Incidence of corporate tax credit on profits, wages and employment: evidence from a French reform[1]

First draft: February 2017
PRELIMINARY

Clément Carbonnier
Université de Cergy-Pontoise, THEMA

Clément Malgouyres
Banque de France

Loriane Py
Banque de France

Camille Urvoy
Sciences Po

Abstract

This paper exploits a far-reaching French reform as well as a very rich set of administrative data to evaluate the impact of a corporate tax credit aimed at reducing labor costs on several outcomes: employment, profit and wages. The effects of the Competitiveness and Employment Tax Credit (CETC), a refundable tax credit based on the wagebill, introduced in France in 2013, are estimated thanks to double (and triple) difference methodologies, instrumented by the intensity of the intention to treat, thanks to data at the firm and individual levels on the period 2010-2014. Our results show that this relatively large tax break - about 17 billion euros per year - does not succeed in boosting employment in the first two years after being set. However they suggest that firms used the CETC to restore their margins. Moreover wages have increased significantly in more intensively treated firms, particularly those of white-collar employees. These results cast doubts regarding the effectiveness of such tax credits to boost employment. More importantly, they also provide new quasi-experimental evidence regarding rent sharing in the labor market.

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

[1]We would like to thank institutions that built the databases (Insee, DGFiP, ACOSS, Douanes) and made them available for research on the CASD, and particularly their agents who diligently answered our questions about their content. We also thank Marc Ferracci, Sara Guillou, Yannick L'Horty, Etienne Lehmann, Benoit Ourliac, Bruno Palier, Etienne Wasmer for their comments in *ad hoc* meeting organized by *France Strategy* or in academic seminars. This work is supported by public grants overseen by the French National Research Agency (ANR) as part of the "Investissements d'avenir" program within the frameworks of the *Centre d'accès sécurisé aux données* - CASD (ANR-10-EQPX-17) and the LIEPP center of excellence (ANR-11-LABX-0091, ANR-11-IDEX-0005-02).
Researchers from the Banque de France participated to this work, the views expressed in this paper are their own and do not necessarily reflect those of the Banque de France or the Eurosystem.

1 Introduction

In the context of a globalized economy in which many developed countries face sluggish economic growth and low employment rates, governments have sought to increase firm competitiveness and boost economic activity by implementing policies aiming at reducing labor costs. In France, between 1993 and 2004 a series of direct payroll tax cuts have been implemented, targeting the bottom of the wage distribution, partly in order to offset the impact of the minimum wage on labor cost (Bunnel and L'Horty, 2012). In 2013, this set of policies was complemented by a large corporate income tax (CIT) credit whose amount is proportional to the wage bill. This policy, called the Competitiveness and Employment Tax Credit (CETC) was conceived as a mere continuation of the pre-existing payroll tax cuts. It is well possible however that firms respond to wage bill-based CIT credits differently than to payroll tax cuts. They could for instance respond to a CIT credit by increasing their net profit or by sharing the benefit with their employees through wage increases. These effects are likely to be all the more different that labor and good markets are not competitive and that there exists rent sharing between employers and employees within the firm.

In this paper, we evaluate this far-reaching reform, taking advantage of a very rich set of administrative data, in order to understand how firms react in response to tax cuts and labor cost shocks. The competitiveness and employment tax credit (CETC) was introduced in France in January 1^{st} 2013. This scheme consists of a CIT credit equal to 4% of the eligible wagebill in 2013 and 6% of that eligible wagebill in 2014. Crucial for identification strategy, the eligible wagebill corresponds to the sum of gross wages for employees paid less than two and half-time the hourly minimum wage. In other words, the wages of salaried just above this threshold are not eligible to the CETC and this discontinuity generates important variation in the intensity of treatment between firms (even for firms with very similar wage structure) that we are going to exploit.

Our empirical analysis relies on a very rich dataset at the firm and individual levels. Very precise data on firms wage structure are found in annual social declarations database (DADS) at the level of each job (one observation per position for each company and one observation per job for each employee), built by Insee (French statistic agency). This database, exhaustive at the level of salaried jobs, also informs about the type of position held both in contractual terms and in terms of tasks. General information on the corporate structure of production and profits come from FARE database and consist in corporate tax return which are collected by the General Directorate of Public Finance - DGFiP - matched with survey data build by Insee. This database is exhaustive at the level of the companies. DGFiP also specifically builds MVC file informing the company's entitlements to CETC and its imputations, deferrals or reimbursements.

Our empirical strategy consists in estimating the effects of the CETC by comparing the evolution of employment, wage and profits (in levels or in growth rates) for companies more or less beneficiaries of the CETC due to the existence of this threshold in wagebill eligibility (double and triple differences estimations). To ensure the exogeneity of the treatment – the magnitude of CETC received as a share of labor cost – we also instrument the CETC actually received by the CETC that firms

could get, according to the characteristics of their production structure the years preceding the introduction of the CETC. To check for the robustness of this identification strategy, fixed effects and controls are introduced to ensure the common trend assumption between the treated and control firms. Finally, the potential existence of remaining diverging trends is also directly tested through placebo regressions.

Our results suggest that CETC did not succeed in boosting employment in the two first years (2013 and 2014). However the results suggest that firms used the CETC to restore their margins as the CETC has a positive and significant impact on three profit indicators (which tends to increase over time). Moreover the estimates show that the corporate income tax credit has been partially shifting on to wages. Differencing the effect on wages per type of worker, it appears that white collar are the main indirect beneficiaries of the scheme. These results bring new evidence on the existence of rent sharing between capital and labor and in favour of insiders (especially for white-collar workers).

