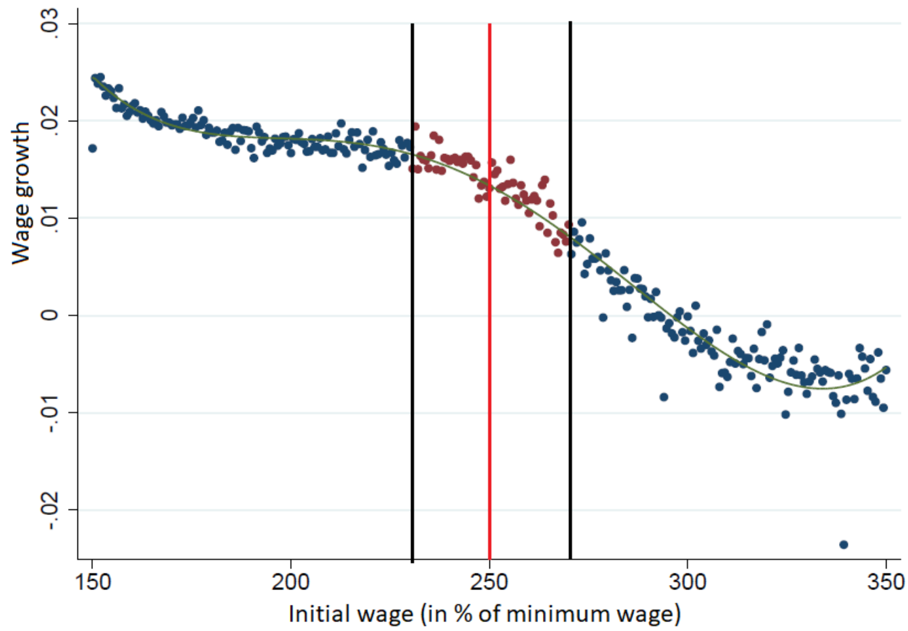
Note: $\hat{g}$ refers to the predicted mean wage growth based on a 10-degree polynomial in initial wage in the window around the threshold. The model is estimated excluding observations in the window. $\bar{g}$ is the observed sample mean of wage growth in the same window. *Sample restriction*: The sample is restricted employees in permanent contracts from year t to $t-1$ who stay within the same employer. We further restrict the analysis to employers included in the firm-level estimation sample. *Sources*: MVC, FARE, DADS Postes. *: p<0.05; **: p<0.01; ***: p<0.001

The econometric test shows that wages around the threshold do not increase less rapidly as the gaps between actual and predicted mean wage growth are close to zero, or even positive in 2013. Indeed, the 2013 estimate is significantly positive, implying that wages in the window actually increased more rapidly than predicted by the polynomial interpolation. This is not what should be expected assuming firm maximize their exposure to the reform. However in 2013, the CICE was equal to 4% of gross wage up to 2.5 MW. Therefore the significant coefficient for 2013 corresponds only to 0.16% additional wage growth in the window. Even significant, the coefficient may

[14] That is: $\overline{w_0^p} \equiv (\sum_i \mathbb{1}(i \in \text{window}))^{-1} \sum_i \mathbb{1}(i \in \text{window}) \times w_{0,i}^p$.

be interpreted as a significant zero. To confirm this interpretation, we therefore complement the statistical investigation with a graphical analysis by focusing on the year 2013 in figure 7.[15] We start by grouping employees in fine bins according to their wage in the previous year and compute the mean wage growth for each bin. We plot these wage growth means against hourly wage in year $n-1$. We then estimate a polynomial interpolation of this relationship (green line) excluding observations just around the 2.5 MW threshold (red dots).

Figure 7: Distribution of wage growth of stayers, in 2013



Note: Stayers are defined as workers in permanent contract working full-time (32 hrs per week or more) who kept the same occupation within the same firm between t and $t-1$. *Sample restriction*: The sample is restricted employees in permanent contracts from year t to $t-1$ who stay within the same employer. We further restrict the analysis to employers included in the firm-level estimation sample. Bins are defined as intervals of equal length. The length is set so that there are 150 bins over the [150%, 350%] interval. Source: DADS 2013.

We therefore conclude from this analysis that firms did not condition their wage increase behaviors to the eligibility of workers to the CICE. The reform had no effect on the wage distribution of new hires, although their wages could possibly be easily set below the cutoff. It also resulted in no deformation of the wage growth distribution, although we could expect firm to limit the wage increase for employees close to the threshold.

[15]This figure is also presented for other years in the appendix, see panels of figure A1

4.3 Implications and rationalization

The wage incidence analysis at employee-level compellingly documents that firms did not change their wage setting behaviors in response to the eligibility status of employees. Bozio *et al.* (2017b) points that incidence differs depending on the existence of tax-benefit linkage, with incidence at employee level only for contributory levies. For non-contributory levies - the case of the CICE - several papers conclude to the absence of incidence at the employee level (Alvaredo *et al.*, 2017; Bozio *et al.*, 2017a; Bosch and Micevska-Scharf, 2017).

Such absence of bunching at the notch is not trivial as it implies a sharp labor cost differential among otherwise similar workers. This result is at odds with standard models of the labor market where workers' marginal cost is equated to their marginal productivity. A first rationalization may lie in labor market institutions: collective wage agreements at the industry level - which cover an overwhelming majority of workers in France (OECD, 2012) - could prevent firms from adjusting wages. However, this agreements are not fully binding and firms still have flexibility in wage setting, as highlighted by the limited importance of downward wage rigidity after the 2008 recession in France (Verdugo, 2016).

An alternative rationalization is that the absence of incidence at the employee level hides wage incidence at the firm-level independently from the individual employee eligibility. Saez *et al.* (2018) study an age targeted non-contributory payroll tax cut in Sweden and find no incidence at the employee level (around the age threshold) but a substantial incidence at the firm level (wage increase independently from the age threshold). They propose within firm fairness norms as potential explanation (as previously proposed by Saez *et al.* (2012)). In the presence of relative wage concerns among coworkers, granting lower wage growth to employees just below the threshold so that they remain eligible might be perceived as arbitrary and unfair. Such firm policy could reduce motivation and eventually result in drop out (Dube *et al.*, 2015). As an employee-level analysis would fail to detect such a collective incidence, we now turn to a firm-level analysis.

5 Substantial Firm-level Incidence

We have documented that employee eligibility has not affected wage rates for new hires or wage growth for job stayers. The aim of this section is to analyze whether the CICE has led to wage adjustment at the firm level following the introduction of the CICE 2013.

5.1 Identification strategy

The aim of our firm-level analysis is to compare firms that are similar except in their exposition to the CICE, before and after its implementation in 2013. This section presents in details our identification strategy.

Exploiting firm-level treatment intensity

Our econometric analysis relies on the exploitation of firm-level variation in treatment intensity rather than in treatment status as most firms have at least on person eligible to the CICE and therefore benefit from it. Treatment intensity is defined as the ratio of tax credit claimed to the firm wage bill : it corresponds to an effective tax credit rate. Using data on the amount of tax credit claimed each year from 2013 to 2015, we can construct the treatment intensity as follows:

$$D_{it} = \left(\sum_{j \in i} w_{jt} h_{jt} \right)^{-1} \text{Tax Credit Claimed}_{it}$$

where w_{jt} and h_{jt} denote hourly wage and hours worked for worker j employed in firm i during in year t.

Addressing endogeneity concerns

We then need to tackle a potential endogeneity bias in treatment intensity which can arise from reverse causality issues. Indeed, treatment intensity is obviously a function of wages and employment, which are potential determinants of the CICE. For instance, if as expected by policy makers, the policy contributes to boost employment, it can automatically increase the amount of tax credit a firm can claim – as long as the hired person is paid below 2.5 MW. Alternatively, if a firm suffers from a negative shock, it might lower its wages, and the amount of tax credit with respect to overall wage bill should increase accordingly.

A common solution used in the literature to tackle this reverse causality is to build the treatment intensity using lagged values of the tax (or subsidy) base. This strategy was first used by Auten and Carroll (1999) in their estimation of the elasticity of taxable income, by applying the variation in the rate to the earned income the year preceding the reform. The same type of methodology, that involves applying contemporaneous changes in policy variables to pre-reform firm-level outcomes, was also used to evaluate the impact of social contribution rebates in France (Crépon and Desplatz, 2001) or more recently in Sweden (Saez *et al.*, 2018).

In the present case, we use the share of eligible wage bill (i.e. the amount of wages accruing to employees paid less than 2.5 MW divided by the total wage bill) in 2012, the year preceding the introduction of the CICE, to instrument the treatment intensity. The relative stability of the wage structure ensures a strong correlation between the *ex ante* measure of exposure and the *ex post* one. The predicted treatment intensity, based on the 2012 wage bill, is computed as follows:

$$Z_i = \left(\sum_{j \in i} w_{j0} h_{j0} \right)^{-1} \sum_{j \in i} w_{j0} h_{j0} \mathbb{1}(w_{j0} < 2.5 \times \text{Min Wage}_0) \times \tau \quad (3)$$

where w_{j0} and h_{j0} denote hourly wage and hours worked for worker j employed in firm i during the last pre-reform year denoted 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 yields 5.33%).

Our paper focuses on reduced-form estimates. It is nevertheless informative to check how our predicted treatment intensity relates to the actual treatment intensity. We thus regress the actual treatment intensity, our endogenous variable, on the predicted treatment intensity, our instrument. Table 3 presents estimation results and shows that our instrument is a good predictor of firms' treatment intensity. The coefficients are highly significant and not too distant from unity, with R-squares of .359 and .457 for 2013 and 2014 respectively.

Table 3: Regressions of the actual treatment intensity on the instrument for 2013 and 2014

	Actual Treatment Intensity (2013)	Actual Treatment Intensity (2014)
Predicted Treatment Intensity (2012)	0.832***	0.804***
	(0.002)	(0.002)
Observations	325,329	325,329
R^2	0.359	0.457

Note: This table presents results from a linear regression of the endogenous variable (amount of CICE claimed over gross wage bill) on the instrument (predicted CICE ratio based on 2012 wage structure). This is a cross-sectional regression with one observation per firm. Sources: DADS, FARE, MVC 2010-2015. *: p<0.05; **: p<0.01; ***: p<0.001.

Common trend assumption and cell matching

In this subsection, we describe our identification strategy in more details. An obvious issue with the instrument defined in this Equation (3) in a double difference setting is that differences in exposure to the CICE might reflect differences in firms' production function or in skill-intensity. If variation in treatment intensity were driven by such fundamental differences, the common trend assumption upon which the difference-in-differences relies would not hold. To deal with this obvious issue, our firm-level regression aims at isolating variation in treatment intensity which is driven only by discontinuity in eligibility at the 2.5 MW threshold.

In an ideal setting, the natural experiment approach would be to leverage this discontinuity by implementing a regression discontinuity design at the firm level, comparing the relative wage evolution of firms whose entire workforce is paid just below 2.5 MW in 2012 to those whose entire workforce is paid just above the threshold. However, in practice all firms exhibit a positive degree of within-firm wage dispersion and applying a direct RDD is not feasible. We approximate this approach by augmenting our difference-in-differences specification so as to rely on variation in treatment intensity that stems from local differences in the wage distribution around the 2.5 MW threshold.

We group firms so that they are similar in terms of industry, size and wage distribution, except around the eligibility threshold. To do so, we set the window within which we allow the distribution of wages to vary. The bounds of this window are 2.2 MW and 2.8 MW.[16] To ensure the distribution outside the window is comparable, we compute for each firm the share of the wage bill below 2.2 MW and below 2.8 MW in 2012, the year prior the reform. We discretize these variables ranging from 0% to 100% with a step of 3.33 percentage points.[17] The interaction of these two variables yields a grid. We implement estimations within cells constituted as the interaction of the grid and the sectoral and size categories. Figure 2 illustrates this method for firms in a given industry $\times$ size group. Within each cell, firms have a similar share of wage bill accruing to workers paid less than 2.2 MW and less than 2.8 MW.

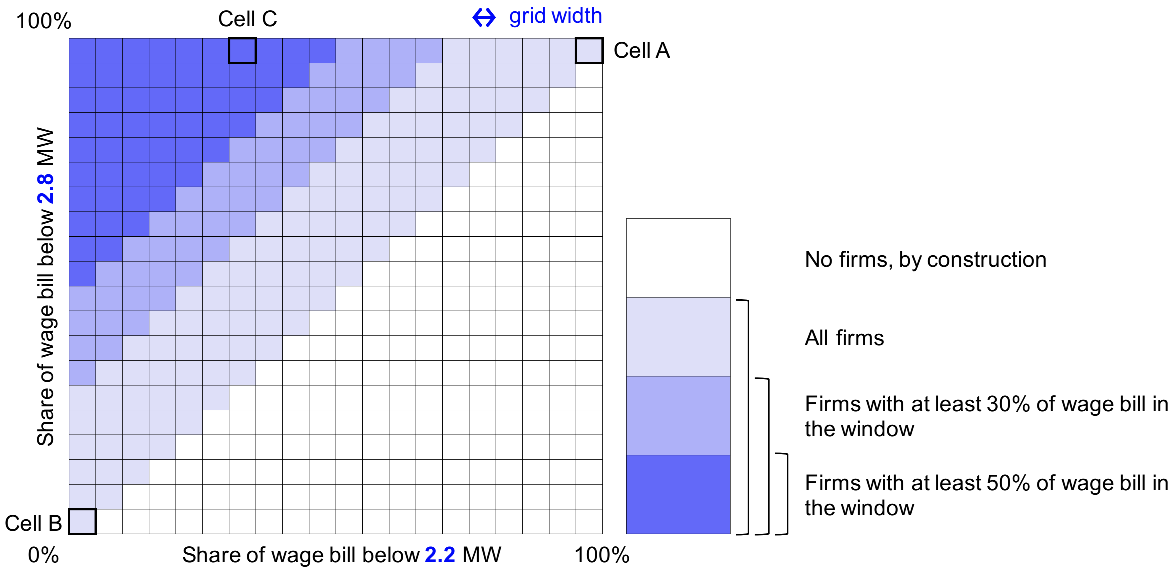
Figure 8 shows that firms in cell A have between 95% and 100% of their wage bill accruing to workers paid less than 2.2 MW, they are intensely exposed to the CICE, while firms in cell B have between 0% and 5% of their wage bill accruing to workers paid less than 2.8 MW. They are (almost) not exposed to the CICE. Although very differently exposed to the CICE, they are likely too different to be comparable. Instead, our methodology will compare firms with a similar wage distribution, except immediately around the eligibility threshold. To ensure this variation is substantial within-cell, we define subsets of firms according to the share of their wage bill paid to workers earning between 2.2 and 2.8 MW. The first subsample includes only firms whose share is higher higher 30%, the second firms whose share is at least 50%.[18] The existence of differences in the local repartition of wages produces substantial variation

[16] As a sensitivity check, we also experiment with setting the bounds to 2.3 MW and 2.7 MW.

[17] As a sensitivity check, we also use a step of 2 and 5 percentage points.

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

Figure 8: Illustration of the grid methodology applied to firms belonging to the same size category and industry



Lecture: Firms in cell A have between 95% and 100% of their wage bill accruing to workers paid less than 2.2 MW. Firms in cell B have between 0% and 5% of their wage bill paid to workers earning less than 2.8 MW. Firms in cell B have between 30% and 35% of their wage bill paid to workers earning less than 2.8 MW, but between 95% and 100% of their wage bill above 2.2 MW. It implies that 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 firms, by construction, as the share of wage bill below 2.2 MW cannot be larger than the share of wage bill below 2.8 MW.

in treatment intensity between otherwise similar firms. In addition, this variation is less likely to be correlated with confounding factors.

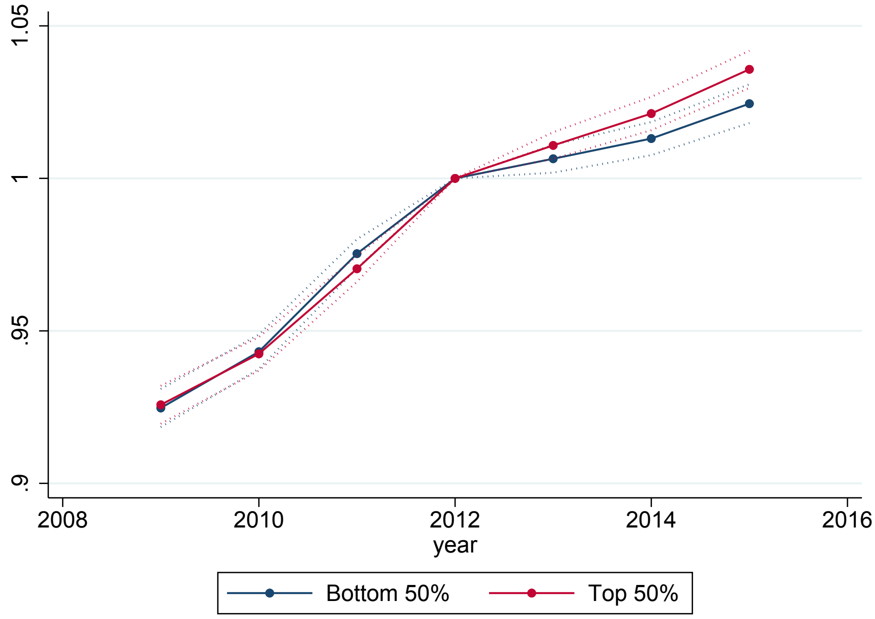
To illustrate the intuition behind our cell methodology, we plot the evolution of the mean hourly wage (base 1 inf 2012) of two categories of firms (Figure 9). We compute a within-cell median treatment intensity, and compare firms whose treatment intensity is above or below the within-cell median. Since, within-cell, firms are similar, except for their exposure to the CICE, we expect that the two groups have similar pre-trends. Indeed, wages of the two groups superimpose nearly perfectly for the pre-reform years. From 2013, wages gradually diverge as they increase relatively more in firms more intensely treated, i.e. above the within cell median.

Specifications

The baseline specification writes as follows:

$$\ln(Y_{i,t}) = \alpha_i + \alpha_{c,t} + \beta \cdot Z_i \cdot \mathbb{1}\{t \geq 2013\} + X'_{i,t-1}\gamma + \varepsilon_{i,t} \quad (4)$$

Figure 9: Mean wage of firms depending on their position with respect to median treatment intensity in their matching cell



Note: The y-axis refers to the group average mean firm-level hourly wage, expressed with respect to the pre-reform (2012) level. Bottom and top 50% are defined within cells, where cells 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 MW (both variables are discretized through truncation into 31 values), 3-digit sector and size (4 categories) Source: MVC, DADS 2009-2015.

where Y_{it} is the wage of firms i at time t, Z_i is the predicted reduction in labor costs defined in equation (3), and $\mathbb{1}\{t \geq 2013\}$ is the post treatment indicator variable. The coefficient β can be interpreted as a semi-elasticity of the variable $Y_{i,t}$ relative to the tax credit rate. $X_{i,t-1}$ is a vector of lagged observable controls that will be progressively included in the specification. The vector contains the lagged share of employees paid less than 1.5 MW as another policy targeting these employees was implemented in 2015, the lagged value of assets (in log), and the lagged productivity (in log). We include these controls in order to gauge the robustness of our results to the role of potential confounders but they do not play an important role in the identification of the effect of interest. The term α_i refers to a firm fixed-effect while $\alpha_{c,t}$ is a cell $\times$ year fixed effect defined above. The inclusion of cells $\times$ year fixed effects implies that (1) we are comparing *ex ante* similar firms, as they have a similar pre-reform wage distribution, are in the same industry and the same size group and that (2) the common trend assumption needs only to hold within-cell. Indeed, the interaction of cells indicators with year dummies allows to controls for cell-specific time trends. Indeed, firms with different skill mixes, of different sizes or belonging to different industries can be differently affected by business cycle fluctuations (Moscarini and Postel-Vinay, 2012). This procedure is standard in the literature on firm-level evaluation of tax policy (Yagan, 2015).