An important literature has been dedicated to the evaluation of policies aiming at reducing labor cost. Many studies focus on social contribution cuts. Bohm and Lind (1993) and Bennmarker et al. (2009) for Sweden, and Korkeaäki and Uusitalo (2009) for Finland have developed estimates based on geographical differences in rates (taking advantage of regional reforms in social security contributions), and conclude to the absence of effect on employment. Social contribution cuts in France appear to have had more favorable effects on employment (Crépon & Desplatz 2001, Kramarz & Philipon 2001, Chéron et al., 2008). A potential reason could be that they are more targeted on low wages. However, Huttunen et al. (2013) use double difference estimation method (by age group) to assess the impact of social contribution cuts targeting low-wage workers in Finland: they found no impact at the extensive margin and only a very limited impact at the intensive margin.

One reason of the importance of low-wage targeting for explaining the impact of social contribution cuts comes from wage incidence. Gruber (1994), Anderson and Meyer (1997, 2000), and Murphy (2007) demonstrate, through natural experiments in the United States, that the share of social contributions actually paid by employers is inversely proportional to the level of wages. Moreover, taxation incidence on wages is not limited to social contributions: three recent empirical analyses (Arulampalam et al., Dwenger et al., 2011, Liu & Altshuler 2013) found that about half of the CIT rate cuts were passed on to employees through wage increases.

Our contribution to the literature is threefold. First, we evaluate the impact of the CETC jointly on three different outcomes (employment, wage and profits) and thus provide new quasi-experimental evidence on the incidence of corporate income tax credit. Moreover using detailed information on individual workers, we are able to document the existence and magnitude of rent sharing in the labor market and more importantly to which categories of employees it is most relevant (job stayers versus new hires, white-collars or blue-collars). Finally, our estimates, based on a large and still ongoing CIT credit program, point to weak employment effects, which casts new doubts on the relative effectiveness of such incentives. Given the popularity of cuts in corporate

income tax as a way to boost economic activity, our results are informative to the current policy debate.

Our current results are based on a linear difference-in-difference. However, we are currently implementing a matching estimator that allows us to isolate the variation in treatment intensity that comes from the discontinuity in the eligibility at the 2.5 minimum wage threshold. The matching estimator consists roughly in matching firms prior to treatment on several point of their cumulative density function ensuring that the remaining variation in treatment intensity stems from difference in wage structure around the 2.5 minimum wage threshold that we consider as good as random. The pre-treatment period allows us to perform several auxiliary exercises (placebo tests).

The rest of the paper is organized as follows. The databases are presented in section 2 and the identification strategy is detailed in section 3. The results of the estimates are presented and discussed in section 4. Section 5 concludes and put into perspective the impacts of CETC onto the different output in order to draw the global picture of the CETC aftermaths.

2 Data

This empirical analysis is based on three administrative databases, built from firms returns to the tax agency (DGFiP, the French General Directorate for Public Finance) and to the institution responsible to collect social contribution (ACOSS). DGFiP has computed, since the reform, a database specifically informing about the amount and use of the CETC at the firm-level (MVC database). They also provide in association with Insee (the French statistical agency) 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 got access to these databases for the years 2010 to 2014.

2.1 MVC database

DGFiP specifically built the MVC files informing the firms' initializations of CETC rights. This database, which began to be created for the 2013 vintage, contains five variables for all firms likely to benefit from the CICE - *i.e.* more than 800,000 observations. These five variables are: *initialization*, the amount of tax credit to which the company is entitled, initialized on its tax returns; *increase*, upward adjustments given the evolution of the company's wage structure; *decrease*, similar downward adjustments; *imputation*, the amount of CETC that companies were able to deduct from their CIT.

These variables allow us to understand the CETC distribution. After pairing with the other databases (and the loss of some companies absent from certain bases), the total amount initialized in 2013 is 9.8 billion euros. A large number of companies benefit from a relatively small amount of CICE, with about EUR 2 756 for micro-enterprises and EUR 24 492 for SMEs, whereas the amounts received by large companies are ten to one hundred times larger: the 288 large companies

present in the base have initiated in 2013 a tax credit approximately equal to that of the 496,750 micro-enterprises.

Initializations, besides being highly variable, represent relatively small amounts for companies: it exceeded one percent of turnover for only one quarter of the companies in 2013 and less than half in 2014 (which also illustrates the increase in CETC amounts between 2013 and 2014). Moreover, the collection of these amounts remains very spread out over time because of the nature of the tax credit nature of the CETC. The econometric results depend on this since any CETC impacts which would occur through the relaxation of budgetary constraints could only be observed after some years and cannot be estimated in our framework. Note that these mechanisms should not be at stake given that French firms are not budget constraints (Kremp and Sevestre 2013). On the contrary, incentive effects - especially in terms of employment linked to labor costs - are quicker to occur and our econometric framework should therefore be adapted to estimate them.

2.2 FARE database

General information on the production structure of companies and their benefits is 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. The purpose of the survey questionnaire is to produce structural business statistics; it should be noted that the questionnaire sent to companies was amended in 2011. This database covers all firms (including firms without employees) with the exception of the financial sector and farms.

Our dependent variables regarding the average workforce or the profitability of the firm come from this dataset. On this last point, the accounting entry for the CETC is unclear and it is likely that the different companies have accounted for it differently (deduction of labor costs, operating subsidies, other operating income or CIT deduction). Thus, the various measures of profit may or may not take into account the CETC according to how it is accounted for. We have tried to address this problem by considering three profit indicators: EBIT and EBITDA as a proportion of turnover, as well as operating income as a proportion of operating costs.