Additionally, we implement event-study regressions to check whether the common trend assumption holds. The exogenous treatment intensity is interacted with a full set of year dummies (omitting the pre-reform year):

$$\ln(Y_{i,t}) = \alpha_i + \alpha_{c,t} + \sum_{d \neq 2012} \beta_d \times Z_i \mathbb{1}\{d = t\} + X'_{i,t-1}\gamma + \varepsilon_{i,t} \quad (5)$$

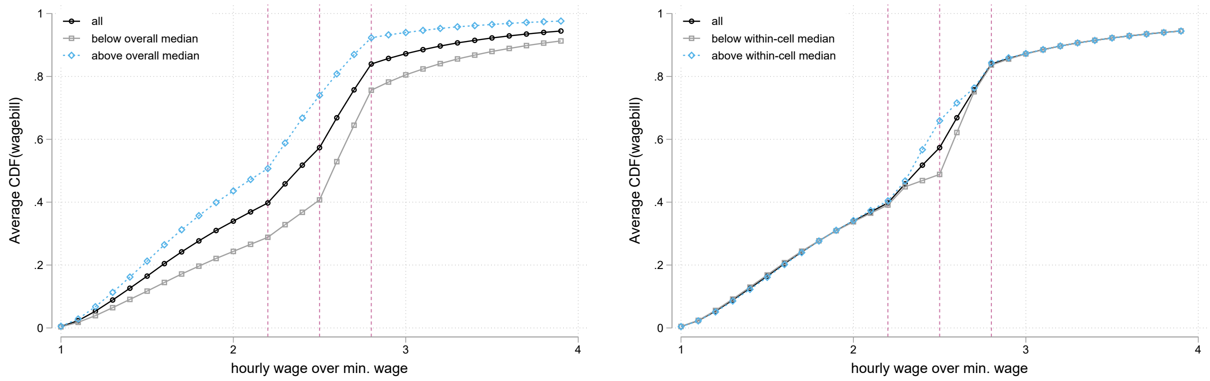
Finding pre-reform β_d coefficients that are not significantly different from zero and follow a stable path would support the common trend assumption. Post-reform β_d coefficients will reflect how the effect of the policy builds up after the reform.

Assessing the validity of the research design

To test how this method matches firms with similar wage structures, we compare the wage distribution of firms more or less intensely treated. We plot the wage distribution of two categories of firms: those with treatment intensity under or above *(i)* the full sample median (Figure 10, left panel) or *(ii)* the within cell median (Figure 10, right panel). On the left panel, the wage distributions outside the 2.2-2.8 MW window are different, with most treated firms counting on average more low-wage employees. On the opposite, the two groups of the right panel have a similar wage distribution outside the 2.2-2.8 MW window, and only differ in wage structure within that window. It therefore demonstrates that our cells fixed effects successfully group firms that have very similar wage distributions even beyond the points on which we match, while exhibiting differences in the repartition of wages immediately around the cutoff, thus creating variation in the treatment intensity.

Our method aims at exploiting variation in treatment intensity that stems from local differences in the wage distribution of firms with otherwise similar production profiles. Hence, conditional on cell fixed-effects, there should be no *ex ante* correlation between treatment and firms' observable characteristics. To test this, we compute these correlations, first unconditionally, then conditioning on sector $\times$ size fixed-effects and, finally, conditioning on cell fixed-effects. They are computed for 2012, the year prior the reform, and for the following firm observables: the value added per worker $(VA/L)_i$, the value of assets ($Assets_i$) and the share of the wage bill below 1.5 MW ($Sh1.5MW_i$). Results are shown in Table 4. While controlling for sector fixed-effects barely affects the correlation between covariates and treatment, the inclusion of cells fixed-effects is, in contrast, very effective at reducing that correlation. It confirms that *ex ante* treatment intensity is as good as random within cells.

Figure 10: Within-firm wage distribution for firms above and below median of treatment intensity where the median is defined globally (left panel) and within cells× sector × size category (right panel)



a. Median defined globally

b. Median within category

Note: The left panel displays the average wage bill CDF of firms depending on whether they lie above or below the median of the treatment intensity where the median is defined for the entire sample. The right panel does the same thing, except that the median is now defined within category (cell × 3-digit industry × size). The sample is here restricted to firms for which more than 30% of 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 4: Within-cell correlation between treatment intensity and covariates

(1)	(2)	(3)	(4)	(5)	(6)
Statistic	Sample	# firms	Uncondit.	Sector × Size FEs	Cells FEs
$\rho(Z_i, \ln Assets_i)$	all	325,329	-0.161	-0.096	-0.003
$\rho(Z_i, \ln(VA_i/L_i))$	all	325,329	-0.336	-0.280	-0.007
$\rho(Z_i, Sh1.5MW_i)$	all	325,329	0.604	0.507	0.000
$\rho(Z_i, \ln Assets_i)$	% WB $\geq$ 30%	30,089	-0.054	-0.023	-0.006
$\rho(Z_i, \ln(VA_i/L_i))$	% WB $\geq$ 30%	30,089	-0.187	-0.181	-0.025
$\rho(Z_i, Sh1.5MW_i)$	% WB $\geq$ 30%	30,089	0.353	0.316	0.011
$\rho(Z_i, \ln Assets_i)$	% WB $\geq$ 50%	8,130	-0.017	-0.033	-0.030
$\rho(Z_i, \ln(VA_i/L_i))$	% WB $\geq$ 50%	8,130	-0.079	-0.097	-0.041
$\rho(Z_i, Sh1.5MW_i)$	% WB $\geq$ 50%	8,130	0.179	0.228	0.019

Note: Cells 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 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 × size FEs. Column (6) shows the correlation between the same variables after absorbing sector × size × cells FEs.

Obviously, a cost of within-cell estimation is that the average difference in *ex ante* exposure to CICE is lower. We check that absorbing cells fixed effects leaves enough treatment intensity variation across firms for several subsamples. Results are presented in Table 5. Although cells do account for a large share of the variation in treatment intensity across firms in the full sample, a substantial share remains within cells for firms with a high share of workers in the window. The standard deviation of treatment intensity is greater than 1% (for a maximum possible treatment intensity of 6%) and within cells variation is substantial: about 50% (respectively 70%) of the variance when restricting to the sample of firms with at least 30% (respectively 50%) of the wage bill between 2.2 and 2.8 MW.

Table 5: Between/within cell variation in treatment

Statistic	Sample	# firms	Std deviation	Between Cells	Within Cells
$\hat{\mathbb{V}}(Z_i)$	all	325,329	0.0094	92.0%	8.0%
$\hat{\mathbb{V}}(Z_i)$	% WB > 0.3	30,089	0.0107	50.2%	49.8%
$\hat{\mathbb{V}}(Z_i)$	% WB > 0.5	8,130	0.0120	27.6%	72.4%

Note: Cells 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 MW (both variables are discretized through truncation into 31 values), the size category and the sector of activity (3-digit). "% 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.

We have so far shown (1) that cells effectively group firms with similar wage distributions outside the 2.2-2.8 MW but with substantial differences in the repartition of wages around this cutoff, (2) that including cells vastly reduces pre-reform correlation with counfounding factors, showing that within-firm variation is as good as random, and (3) that this methodology not only isolates likely random variations, but also preserves substantial within-cell variation in treatment intensity, necessary to identify the wage incidence of the tax credit under study.

It should be noted at this point that given our methodology, our estimates are local as firms with no employee paid between 2.2 and 2.8 MW do not serve for the identification. However, the focus on variation in the exposure to the policy driven by local differences in wage distribution around the discontinuity is necessary to identify the causal impact of the CICE and to obtain consistent estimates.

5.2 Results

In this section, we present the results of the impact of the CICE on wages at the firm-level. They indicate that wages increased relatively more in firms relatively more exposed to the CICE after the implementation of the scheme. It contrasts with the absence of incidence at the employee-level around the 2.5 MW eligibility threshold described previously. We first present the baseline estimation results, which stem from the implementation of our within cell double difference estimation strategy. We then present the results of an event study analysis which aims at assessing the validity of the common trend assumption. We finally show that our results are robust to alternative size of grids, to alternative windows of estimations, to alternative definitions of predicted treatment intensity and to a placebo test.

Baseline results

The results of the impact of the CICE on firm-level wages are presented in Table 6. The dependent variable is the mean hourly wages of full-time workers. The first three columns of the table correspond to the full sample, the next three 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 three columns to the subset of firms with at least 50% of their wage bill in this window. In columns (1), (4) and (7), regressions include years 2009-2015, columns (2), (5) and (8) rely on years 2010-2015 and columns (3), (6) and (9) include lagged time varying controls at the firm-level. Results reported here correspond to cells-length of 3.33 percentage points of wage bill cumulative function (31 points grid). All regressions include cells interacted to years fixed and firm fixed effects.

Table 6: Impact on hourly wage: DiD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.454***	0.455***	0.431***	0.529***	0.515***	0.486***	0.668***	0.663***	0.647***
	(0.0758)	(0.0735)	(0.0687)	(0.0942)	(0.0914)	(0.0854)	(0.137)	(0.133)	(0.124)
Observations	806735	695259	679082	154310	133200	129296	41451	35759	34456
R^2	0.910	0.920	0.925	0.819	0.834	0.845	0.723	0.740	0.760
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)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033	.033	.033	.033
Lagged Controls			$\surd$			$\surd$			$\surd$
# firms	121597	121533	120448	23601	23588	23334	6361	6358	6270

Note: Reported coefficients are for the treatment intensity variable interacted with a post-2013 dummy as presented in Equation (4). Each regression includes lagged controls, firm fixed-effects and a set of cells $\times$ year fixed-effects. Cells are defined as the interaction of di
Cells 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), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null.
Robust standard errors in parentheses (firm-level cluster), * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: DADS, FARE, MVC 2009-2015.

The results point to a positive and significant impact of the CICE on wages at the firm-level. Estimates are stable when the number of years included in the panel changes (column 2), or when lagged observable characteristics at the firm-level are included (column 3). The coefficients also remain significant and close in magnitude when we restrict the estimation sample to firms with a larger share of their workers paid in the 2.2 to 2.8 MW window (respectively 30% and 50% in columns 4 to 9). This suggests that our methodology can successfully isolate variation in treatment intensity that comes from the CICE policy. In our central specification (column 9), we find that exogenously receiving an amount of tax credit equivalent to 1% of the wage bill translates into a 0.65% increase in average wages at the firm level.

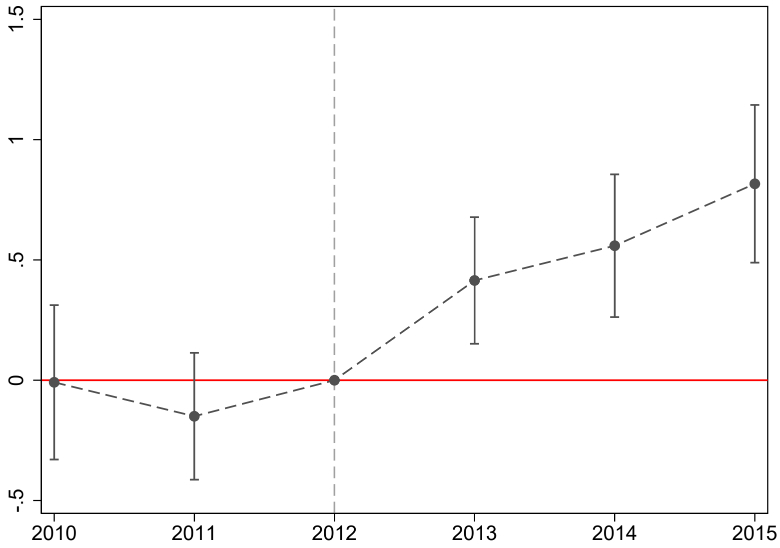
Overall, the magnitude of the coefficients suggests that about half of the CICE was transferred to workers through wage increases. This result is in line with previous estimates in the literature on the incidence of corporate income tax. Liu and Altshuler (2013) study the US case thanks to cross-industries analysis, Arulampalam *et al.* (2012) investigate the European setting thanks to a cross-country panel analysis, Fuest *et al.* (2018) analyze the corporate income tax in Germany, thanks to a cross-municipality panel analysis while Dwenger *et al.* (2011) use a quasi-natural experiment based on heterogeneous impacts of a reform decreasing the CIT rate and broadening the base in Germany. Interestingly, despite obvious differences in the context, design of the policy and method implemented to estimate the effects, all estimates are very close and indicate that about 50% of variation in corporate income tax is transmitted to employees through changes in wages.

Event study analysis

To complement our within cells double difference estimations, we also implement an event study analysis as described in equation (5). It has two purposes: first to assess the validity of the common trend assumption and second to understand how the wage effect of the CICE unfolds over time. This event study, which is presented in Figure 11, mirrors the double difference specification of column (9) in Table 6. It presents the evolution, each year, of the point estimates and the associated confidence intervals (vertical bars) of the difference in mean hourly wages between more or less treated firms. Because our exogenous treatment intensity is time invariant and our specification includes firm fixed effects and time fixed effects, we normalize one of the β coefficients. We choose to set β_{2012} to 0, as it is the last year before the introduction of the CICE. It implies that preceding and subsequent coefficients should be interpreted as a log difference in mean wages across more or less treated firms with respect to the year 2012. As explained above, the common trend assumption is valid only if the pre-reform β coefficients are not significantly different from zero.

Results indicated that there is no difference in wages between more or less treated firms before the introduction of the CICE as pre-reform estimates are not significantly different from zero. This confirms the validity of the common trend assumption on

Figure 11: Effect of the reform on firm-level average wage: central specification



Note: The figure presents point estimates correponding to Equation (5), including lagged controls, and estimated using the sample of firms with more than 50% of their wage bill accruing to workers in the (2.2, 2.8) window. 95% confidence intervals are displayed. 2012, the last pre-reform year, is used as the reference year, and the point estimate is equal to 0 by construction. Regressions include cells × size × sector × year fixed-effects. Cells 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 categories), 3-digit sector and size category (4 values). The lagged controls are the following: the (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. Sources: DADS, FARE, MVC 2009-2015.

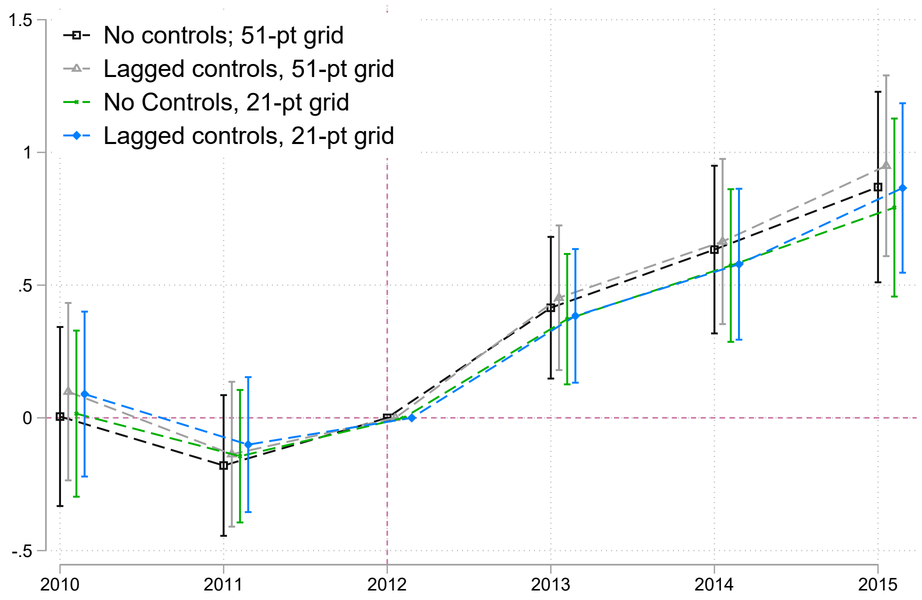
which relies our estimation strategy. Turning to post-reform estimates, coefficients become significantly positive in 2013, and increase in 2014 and 2015, possibly because the rate of tax credit changed from 4% to 6%.

Overall, our baseline results suggest that the CICE has a positive impact on wages at the firm-level and that its effect gradually gained in magnitude. We now turn to an assessment of the robustness of these results.

Alternative size of grids

As explained above, an important novelty of our estimation is strategy is to compare firms within cells. Yet, we made a number of choices to define cells and it is crucial to carry out a sensitivity analysis. A first potential concern relates to the size of grids used to assign group of firms in different cells depending on their wage distribution. To test the robustness of our results to changes in the size of grids, we reproduce our event study regression for larger cell (21 points grid) and smaller cells (51 points grids), including controls or not in the regressions. Such results in the form of event study are presented in panel $a.$ of figure 13 while results of difference in difference estimations with 21 and 51 point grids are presented in the Appendix (see Tables A3 and A4). As expected, pre-reform coefficients remain non significant while wage difference between more or less exposed firms to the CICE become positive and significant after 2013 in all specifications. Our results are therefore robust to alternative size of grids and cells.

Figure 12: Wage impact of the CICE, alternative size of cells



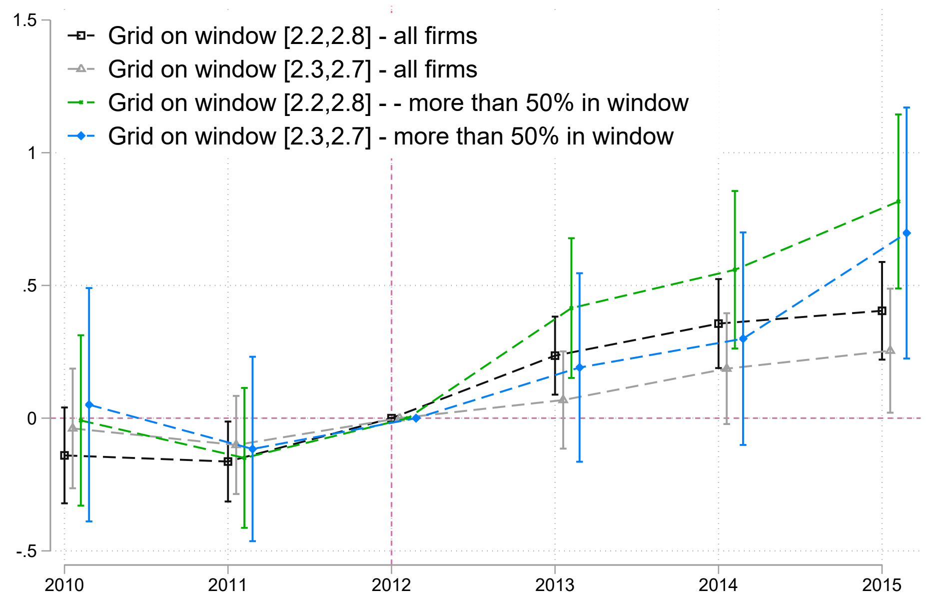
Note: The figure plots coefficients from Equation (5). 95 % confidence interval are displayed. The last pre-reform year, 2012, is used as a reference point. Regressions include cells × 3-digit sector × size (4 categories) × year fixed-effects. Cells are defined as a unique combination of the (coarsened) proportion of wage bill accruing to workers making less than 2.2 and less than 2.8 times the minimum wage. The panel (a) displays results for firms with more than 50% of the wage bill located the (2.2 MW,2.8 MW) window for two different grids used in the discretization of the wage bill share of workers below 2.2 and 2.8 MW and with and without observable controls. The panel (b) displays results for different samples of firms in the wage bill share in the window (see legend) and a tighter window (2.3, 2.7) around around the 2.5 MW threshold. *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. Sources: DADS, FARE, MVC 2009-2015.