We also use variables from FARE as controls: productivity (value added divided by average workforce) and capital stock (tangible and intangible assets). As a robustness test, we also consider the set of control variables used by Gilles et al. (2016): margin rate (EBITDA / VA), economic profitability (EBITDA / fixed assets), productivity (VA / workforce), capital intensity (tangible assets / workforce), share of exports in turnover, investment rate (tangible investments / VA), debt ratio (borrowings and debts on the share capital, emission premiums, income from operations, investment subsidies on liabilities and other equity), financial drawdown rate (interest on loans / EBITDA).

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. For the six historical profiled companies, which only appear in the database in their profiled form, we consider the profiled company and similarly profile the other databases. The hundreds of other profiled companies are present in the database both under their individual SIREN and under their profiled SIREN. We consider for them only the individual SIREN.

2.3 DADS database

The annual social data declaration (DADS) files contain information on each salaried contract in each company: net and gross wages, working time, socio-professional categories, types of contracts, sex of the employee... There is one observation per contract for each company and employee. Thus, the same employee can be found several times in the dataset if she has contracts with several companies. It is therefore a database to be used from the point of view of the companies and not of the employees. In addition, it is important to know that DADS are presented in the form of regional files and that observations concerning employees of an enterprise located in one region but residing in another region are present in the regional files of the two regions. A first work before starting the analysis therefore consisted in purifying these databases from double accounts.

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 company identifiers are the SIRENs, stable over time, and we therefore constitute panels of companies. Thus, as far as wage increases are concerned, we have operated in two ways. On the one hand we calculated the average wages per firm each year, and compared them from one year to the next. Since the changes can be due both to changes in the wages themselves or to the structure of employment in the company, we have also calculated the growth hourly wage for each position present two following year in the same firm. Then, we aggregated by calculating for each year the average of individual wage growth.

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 amounts received during the contract). This can lead 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. In order to measure the growth of hourly wages, it is therefore necessary to purge the bases of these observations at the end of the contract.

In order to carry out our identification strategy, it is necessary to be able to measure the potential CETC to which an enterprise would have been entitled before the actual reform implementation, according to its productive structure. However, this is not possible on the basis of actual tax data,

which did not collect such information prior to 2013. However, the DADS database allows us to approach these values thanks to the precision on wage structures companies. It is indeed possible to calculate the share of wagebill below 2.5 minimum wage, and to compute a potential CETC. This calculation for the years 2013 and 2014 is very close to the amounts of CETC actually initialized with the tax departments and presented in the MVC database. Similar wage structure indicators are also built at other thresholds: 1.5, 2, 3 and 3.5 minimum wage.

2.4 Building the merged database

We are working on the matching of the three previously presented databases. After the DADS-MVC matching, we calculate the ratio of the CETC imputed on the basis of the DADS database over the actual CETC initiated in the MVC database. We exclude from the sample the firms whose ratio belongs to the upper percentile in 2013 or in 2014. We then perform the matching with the FARE database. Only the companies present in the 3 bases (DADS-FARE-MVC) are kept.

We then make two selections. First, we keep only companies that have at least one full-time equivalent job over the year, so as not to be biased by the empty shells. Then, we constructed the balanced panel database over the period 2010-2014. There are then slightly fewer than 500,000 firms in the final database used for the estimates.

3 Identification Strategy

The aim of the present evaluation is to use the French CETC reform to estimate the impact of wage costs' decreases on firms' behavior. Calling Y the dependent variable and C the cost variable (depending on the types of behavior studied, we can look at the total production costs TC or wagebill only WB only). We intend to measure the elasticity $\epsilon_{Y,C} = \partial \ln(Y)/\partial \ln(C)$. This elasticity may result from various economic mechanisms. This may be a form of rent sharing in the case of wage increases resulting from CIT rates decreases (Arulampalam et al. 2012, Liu and Altshuler 2013, Dwenger et al. 2011), substitution between production factors whose relative prices have been modified (Chirinko et al., 2011, Karabarbounis and Neiman 2013) or changes in the volume of output due to lower prices associated with lower costs.

Obviously, firms' production costs are strongly related to firms' behavior, and it is not possible to directly estimate the link between costs and the various dependent variables. The reform of the CETC not only needs to be evaluated *per se*, it also provides an opportunity to assess how firms react to an (indirect) decrease in wage cost. Indeed, it exogenously alters the production costs through a tax credit based on wages lower than 2.5 minimum wage. We use this exogenous variation in production costs to implement a double difference estimation of the impact of wage costs on firms' behavior.

However, such an estimate generally requires dividing firms in two groups: the treatment group containing firms impacted by the reform and the control groups containing those which are not.

It is not possible here to set up a control group because virtually all companies benefit from the reform. However, the extent to which CETC reduces production costs varies widely among firms, including between like-minded firms in the same economic sector. Figure 1 present the distribution of treatment intensity within two category of firms (categorization with respect to four sizes and six industries), which correspond to the two extreme in terms of distribution of treatment intensity. Both show a large variation in treatment intensity.