Alternative window of estimations

A second potential concern lies in the choice of estimation windows. We therefore test alternative specifications regarding the width of the window around the 2.5 MW threshold: we compare point estimates when cells and estimations windows are defined with respect to 2.2-2.8 MW, or with respect to 2.3-2.7 MW. We also impose or not sample restrictions, by using either all firms, or only those with at least 50% of their wage bill comprised in these respective windows. The results are presented in panel *b.* of figure 13 while results of difference in difference estimations are presented in the

Appendix (see Tables A5, A6 and A7). Pre-reform coefficients remain not significantly different from zero, and post reform coefficients remain overall significant with magnitudes similar to the ones of the baseline specifications. These tests therefore confirm the robustness of our results to changes in the window of estimation and to sample restrictions.

Figure 13: Wage impact of the CICE, alternative estimation windows

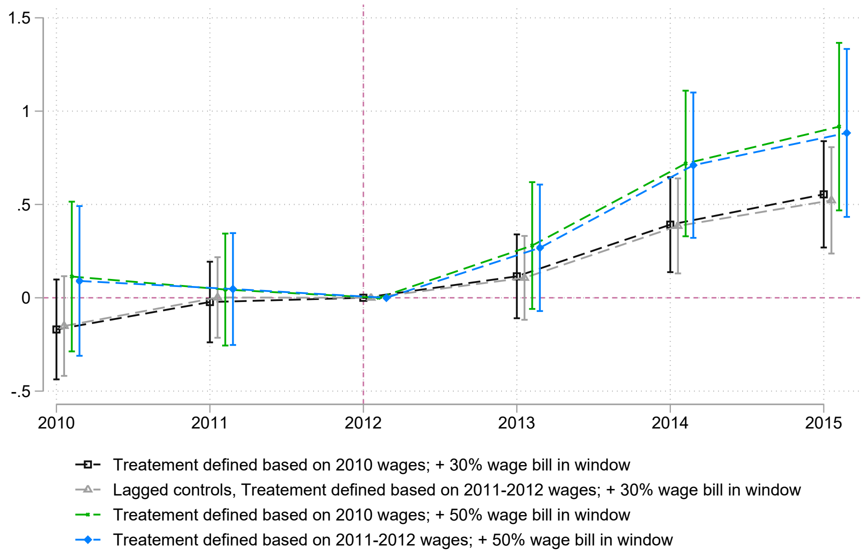


Note: The figure plots coefficients from Equation (5). 95 % confidence interval are displayed. The last pre-reform year, 2012, is used as a reference point. Regressions include cells × 3-digit sector × size (4 categories) × year fixed-effects. Cells are defined as a unique combination of the (coarsened) proportion of wage bill accruing to workers making less than 2.2 and less than 2.8 times the minimum wage. The panel (a) displays results for firms with more than 50% of the wage bill located the (2.2 MW,2.8 MW) window for two different grids used in the discretization of the wage bill share of workers below 2.2 and 2.8 MW and with and without observable controls. The panel (b) displays results for different samples of firms in the wage bill share in the window (see legend) and a tighter window (2.3, 2.7) around around the 2.5 MW threshold. *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. Sources: DADS, FARE, MVC 2009-2015.

Treatment intensity based on other years

A third potential concern lies in the year of reference to compute exogenous predicted treatment intensity. Indeed, as explained *supra*, we initially defined it as the amount of tax credit a firm would have been entitled to claim given its pre-reform wage bill. In other words, we compute the ratio of the sum of wages below 2.5 MW to the total wage bill in 2012, and scale it given the policy rate. However, it could be that this year was rather atypical (Europe was in the middle of the Debt Crisis), and hardly reflected true firms' characteristics. In alternative definitions, we construct the exogenous treatment based on the 2010 wage bill, as well as a mean of 2011 and 2012 wage bills. Event-study estimation results are presented in figure 14 while difference-in-difference estimation results can be found in tables A8 and A9.

Figure 14: Effect of the reform on firm-level average wage for alternative treatment instruments



Note: The figure plots coefficients from Equation (5) when the *ex ante* exposure to the policy is computed based on different base years: 2010, or an average of 2010 to 2012 in instead of 2012 in the baseline specification. 95 % confidence interval are displayed. The last pre-reform year, 2012, is used as a reference point. Regressions include cells × size (4 categories) × 3-digit sector × year fixed-effects. Cells are defined as a unique combination of the coarsened proportion of wage bill accruing to workers making less than 2.2 and less than 2.8 times the minimum wage. *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. Sources: DADS, FARE, MVC 2009-2015.

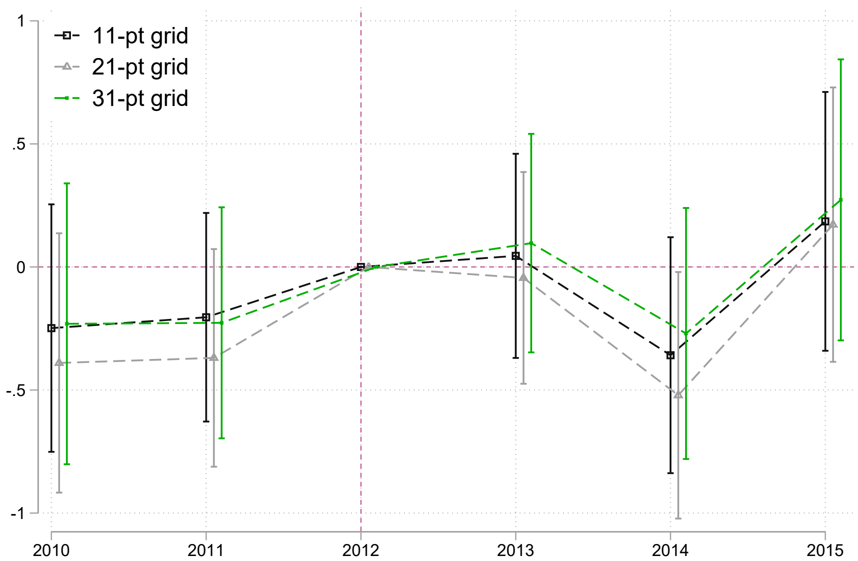
Reassuringly, pre-reform coefficients remain non significant. Post reform coefficients are not significant in 2013 but remain positive and significant in 2014 and 2015 when the rate of tax credit increases from 4% to 6%.

Overall all these robustness checks confirm that the CICE has had a positive impact on wages at the firm level and that our results are not sensitive to the definition of cells, window of estimations, sample restrictions, and definition of treatment intensity. A remaining question is whether our results are affected by confounding factors.

Placebo test

In order to further test the robustness of our results against the presence of potential confounding factors, we finally implement a placebo test. It consists in replacing the actual eligibility threshold at 2.5 MW by a virtual one at 3.1 MW, and by comparing firms just above and just below this threshold in the 2.8-3.4 window. The rational for choosing this cutoff is to be sufficiently low so that there remains local variation in wages between firms around the threshold while not interfering with our previous estimation window. If the incidence on wages highlighted previously is caused only by the introduction of the CICE, there should be no significant difference in the evolution of wages between firms in these two groups after 2013.

Figure 15: Effect of the reform on firm-level average wage: placebo test



Note: Placebo treatment is defined as an ex-ante exposure to policy based on 2012 wage distribution as if the eligibility threshold was 3.1 MW instead of 2.5 and the cells are defined as a unique combination of the proportion of wage bill accruing to workers making less than 2.8 and less than 3.4 times the minimum wage (both variables are coarsened through truncation into 11, 21 or 31 values). As in other tables, regressions include cells × 3-digit sector × size (4 categories) × year fixed-effects. As there is no policy matching the threshold one would expect no effect. Results are displayed for different fineness of the grid used for the discretization of the wage bill share of workers below 2.8 and 3.4 MW. Estimates based on the sample if firms with at leasr 50% of their wage bill accruing to workers in the window (2.8,3.4 MW) window. Sources: DADS, FARE, MVC 2009-2015.

Results indicate that in this case, coefficients are not different from zero pre-reform, but more importantly, not different reform post reform. This suggests that all the effect we capture is well link to the differences in wages distribution around the 2.5 MW. Given that this threshold is specific to the CICE, this considerably reduces the possibility that our results be biased by confounding factors.

Overall, our results so far suggest that the absence of incidence at the employee level according to the individual eligibility status hides a substantial collective incidence at the firm -level. This result meets those found by Saez *et al.* (2018) in the context of Sweden, where payroll tax cuts on young workers seem to have induced wage increase for all workers independently from their eligibility status. Note that such type of incidence would not be detectable with an employee-level analysis which suggests the importance of methods on the assessment of incidence.

5.3 Heterogenous impact of CICE on wages

In the previous section, we have documented that about half of the corporate income tax credit presently studied translated into higher wages, in line with the corporate income tax incidence literature. However, this positive average effect on wage may hide heterogeneity across types of firms or workers. First, incidence on wages may depend on firm characteristics: marginal rent sharing should depend on the amount of surplus firms can actually share with their employees. We therefore test the impact of *ex ante* profitability. Moreover, the incidence on wages is likely to vary according to employees characteristics, particularly their bargaining power. We therefore test whether wage incidence differs by socioprofessional category.

Effect of the CICE on wages according to firm profitability

Wage incidence of the tax credit is likely to depend on a firm's profitability. As wage incidence results from a sharing of the policy benefit (or burden) with the employees, less profitable firms might be less keen on sharing the tax credit with their employees, if for instance, they are more in need to restore their margin. To test this hypothesis, we interact our treatment intensity with a dummy variable indicating whether a firm is profitable or not. Firms are considered profitable when they have strictly positive taxable profits in the three pre-reform years. The results are presented in Table 7 for different restrictions in the estimation sample.

As expected, results indicate that profitability indeed plays a role, as wage incidence is significantly larger in more profitable firms. However, though lower, the wage incidence also occured in non-profitable firms.

Impact of the CICE on wages according to the CSP

Previous results suggests that firms share the tax credit with their employees independently from their individual eligibility. Nevertheless, wage incidence may vary according to employees characteristics, in particular their bargaining power. To test this hypothesis we carry out the estimations of the CICE impact at firm-level separately for high skill and low skill workers, making the assumption that they exhibit different bargaining power. In order to do so, we split the French scale of socioprofessional category – informed in the DADS database – between two groups: low skill and high skill. High skill corresponds to executives, intellectual and intermediate occupations (skill levels 3 and 4 of the ILO classification). Low skill correspond blue

Table 7: Impact of CICE on wages, impact of firm profitability

	Share of wage bill in the estimation window [2.2,2.8]		
	all firms	> 30%	> 50%
Impact of the CICE policy for non-profitable firms	0.273***	0.191**	0.299**
	(0.067)	(0.090)	(0.143)
Additional impact for profitable firms	0.061***	0.143***	0.149***
	(0.005)	(0.016)	(0.035)
Observations	737,156	140,930	36,559
R^2	0.924	0.846	0.756

Note: Reported coefficients are for the treatment intensity variable interacted with a post-2013 dummy as presented in Equation (4). Each regression includes lagged controls, firm fixed-effects and a set of cells × year fixed-effects. Cells are defined as the interaction of di
Cells 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), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null.
Robust standard errors in parentheses (firm-level cluster), * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: DADS, FARE, MVC 2009-2015.

collar and other employees (skill levels 1 and 2 of the ILO classification). The results are displayed in Table 8 for the baseline specification.

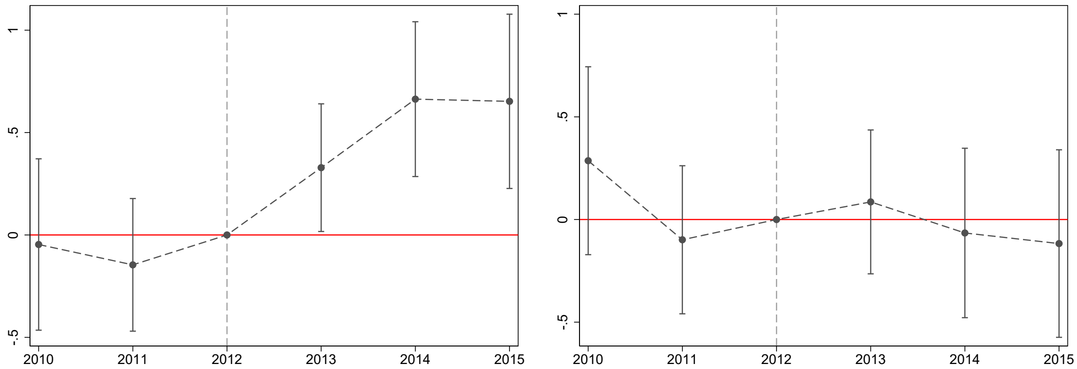
Table 8: Impact of CICE on wages by skill level, DiD

	Share of wage bill in the estimation window [2.2,2.8]					
	all firms	> 30%	> 50%	all firms	> 30%	> 50%
	Low skill workers			High skill workers		
CICE policy	-0.047	-0.048	-0.087	0.437***	0.420***	0.605***
	(0.078)	(0.103)	(0.163)	(0.102)	(0.118)	(0.157)
Observations	616,093	99,010	23,379	528,764	91,246	23,245
R^2	0.888	0.870	0.869	0.866	0.826	0.765

Note: Reported coefficients are for the treatment intensity variable interacted with a post-2013 dummy as presented in Equation (4). Each regression includes lagged controls, firm fixed-effects and a set of cells × year fixed-effects. Cells are defined as the interaction of di
Cells 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), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null.
Robust standard errors in parentheses (firm-level cluster), * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: DADS, FARE, MVC 2009-2015.

This analysis provides clear-cut results: while coefficients for low-skill workers are close to zero and non significant, the wage incidence for high-skill workers is strongly significant. As previously, we also run event studies in order to assess the validity of the common trends assumption and to uncover post reform dynamics. Figure 16 shows the baseline specification.

Figure 16: Impact of CICE on wages by skill level, event study



a. White collars *b.* Blue collars

Note: The figure presents point estimates correponding to Equation (5), including lagged controls, and estimated using the sample of firms with more than 50% of their wage bill accruing to workers in the (2.2, 2.8) window. 95% confidence intervals are displayed. 2012, the last pre-reform year, is used as the reference year, and the point estimate is equal to 0 by construction. Regressions include cells × size × sector × year fixed-effects. Cells 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 categories), 3-digit sector and size category (4 values). The lagged controls are the following: the (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. Sources: DADS, FARE, MVC 2009-2015.

These event studies confirm the robustness of the results: the pre-reform point estimates are not significant and close to zero for the two groups, which validates the common trend assumption. Then, the post-reform point estimates stay close to zero and are not significant for low-skill but grow to a significantly positive level for high-skill. The CICE translated in wages increases only for high-skill workers. This pattern is consistent with the surplus-sharing hypothesis, as employees with a higher socio-professionnal status are likely to have more bargaining power, allowing them to get a larger share of the surplus. This meets the results of Kline *et al.* (2018) using a different shock in the rent to share: they show that the incidence of patent-induced surplus on wages are concentrated among workers at the top half of the earnings distribution.

Interestingly, high-skill worker are less likely than low-skill to contribute to the eligibility of the firm to the CICE as they are less likely to be paid below 2.5 MW (see Figure 2). These findings suggest that there was sizable spillovers of the policy from the eligible workers to those not necessarily eligible due to rent sharing at the firm level. They also indicate that the CICE policy has had in the French case stark anti redistributive effect as the tax credit targeted the lowest part of the wage earners but benefited to employees earning higher wages.

6 Conclusion

In this paper, we take advantage of a large corporate tax credit (CICE), introduced in France in 2013, to shed new light on the wage incidence of corporate taxation. The CICE is proportional to the wage bill of workers paid below a hourly wage threshold (2.5MW). This induces a discontinuity in mandatory levies at the employee level. Hence, we first implement an employee-level identification strategy to assess the wage incidence of the CICE. In addition, the scheme generates substantial difference in treatment intensity between very similar firms, depending on the number of employees just above or just under the 2.5MW threshold. Hence, we also develop an identification strategy at the firm level based on treatment discontinuity at the employee level, to asses firm-level wage incidence of the CICE. We are therefore able to contrast wage incidence at employee and firm levels for the same reform – in the same economic and institutional environment – on the same economic agents.

Our results demonstrate the absence of wage incidence at the employee level contrasting with a substantial wage incidence at the firm level (about 50% of the tax credit is shifted into wages). This means that the corporate tax credit is actually shared with the employees, independently from their individual eligibility. This highlights that individual strategies of identification do not provide the comprehensive picture of wage incidence, as collective rent-sharing cannot be detected.

Though collective, wage incidence may be heterogeneous between groups of workers. We actually demonstrate that the CICE wage incidence falls only on high skill workers. Low skill workers, despite more likely to be eligible, did not benefit from wage increases caused by the CICE. This highlights the anti-redistributive impact of this corporate tax credit.

Both these results highlight the importance of within firm wage bargaining for assessing corporate tax incidence and its distributional aftermath. This adds another motivation for further analyzing the impact of workers' characteristics (such as scarcity or substitutability of the skills) or labor market institutions (such as the level of decentralization or the degree of unionization) on wage setting.

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A Data appendix

A.1 Details on databases

The present empirical analysis is based on three administrative databases, built from firm returns to the tax agency (DGFiP, the French General Directorate for Public Finance) and from firm returns to the institution responsible to collect social contribution (ACOSS). Furthermore, the DGFiP has computed since the reform a firm-level database informing the amount and use of the CICE (MVC database). It also provides, in association with the French statistical agency (INSEE) a database on firms accounting (FARE database). ACOSS provides in association with Insee a database on workers and wages at the contract level (DADS database). We were given access to these databases for the years 2009 to 2015.

A.1.1 MVC database

DGFiP specifically built the MVC files informing the firms' initializations of CICE rights. This database, whose first vintage is 2013, gathers information for all firms benefiting from the CICE. For each firm, details are provided regarding: *initialization*, the amount of tax credit to which the firm is entitled, initialized on its tax returns; *increase*, upward adjustments given the evolution of the firm's wage structure; *decrease*, similar downward adjustments; *imputation*, the amount of CICE that firms were able to deduct from their CIT. These variables allow us to understand the CICE distribution. For a given year, the eventual amount of CICE claimed by a firm for a given year is equal to *initialization* plus *increase* minus *decrease*. We do not use the actual year of *imputation*.