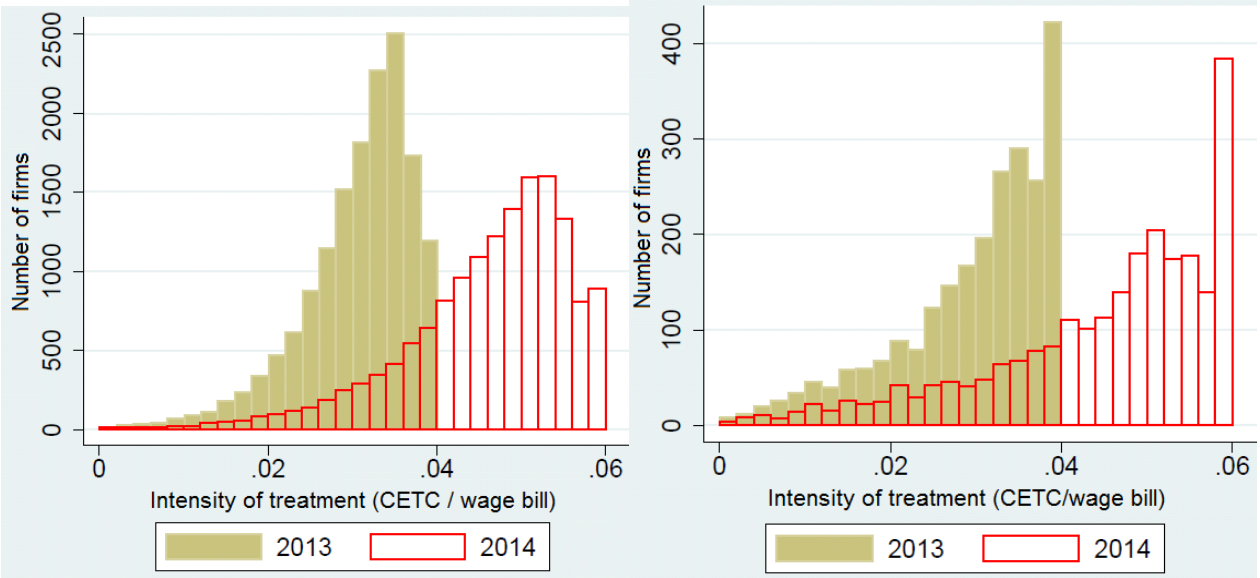
We therefore implement double difference estimations on the treatment intensity, considered as a continuous variable. The logarithm of the dependent variable is regressed on the logarithm of the production cost (less the CETC from 2013). This intensity of treatment can be considered on the basis of wage costs only (regressions on employment and wages) or on total production costs (profits).

However, two main reasons may cause estimation bias:

1/ Reverse causality: any company increasing its payroll one year - for reasons independent of the CETC - *de facto* increases the intensity of its treatment. Thus, treatment intensity is fundamentally endogenous to payroll growth (as long as it remains below 2.5 minimum wage).

2/ Common trend assumption: any double difference estimate requires that the common trend assumption between more intensively and less intensively treated groups be verified. Here, this means that firms that are more or less intensely treated by the CETC (*i.e.* companies whose eligible wagebill is more or less important compared to production costs) would have behave the same way in the absence of the CETC. This is not the case and it is therefore necessary to ensure that regressions are effectively controlling for potential heterogeneity of trends.

Figure 1: Distribution of treament intensity among manufacturing and personal services SMEs



Source: DADS-FARE-MVC 2013-2014

The statistical treatment of these two potential biases will be different and presented in the next two subsections.

3.1 Reverse causality

In order to tackle the reverse causality issue, a common solution used in the literature is to assign treatment on the lagged values of the variables which constitute the tax base or the subsidies. This strategy was used by Auten and Carroll (1999) in their estimation of the impact of the taxation of earned income, by applying the variation on the rate of earned income the year preceding the reform. In our case, it comes to use the relative stability in the production structure and to consider the ratio of eligibility the years preceding the introduction of the CETC as a proxy of the ratio of eligibility *ex ante*. In other words, we use the ratio of eligibility that firms would have had given their production structure before the implementation of the CETC so as not to take into account their endogenous response behavior in the computation of the ratio of eligibility and avoid the reverse causality issue. The same type of methodology was used in the case of France by Crépon et Desplatz (2001) to evaluate the impact of social contribution rebates in France.

In order to ensure that the reverse causality is not anymore an issue, one has to check for the validity of the instrument. A good instrument must fulfill two conditions: it has to be exogenous (contrary to the regressor that it is intended to instrument) and is has to be highly correlated with this regressor. Regarding the first criterion, temporal lags ensure exogeneity. The intensity of treatment is calculated with previous year wage structure, which has been chosen by firms in order to adapt the 2012 economic situation (with past dependency), without knowing about the existence and future introduction of the CETC. Indeed, the tax credit has been voted at the very end of 2012, and was presented and discussed in very short time at the end of this year. Consequently, the intensity of the intention to treat (potential treatment prior to firm behavior in response to treatment) can be considered as exogenous.

Regarding the second criterion, one can analyze the power of prediction of the effective treatment (i.e. of the CETC initialized in 2013 according to the tax records in the MVC database). In our case, it comes from the relative stability of firm production structure over time. Indeed, regressions of effective treatment (tax credit effectively initialized by companies in 2013 according to the MVC) on the instrument (i.e. tax credits predicted according to the wagebill of wages inferior to 2,5 the minimum wage the year preceding the reform), reveal the predictive power of this instrument. The coefficient is always highly significant and very near from unity. Besides, these first stage regressions contribute to explain most of the variance of the instrumented variable : more than 90% of the CETC initialized when measured as as a share of wage cost and between 66% and 80% of CETC initialized when measured as a share of total costs.

The main principle of the double difference estimation is to compare the evolution of treated and control groups before and after a reform. In order to do so, we estimate a panel regression with individual fixed effects, a time dummy and a time dummy interacted with the treated group.

Given that we our control group is not composed of firms which do not benefit from the tax credit (extensive margin) but rather by firms less intensively treated (intensive margin), we interact the time dummy with the "intention to treat" (i.e. the tax credit that firms could get given their production structure the years preceding the reform). This is summarized in the following equation 1.

$$\ln(Y_{i,t}) = \alpha + \beta_{13}.I_{i,t}.\mathbb{1}_{[t=2013]} + \beta_{14}.I_{i,t}.\mathbb{1}_{[t=2014]} + \sum_j \gamma_j.X_{j,i,t} + \sum_f \gamma_f.\mathbb{1}_{[f]} + \epsilon_{i,t} \quad (1)$$

where $I_{i,t} = -\ln(1 - \frac{CICE^i_{i,t-1}}{C_{i,t-1}})$ is the intention to treat computed as a share of production costs. Note that depending on the specification, production costs correspond to the total wagebill (from the DADS database) or to the total production costs (from the FARE database). $X_{j,i,t}$ stand for the values of different controls j, $\mathbb{1}_{[f]}$ for the different fixed effects and i refers to firms and t to year. For ensuring exogeneity, as for treatment variables, control variable are also lagged one year. The coefficients β_{13} et β_{14} can be interpreted as elasticities of the variable $Y_{i,t}$ relative to production costs for 2013 and 2014. Note that a "negative sign" was added in front of the intensity variable for the ease of interpretation given that the CETC represents a diminution in costs. This choice was made so that regressions coefficients can be easily interpreted as the impact of the CETC: a positive sign in result tables means that the CETC has a positive impact on the outcome variable.

3.2 The common trend assumption

To be relevant, the double difference estimation relies on a strong assumption: the one of "common trend assumption". In other words, this method is valid if and only if firms with different intensity to treat follow the same trend before the introduction of CETC. This assumption, when verified, means that differences between the treated and less treated firms would have remained constant over time in the absence of the policy under evaluation, and therefore ensure that relatively less intensively treated firms can serve as valid counter-factual for more intensively treated firms. In the opposite case, if those firms would not follow the same trend before the reform, it would be impossible for the econometrician to disentangle, in the post-reform evolution between the treated and less intensively treated firms (double difference estimation), what can be attributable to the policy from what can be attributable to any other confounding factor.

As this is not always the case, one can take into account any potential pre-reform different trends by including several controls for capturing these trends. This becomes a common trend *ceteris paribus*. In this purpose, we add may controls in the regressions. First, we introduce "sector $\times$ year" and "firm-size $\times$ year" fixed effects in order to capture all specific sectoral and size class trends. We also control for firm fixed effects to control for all the unobservable which are specific to a firm and constant over time. Moreover we add different time-varying controls at the firm-level which can also influence our outcome variables: productivity (measured as a ratio of valued added on average employment), capital stock (tangible and intangible assets) as well as firm average wage. Moreover, we add many controls of a firm production structure. In order to capture most of the

intrinsic differences pre-reform between the firms which (will) become more intensively and less intensively treated after the introduction of the CETC, we control for the share of wagebill inferior to 2.5 the minimum wage, $I_{i,t}$, without interacting it with the year dummies.

Moreover, as in France, there are some yearly variations in the minimum wage and some exemptions which are proportional to the minimum wage, these variations can impact the total wagebill and therefore also impact our outcome variables. As a consequence, we also add controls of the share of wagebill exposed to the minimum wage variations to avoid estimation bias. In particular, we introduce $IMISC^a_{i,t} = \frac{MS^{[1,5]}_{i,t}}{MS_{i,t}} * \mathbb{1}_{[t=a]}$ for the different years a, where $MS_{i,t}$ is the gross wagebill of firm i in year t and $MS^{[1,5]}_{i,t}$ its wagebill when workers payed less than 1.5 the minimum wage are considered.

Moreover, we also include specifications in which we replace our set of controls by the one used by Gilles et al. (2016): profit margin (Gross operating surplus/value added), economic rentability (Gross operating surplus/tangible and intangible assets), productivity (Value added/workers), capital intensity (tangible assets / workers), share of export in the turnover, investment rate (tangible investments/value added), rate of debt (borrowing and debts on the sum of share capital, issue premium, investment subsidy in the liabilities and other equities), rate of financial burden (borrowing interest/gross operating surplus), as well as different elements of the composition of workers by gender (share of women) by socio-professional category (share of blue-collars, white-collars etc.) or by type of contract (full-time, part-time, short-term or long-term contracts).

In order to check that these controls properly capture the intrinsic pre-reform differences between firms relatively more or less intensively treated, a usual test consists in operating "placebo" regressions. The idea is to proceed to the same regression as described in equation 1, but only on the years preceding the effective introduction of the CETC, in order to measure the " fictive effect" of the introduction of the CETC in 2012. This comes to estimate the following equation 2.

$$\ln(Y_{i,t}) = \alpha + \beta_{placebo}.I_{i,t}.\mathbb{1}_{[t=2012]} + \sum_j \gamma_j.X_{j,i,t} + \sum_f \gamma_f.\mathbb{1}_{[f]} + \epsilon_{i,t} \tag{2}$$

The placebo test is valid only if the coefficient $\beta_{placebo}$ is not significantly different from zero; meaning that the dependance of the outcome variable in the structure of production which induces the intention to treat is stable before the introduction of the CETC. In other words, this is a test of the common trend assumption. If the placebo test validates the common trend assumption, the coefficients β_{13} and β_{14} can be seen as unbiased estimates of the elasticity of the outcome variable Y on wage cost (or production costs depending on the outcome of interest). If the placebo test rejects the common trend assumption, one has to better control for this trend heterogeneity.