A.1.2 FARE database

General information on the production structure of firms, income statement and balance-sheet are presented in the FARE database of the ESANE system (annual business statistics). It is built by Insee on the basis of the tax data, social declarations and a survey[19]. This database covers all firms (including firms without employees) with the exception of the financial sector and farms. We use it to control for firm characteristics, in terms of workforce, productivity (value added per employee) and capital stock (tangible and intangible assets).[20]

[19] The purpose of the survey questionnaire is to produce structural business statistics; it should be noted that the questionnaire sent to firms was amended in 2011. However, the variables we use are extracted from the tax data

[20] Note that information on profits is also available but is not sufficiently reliable to be used as a dependent variable because firms can account differently for CICE (as a deduction of labor costs, as

While all other databases are defined at the firm-level (with the SIREN number identifying them), the FARE files compute combinations for some of them, which is called profiling. Indeed, some major groups have transformed parts of their production chain into independent legal units, while decisions remain at the central level. In order to provide a better overview of the productive structure, Insee gathers different legal units (with different SIREN numbers) into a single entity – profiled firm. Among the profiled firms, six are present in the database only at the group level (the six historical profiled firms) while the data on the others are given at both the group and legal unit levels (a hundred of profiled firms). For those latter firms we consider only the individual legal units for all databases. For the six historical profiled firms, we profile the other databases in order to have employement, wages and CICE data at the group level and be able to match the databases.

A.1.3 DADS database

The annual social data declaration (DADS) files contain information on each salaried contract in each firm: net and gross wages, working time, socioprofessional categories, type of contract, sex of the employee, etc. There is one observation per contract for each firm and employee. Thus, the same employee can be found several times in the dataset if she has contracts with several firms. In addition, it is important to know that DADS are presented in the form of regional files and that observations concerning employees of a firm located in one region but living in another one are present in the regional files of the two regions. A first work before starting the analysis therefore consisted in excluding double counts from these databases.

Moreover, for each item, the values of the variables are also given for the previous year. This makes it possible to construct changes in the variables from one year to the next for each item. Indeed, the identifiers of the contracts are not recognizable from one vintage to the other and it is therefore not possible to build a panel of contracts. On the other hand, the firm identifiers are the SIRENs, stable over time, and we therefore constitute panels of firms. Thus, as far as wage increases are concerned, we have operated in two ways, depending on the level of the analysis. At the employee-level, we consider the distribution of wages of new hires on the one hand and the growth rate of the hourly wage for employees with a permanent contracts staying in the same firm two consecutive years on the other hand. At the firm-level, in order to capture the changes due to wages variations only – and not due to variation in the structure of employment in the firm – we have calculated mean wage of full time permanent contracts only.

operating subsidies, other operating income or CIT deduction). Thus, the various measures of profit (EBIT, EBITDA, operating income) may or may not take into account the CICE depending on the firm.

Pay data is accurate in that it is at the job level, but relatively imprecise as to what it covers. Gross remuneration includes "all remuneration received by the employee under her contract of employment, before deducting compulsory contributions". For instance, it includes bonuses for end of fixed-term contracts (corresponding to 10% of the amount received during the contract). This could have led to biases in the observation of hourly wage growth since these bonuses inflate the total gross earnings for the contract year but not the number of hours worked. To tackle this, we focus in the empirical analysis on the mean wage of permanent contracts only. Moreover, we are able to predict very precisely the CICE firms can claim, meaning that these data are very reliable for the purpose of our study.

In order to carry out our identification strategy, it is necessary to be able to measure the CICE to which a firm would have been entitled before the reform implementation, based on its productive structure. However, this is not possible on the basis of actual tax data, which did not collect such information prior to 2013. The DADS database allows us to approach these values thanks to the precision on wage structure of firms. It is indeed possible to calculate the share of wage bill below 2.5 MW, and to compute a potential CICE.[21] This calculation for the years 2013 to 2015 of predicted CICE (given past wage structure) is very close to the amounts of CICE actually initialized with the tax departments and presented in the MVC database.

A.2 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. **Stayers**: Stayers 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

[21] Similar wage structure indicators can also be built at other thresholds (1.5 MW in particular) in order to better approximate the distribution of wage in the firms.

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).

A.3 Descriptive statistics of the subsamples

Table A1: Subsamples of analysis (2012)

	All	$> 30\%$	$> 50\%$
Employment and wage			
# of employees	64.36	25.36	10.67
# of white-collars	12.52	11.12	4.21
# of blue-collars	34.01	9.04	3.98
Mean wage	16.03	19.65	20.26
Mean wage white-collars	22.33	22.73	22.38
Mean wage blue-collars	13.75	16.32	16.96
Wage stucture			
Wabe bill (in k €)	876.8	590.5	214.2
Share of $<$1.6MW in WB	0.56	0.24	0.18
Share of $<$2.5MW in WB	0.83	0.69	0.67
Firm performance			
Sales (in k €)	6463	3876	1244
EBIT (in k €)	234	245	67
VA per worker (in k €)	70.1	85.8	96
Assets (in k €)	3378.3	2902.9	538.2
Profitability (EBIT/sales)	3.618 %	6.326 %	5.376 %
Industries			
Manufacturing	0.15	0.11	0.09
Construction	0.17	0.18	0.18
Retail	0.42	0.32	0.31
Services	0.18	0.33	0.37
Other	0.04	0.02	0.02
Observations	328693	30258	8172

Note: 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 : in Column (1) regarding all firms, in Column (2) regarding firms whose wage bill share exceeds 30%, in Column (3) regarding firms whose wage bill share exceeds 50%.

B Robustness

B.1 Employee-level incidence

B.1.1 Distribution of new hires' wage

Table A2: Detection of bunching of new hires before the threshold of CICE, [2.3, 2.5]

	Before the CICE				After the CICE		
	2009	2010	2011	2012	2013	2014	2015
$[2.0, 2.5]$	-0.004	-0.03	-0.03	1.76	-1.95	0.54	1.21
	(0.65)	(0.68)	(0.68)	(0.82)	(0.54)	(0.86)	(0.77)
$[2.1, 2.5]$	0.02	0.29	-0.03	0.75	-0.32	1.32	0.44
	(0.47)	(0.57)	(0.68)	(0.53)	(0.45)	(0.70)	(0.67)
$[2.2, 2.5]$	-0.41	-0.38	-0.03	0.08	-0.07	-0.08	0.37
	(0.38)	(0.38)	(0.68)	(0.35)	(0.37)	(0.51)	(0.39)
$[2.3, 2.5]$	0.39	-0.16	-0.03	0.48	0.10	0.01	0.02
	(0.30)	(0.26)	(0.68)	(0.30)	(0.27)	(0.32)	(0.28)
$[2.4, 2.5]$	-0.05	-0.11	-0.03	0.08	0.04	0.004	-0.10
	(0.18)	(0.19)	(0.68)	(0.17)	(0.16)	(0.19)	(0.19)

Note: New hires are defined as workers whose contract starts during the year and that were not previously employed in the same firm. *Sample restrictions*: The sample is restricted to employees whose contract starts during the year and that were not previously employed in the same firm. We further restrict the analysis to employers included in the firm-level estimation sample. Sources: DADS 2009-2015. *: p<0.05; **: p<0.01; ***: p<0.001.

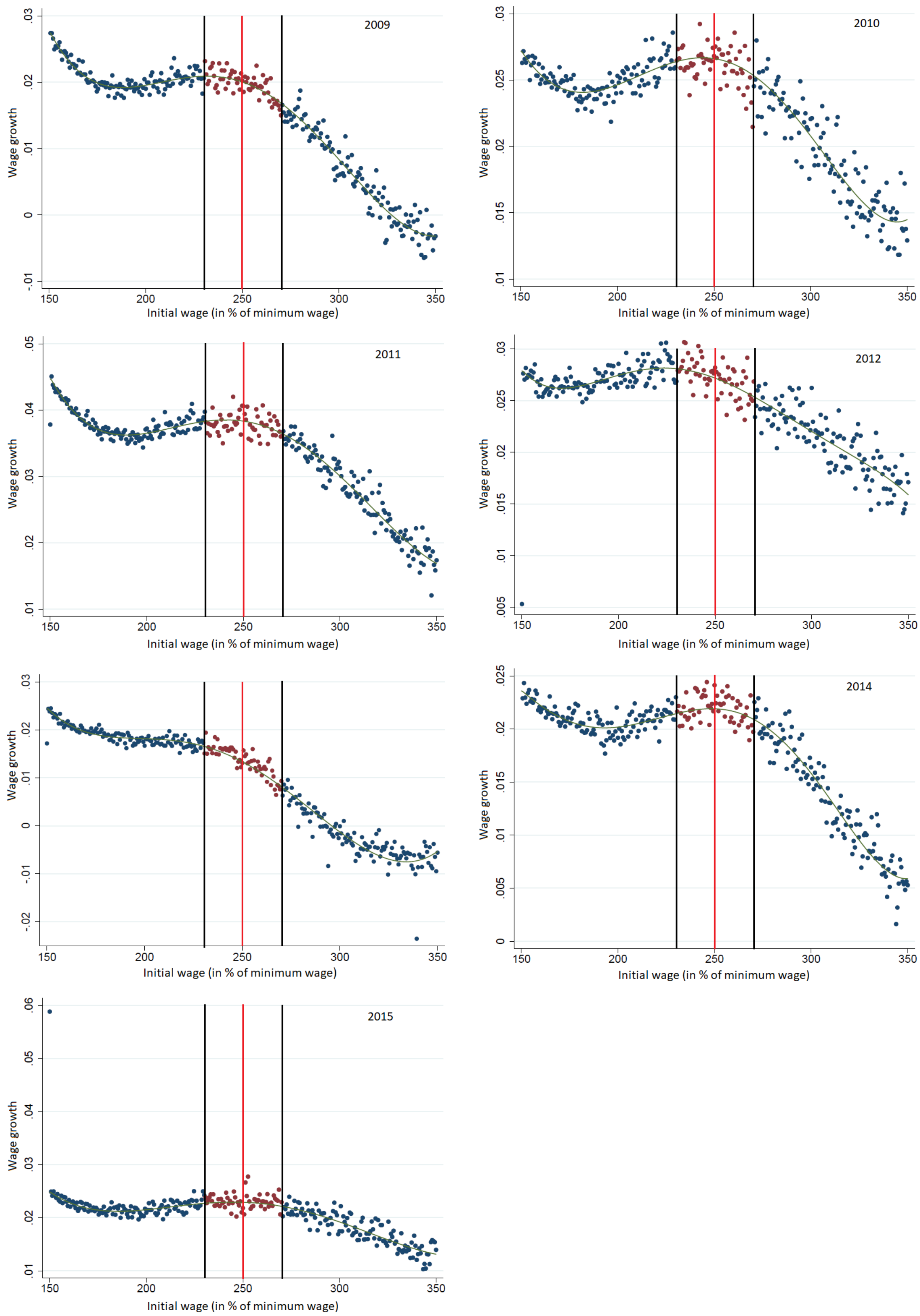
B.1.2 Distribution of continuing employees' wage growth

B.2 Firm-level incidence

B.2.1 Grid definitions

B.2.2 Window definitions

Figure A1: Distribution of wage growth of stayers



Note: Stayers are defined as workers in permanent contract working full-time (32 hrs per week or more) who kept the same occupation within the same firm between t and $t-1$. *Sample restriction*: The sample is restricted employees in permanent contracts from year t to $t-1$ who stay within the same employer. We further restrict the analysis to employers included in the firm-level estimation sample. Bins are defined as intervals of equal length. The length is set so that there are 150 bins over the $[150\%, 350\%]$ interval. Source: DADS 2009-2015.

Table A3: Impact on hourly wages, DiD

Finer grid, 2 percentage points cell width

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.466***	0.467***	0.448***	0.523***	0.515***	0.500***	0.673***	0.695***	0.700***
	(0.0816)	(0.0791)	(0.0739)	(0.0986)	(0.0957)	(0.0893)	(0.142)	(0.138)	(0.129)
Observations	688819	593866	580146	134879	116471	113027	37572	32418	31285
R^2	0.911	0.920	0.926	0.812	0.827	0.838	0.718	0.735	0.756
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)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.02	.02	.02	.02	.02	.02	.02	.02	.02
Lagged Controls			√			√			√
# firms	104147	104093	103170	20652	20643	20414	5773	5770	5693

Note: Each regression includes a set of cells × year fixed-effects. Cells 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 51 values), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null. Robust standard errors in parentheses (firm-level cluster). Sources: DADS, FARE, MVC 2009-2015. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Impact on hourly wages, DiD

Larger grid, 5 percentage points cell width

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.383***	0.386***	0.362***	0.477***	0.470***	0.440***	0.605***	0.619***	0.612***
	(0.0717)	(0.0696)	(0.0650)	(0.0901)	(0.0875)	(0.0816)	(0.131)	(0.127)	(0.119)
Observations	874353	753350	735918	167522	144589	140358	44476	38379	37024
R^2	0.908	0.917	0.923	0.819	0.834	0.846	0.722	0.740	0.761
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)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.05	.05	.05	.05	.05	.05	.05	.05	.05
Lagged Controls			√			√			√
# firms	131481	131416	130252	25560	25548	25268	6819	6816	6722

Note: Each regression includes a set of cells × year fixed-effects. Cells 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 21 values), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null. Robust standard errors in parentheses (firm-level cluster). Sources: DADS, FARE, MVC 2009-2015. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Impact on hourly wages, DiD

2.3-2.7 MW window, 3.33 percentage points cell width

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.278***	0.248***	0.215**	0.339***	0.282**	0.241**	0.489***	0.455**	0.413**
	(0.0945)	(0.0914)	(0.0855)	(0.123)	(0.119)	(0.111)	(0.185)	(0.179)	(0.167)
Observations	648587	558828	545529	83386	71972	69816	21289	18349	17714
R^2	0.917	0.925	0.931	0.803	0.818	0.830	0.715	0.734	0.756
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)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.033	.033	.033	.033	.033	.033	.033	.033	.033
Lagged Controls			√			√			√
# firms	97802	97751	96852	12784	12777	12642	3274	3273	3234

Note: Each regression includes a set of cells × year fixed-effects. Cells are defined as a unique combination of the proportion of wage bill accruing to workers making less than 2.3 and less than 2.7 times the minimum wage (both variables are discretized through truncation into 31 values), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null. Robust standard errors in parentheses (firm-level cluster). Sources: DADS, FARE, MVC 2009-2015. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Impact on hourly wages, DiD

2.3-2.7 MW window, 5 percentage points cell width

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.274***	0.248**	0.211**	0.311**	0.280**	0.237**	0.467**	0.475**	0.459**
	(0.103)	(0.0999)	(0.0937)	(0.130)	(0.126)	(0.118)	(0.194)	(0.189)	(0.179)
Observations	547399	471904	460855	72956	63030	61248	19143	16516	15992
R^2	0.918	0.927	0.932	0.797	0.813	0.825	0.725	0.744	0.764
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)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.02	.02	.02	.02	.02	.02	.02	.02	.02
Lagged Controls			√			√			√
# firms	82826	82793	82066	11196	11193	11078	2948	2947	2914

Note: Each regression includes a set of cells × year fixed-effects. Cells are defined as a unique combination of the proportion of wage bill accruing to workers making less than 2.3 and less than 2.7 times the minimum wage (both variables are discretized through truncation into 21 values), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null. Robust standard errors in parentheses (firm-level cluster). Sources: DADS, FARE, MVC 2009-2015. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Impact on hourly wages, DiD

2.3-2.7 MW window, 2 percentage points cell width

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.251***	0.233***	0.192**	0.320***	0.287**	0.247**	0.477***	0.462***	0.428***
	(0.0896)	(0.0868)	(0.0810)	(0.119)	(0.115)	(0.107)	(0.180)	(0.174)	(0.162)
Observations	707151	609167	594722	90125	77803	75500	22723	19591	18930
R^2	0.913	0.922	0.928	0.801	0.817	0.829	0.709	0.729	0.751
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)	(2.3 ,2.7)	(2.3 ,2.7)	(2.3 ,2.7)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.05	.05	.05	.05	.05	.05	.05	.05	.05
Lagged Controls			$\surd$			$\surd$			$\surd$
# firms	106369	106317	105319	13794	13786	13630	3491	3490	3448

Note: Each regression includes a set of cells $\times$ year fixed-effects. Cells are defined as a unique combination of the proportion of wage bill accruing to workers making less than 2.3 and less than 2.7 times the minimum wage (both variables are discretized through truncation into 51 values), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null. Robust standard errors in parentheses (firm-level cluster). Sources: DADS, FARE, MVC 2009-2015. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.2.3 Treatment intensity based on other years

Table A8: Impact on hourly wages, DiD

Treatment intensity based on 2010 wagebill

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.322***	0.286***	0.254***	0.458***	0.443***	0.385***	0.584***	0.611***	0.575***
	(0.0851)	(0.0825)	(0.0773)	(0.115)	(0.112)	(0.104)	(0.174)	(0.170)	(0.161)
Observations	1066452	919002	898334	146061	126056	122347	33019	28501	27518
R^2	0.906	0.916	0.922	0.812	0.829	0.841	0.717	0.738	0.759
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)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.05	.05	.05	.05	.05	.05	.05	.05	.05
Lagged Controls			√			√			√
# firms	160543	160462	159121	22316	22305	22066	5076	5074	5011

Note: Reported coefficients are for the treatment intensity variable interacted with a post-2013 dummy as presented in Equation (4). Each regression includes lagged controls, firm fixed-effects and a set of cells $\times$ year fixed-effects. Cells are defined as the interaction of di
Cells 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), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null.
Robust standard errors in parentheses (firm-level cluster), * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: DADS, FARE, MVC 2009-2015.

Table A9: Impact on hourly wages, DiD

Treatment intensity based on 2011 and 2012 wagebills

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z_i \times \mathbb{1}\{t \geq 2013\}$	0.351***	0.316***	0.286***	0.487***	0.472***	0.414***	0.616***	0.638***	0.586***
	(0.0848)	(0.0822)	(0.0772)	(0.114)	(0.111)	(0.104)	(0.173)	(0.169)	(0.160)
Observations	1066532	919067	898248	145741	125785	122056	32892	28390	27416
R^2	0.906	0.916	0.922	0.812	0.829	0.841	0.715	0.737	0.758
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)	(2.2 ,2.8)	(2.2 ,2.8)	(2.2 ,2.8)
% WB in window	0	0	0	0.3	0.3	0.3	0.5	0.5	0.5
Width Cells	.05	.05	.05	.05	.05	.05	.05	.05	.05
Lagged Controls			$\surd$			$\surd$			$\surd$
# firms	160547	160460	159102	22263	22254	22009	5059	5057	4997

Note: Reported coefficients are for the treatment intensity variable interacted with a post-2013 dummy as presented in Equation (4). Each regression includes lagged controls, firm fixed-effects and a set of cells $\times$ year fixed-effects. Cells are defined as the interaction of di
Cells 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), 3-digit sector and size (4 categories). *Lagged controls* includes (log of) value-added per workers, asset value and the 2012 share of wage-bill accrued to workers earning below 1.5 times the minimum wage interacted with a full set of year dummies. The baseline sample includes firms in the for-profit sector present in FARE, DADS and MVC files that are observed for the entire period (balanced sample). The number of observations do not include firms in cells where the variation of the treatment is null.
Robust standard errors in parentheses (firm-level cluster), * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sources: DADS, FARE, MVC 2009-2015.

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