One way to do it is to estimate a triple difference rather than a double one, which reduced from is given by equation 3.

$$\Delta \ln(Y_{i,t}) = \alpha + \beta_{13}.\Delta I_{i,t}.\mathbb{1}_{[t=2013]} + \beta_{14}.\Delta I_{i,t}.\mathbb{1}_{[t=2014]} + \sum_j \gamma_j.\Delta X_{j,i,t} + \sum_f \gamma_f.\mathbb{1}_{[f]} + \epsilon_{i,t} \tag{3}$$

where Δ stands for first difference estimator. In this specification "in trend", the firm fixed effects measures the trend (out of treatment) in the growth rate of the outcome variable Y specific to each firm.

4 Results

4.1 Employment

The objectives of the CETC was primarily to boost employment; this is usually the main goal of policies aiming at decreasing labor cost. There are several ways of counting employment: the two most usual being the number of workers employed (whatever their working time) and the number of full-time equivalent jobs (or equivalently the number of hours worked). We analyze these two measures as dependent variables. Furthermore, if the number of hours are only given by the DADS database, the workforce in each firm is given by two different sources: the DADS and the FARE databases. The two variables are regressed for checking the robustness of our results. Note that we are doubling the estimates: a series of regressions is weighted by workforce in 2012 and the other is not weighted. Unweighted regressions give more importance to small firms because they are more numerous; they reveal the behavior of firms, considered as decision-making units. Conversely, weighted regressions give more weight to firms with a greater share of jobs; their coefficients are closer to an interpretation of macroeconomic effects. Besides, as highlighted by Solon *et al.* (2015), the comparison of weighted and unweighted regression is informative as it can reveal the existence of heterogeneity in firm behavior (according to firm size in our case) when coefficients differ. We therefore choose to present both types of estimates because they are both relevant and complementary.

The main results of estimations are presented in tables 1. For robustness checks, alternative specifications are presented in Tables 6 to 11 of annex A.1. Results are quite stable in all specifications. The unweighted regressions validate the common trend assumption, with placebo tests very close to zero (not significant despite very small standard errors). However, placebo tests are not validated for weighted regressions. Whatever the specification and the type of employment measure used as dependent variable, results suggest that the CETC has had no impact on employment. Surprisingly, the effect, if any, would rather be negative even though very small.

Given that average effects can hide heterogeneity between workers, we reproduce these estimates for different categories of employees apart. For these categorized dependent variables, we use only workforce extracted from DADS (as such details are not available in the FARE database). The first categorization concerns the socio-professional categories, the results of the regressions being presented in table 2 (and alternative specifications in annexe A.2 in tables 12 to 19).

We find a small positive effect for executives and higher intellectual occupations, but with a placebo test which fails for unweighted regressions. For weighted regressions, the placebo test validates the estimations and the impact of the CETC is positive in 2014. However, results are

Table 1: Impact of CETC on total employment

	Dependent variable		
	Average employment		Hours worked
	DADS	FICUS-FARE	DADS
Unweighted regressions			
Placebo test	-0.0585	0.0391	0.0705
	(0.0537)	(0.0438)	(0.0467)
Intention to treat intensity, 2013	-0.499***	-0.180***	-0.222***
	(0.0547)	(0.0444)	(0.0416)
Intention to treat intensity, 2014	-0.490***	-0.144***	-0.141***
	(0.0470)	(0.0400)	(0.0386)
Observations	1788824	1788684	1788823
R^2	0.973	0.979	0.982
Weighted regressions			
Placebo test	-0.740	-1.415*	-1.601**
	(0.435)	(0.576)	(0.558)
Intention to treat intensity, 2013	-0.185	-0.354	-1.147***
	(0.277)	(0.289)	(0.331)
Intention to treat intensity, 2014	-0.178	-0.0824	-0.796**
	(0.347)	(0.230)	()
Observations	1788824	1788684	1788823
R^2	0.998	0.996	0.997

Notes: Regression of the dependent variable (logarithm of workforce from DADS or FARE databases and yearly hours) on the intensity of the intention to treat, with controls for firm productivity, capital stock, mean wage, wage structure, minimum wage exposure and fixed effects: year×industry, year×size and firm.
Robust standard errors in parentheses (firm level cluster), * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Sources: DADS, FARE, MVC 2010-2014.

not significant for intermediate professions. For blue collar workers, we find the same results as for global estimations, with validated placebo tests and negative coefficients of impact estimation.

As presented in tables 20 to 23 in appendix A.3, no difference appears between male and female workers: the results for both are the same, and very close to those obtained for all workers estimations. Regarding contract types (tables 24 à 27 in appendix A.4), the results are mainly inconclusive but the placebo tests fail.

Overall the CETC has not had the expected positive effect on jobs, either because firms need more time before adjusting employment or because firms prefer use the CETC for another purpose. This is the topic of the next section.

4.2 Profits

If the CETC has not been used to increase the workforce, keeping the tax credit as net profit is another possible use. However, it is not trivial to properly measure it because there is no automatic way of including it in firm accounts. Therefore, we consider three profit indicators: gross margins (EBIT as a proportion of turnover), net margins (EBITDA as a proportion of turnover), and

Table 2: Impact of CETC on employment per socioprofessional category

	Dependant variable : aveage employment		
	Executives, higher intellectual professions	Intermediate professions	Blue collars workers
Unweighted regressions			
Placebo test	0.709***	0.247*	0.0772
	(0.111)	(0.132)	(0.119)
Intention to treat, intensity, 2013	0.869***	0.144	-0.488***
	(0.102)	(0.120)	(0.110)
Intention to treat intensity, 2014	0.932***	-0.0118	-0.515***
	(0.0871)	(0.0953)	(0.0839)
Observations	706869	804631	1219398
R^2	0.951	0.932	0.949
Weighted regressions			
Placebo test	0.468	0.629	-0.639
	(0.789)	(1.243)	(1.418)
Intention to treat intensity, 2013	0.519	1.103	-1.269
	(0.490)	(0.866)	(1.060)
Intention to treat intensity, 2014	0.793*	1.834**	-2.637***
	(0.417)	(0.730)	(0.994)
Observations	706869	804631	1219398
R^2	0.991	0.989	0.992

Notes: Regressions of the logarithm of average employment per socio professional category on the intensity of the intention to treat, with controls for firm productivity, capital stock, mean wage, wage structure, minimum wage exposure and fixed effects: year×industry, year×size and firm.
Robust standard errors in parentheses (firm level cluster), * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Sources: DADS, FARE, MVC 2010-2014.

operating margins (operating income as a proportion of operating costs). Companies were advised to account for the CETC as "deduction of personnel costs". If they did it in this way, then the CETC should appear in all three profit indicators. Nevertheless, many other accounting entries were possible, some involving not taking into account this writing in one or more indicators, so one should be aware that we might not fully capture the impact on profits.

The results of the estimations of CETC on each of the three profit indicators are summarized in table 3. Regarding unweighted regressions, the placebo tests validate the common trend assumption upon which rely our double difference estimations for gross margins and net margins and the CETC has a positive and significant impact in 2014 in the case of net margins. Regarding weighted regressions, placebo tests are validated for all profits indicators, and results indicate a positive and significant impact of the CETC on profits in 2014. Overall, these results suggest that French firms used the tax credit to restore their margins and moreover that the impact of the CETC tends to increase over time.

Table 3: Impact of the CETC on profits

	Dependant variables		
	Net margins	Gross margins	Operating margins
Unweighted regressions			
Placebo test	-0.100	-0.0187	-0.183***
	(0.0296)	(0.0376)	(0.0307)
Intention to treat intensity, 2013	0.0197	-0.0285	-0.0394
	(0.0284)	(0.0293)	(0.0290)
Intention to treat intensity, 2014	0.0441*	-0.0283	-0.0184
	(0.0213)	(0.0222)	(0.0222)
Observations	1873923	1876084	1786084
R^2	0.759	0.781	0.659
Weighted regressions			
Placebo test	0.0503	0.0556	-0.101
	(0.141)	(0.116)	(0.0872)
Intention to treat intensity, 2013	0.267*	0.079	0.0538
	(0.125)	(1.103)	(0.10980)
Intenstion to treat intensity, 2014	0.372***	0.168*	0.193*
	(0.0996*)	(0.0723)	(0.0732)
Observations	1873923	1876084	1876084
R^2	0.847	0.442	0.718

Notes: Regression of the dependent variable on the intensity of the intention to treat, with controls for firm productivity, capital stock, mean wage, wage structure, minimum wage exposure and fixed effects: year×industry, year×size and firm, and weighted with 2012 workforce.
Robust standard errors in parentheses (firm level cluster), * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Sources: DADS, FARE, MVC 2010-2014.

4.3 Wages

If only a fraction of the CETC has translated into increases in firm profits and if they did not use this credit to hire, firm might have shared the benefits of the tax credit with their employees. However, if any, the potential impact on wages is not clear.

The tax credit scheme is very particular since it is based on the payroll by generating a significant threshold effect: the CETC rate is constant (4% in 2013 and 6% starting in 2014) and suddenly died down to 2.5 SMICs. Thus, a full-time employee paid two and a half times the minimum wage opened in 2014 an annual tax credit of 2,600 euros for his employer. If she had been paid even a few euros more, her employer could not have received any tax credit. Hence, employers may be particularly reluctant to grant increases to their employees close to the threshold or they will seek to set the wage of their new recruits as far as possible below this threshold. In their extreme

configurations, strong employer reactions to these two issues (increases and hires) could lead to bunching at the threshold (Saez 2010).

However, Carbonnier et al. (2014) showed that a gap discontinuity in a framework where the assignment variable was only imperfectly controlled could lead not to a point of accumulation but to a discontinuity in the values of the variables involved. Carbonnier et al. (2016) study the two hypotheses and did not found the tiniest sign of wage setting behavior around the threshold. Therefore the wage behavior may be more spreadly distributed. The benefit of the CETC may be partially redistributed to employees independently on their wage position vis à vis the threshold. In order to test this, we apply our identification strategy to different wage indicators. The main results for all types of employees are presented in table 4 and alternative specifications are reported in annexe B.1 for robustness checks. Three different indicators are considered: mean yearly wage per employee (based on FARE workforce indicator), mean hourly wage of staying employees and the mean of individual hourly wage growth.

Table 4: Impact of the CETC on wages

	Dependent variable		
	Avergae hourly wage	Growth in hourly wage	Average yearly wage
Unweighted regressions			
Placebo test	1.435***	0.788***	1.838***
	(0.0332)	(0.0303)	(0.0629)
Intention to treat intensity, 2013	1.512***	0.715***	1.775***
	(0.0292)	(0.0258)	(0.0570)
Intention to treat intensity, 2014	3.381***	0.776***	1.842***
	(0.0932)	(0.0175)	(0.0480)
Observations	1658208	1658204	1788824
R^2	0.940	0.354	0.907
Weighted regressions			
Placebo test	0.790***	0.334	0.823*
	(0.165)	(0.173)	(0.349)
Intention to treat intensity, 2013	1.245***	0.529***	0.405
	(0.204)	(0.134)	(0.253)
Intention to treat intensity, 2014	1.182***	0.456***	0.572*
	(0.139)	(0.0833)	(0.258)
Observations	1658208	1658204	1788823
R^2	0.946	0.327	0.997

Notes: Regression of the dependent variable on the intensity of the intention to treat, with controls for firm productivity, capital stock, mean wage, wage structure, minimum wage exposure and fixed effects: year×industry, year×size and firm, and weighted with 2012 workforce.
Robust standard errors in parentheses (firm level cluster), * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Sources: DADS, FARE, MVC 2010-2014.

For yearly and hourly wages, double difference placebo tests fail but for the mean of hourly

wage growth, placebo tests validate the common trend assumption for the weighted specifications. In that case, the estimates are significantly positive suggesting that the benefits of the CETC were partially transferred to some employees trough wage increases. Moreover, their value around 50% is very close to previous estimates of the CIT incidence on wages (Arulampalam et al. 2012, Dwenger et al. 2011, Liu & Altshuler 2013).

However, these effects on wages are not the same for all types of workers. Main results of regression by socio-professional category are reported in Table5 (and alternative specifications in annexe B.2in tables 32 to 37. Regarding average hourly wage, weighted regressions suggest a positive impact for executives and higher intellectual professions only. Regarding growth in wages, all placebo tests reject the common trend assumption in the case of unweighted regressions. However, all placebo tests validate weighted regressions. Results suggest a positive and significant impact of the CETC on wages for executive and higher intellectual professions and for intermediate professions and to a lesser extent for blue-collar workers in 2014.

Table 5: Impact of CETC on wages per socio-professional catgeory

	Executives and intellectual professions	Intermediate professions	Blue collar workers
Average hourly wage			
Placebo test	0.274	0.175	0.490
	(0.280)	(0.300)	(0.267)
Intention to treat intensity, 2013	0.865***	0.233	-0.482
	(0.229)	(0.235)	(0.376)
Intention to treat intensity, 2014	0.754***	0.372*	-0.141
	(0.137)	(0.147)	(0.184)
Observations	549869	612930	932181
R^2	0.884	0.881	0.897
Growth in hourly wage			
Placebo test	-0.292	0.146	-0.189
	(0.253)	(0.255)	(0.264)
Intention to treat intensity, 2013	0.594**	0.181	0.270
	(0.194)	(0.194)	(0.184)
Intention to treat intensity, 2014	0.164	0.448***	0.336**
	(0.126)	(0.105)	(0.128)
Observations	549866	612927	932179
R^2	0.330	0.346	0.358

Notes: Regression of the dependent variable on the intensity of the intention to treat, with controls for firm productivity, capital stock, mean wage, wage structure, minimum wage exposure and fixed effects: year×industry, year×size and firm, and weighted with 2012 workforce.
Robust standard errors in parentheses (firm level cluster), * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Sources: DADS, FARE, MVC 2010-2014.

r

5 Conclusion

In this paper, we exploit a large French CIT reform, introduced in 2013, (competitiveness and employment tax credit, CETC) to assess the impact of corporate tax aiming at reducing labor costs on firm behavior. Our empirical analysis relies on three exhaustive databases which contain precise information at the firm and individual levels on the period 2010-2014. We set an identification strategy in double (and triple) difference based on the intensity of the intention to treat to quantify the impact of the introduction of this tax credit on three different outcomes: employment, profit and wages.

Our results suggest that the CETC has had no positive impact on employment. Even more, some counter-intuitive results are found for lower socio-professional categories, whereas the coefficients are close to zero for upper socio-professional categories. Conversely, the impact on firm profits is positive and significant and tends to increase over time. Firms therefore mainly used the CETC to restore their margins. However, effects on wage also appear positive and significant : some of the benefits of the CETC have been distributed to employees through wage increases, a results which is close to previous estimates in the literature. Moreover, the results on wages differ depending on the socio-professional category of the employees. The stronger impact is found for executive and higher intellectual occupations while intermediate occupations benefit from mean wage increases. For blue collar workers and other employees, the results are not always significant and appear less robust. These results give new evidence about the importance of taking into account rent sharing in favor of capital and in favor of white-collar employees when it comes to assess the effectiveness of such tax incentives.

References

Anderson P.A., Meyer B.D. (1997) "The effects of firm specific taxes and government mandates with an application to the U.S. unemployment insurance program", Journal of Public Economics, vol. 65, pp. 119-145.

Anderson P.A., Meyer B.D. (2000) "The effects of the unemployment insurance payroll tax on wages, employment, claims and denials", Journal of Public Economics, vol. 78, pp. 81-106.

Arulampalam W., Devereux M.P., Maffini G. (2012) "The direct incidence of corporate income tax on wages", European Economic Review, vol. 56, pp. 1038-1054.

Bennmarker H., Mellander E., Ockert B. (2009) "Do regional payroll tax reductions boost employment?", Labour Economics, vol. 16, pp. 480-489.

Bohm P., Lind H. (1993) "Policy evaluation quality: a quasi-experimental study of regional employment subsidies in Sweden", Regional Science and Urban Economics, vol. 23, pp. 51-65.

Bunel M., L'Horty Y. (2012) "The Effects of Reduced Social Security Contributions on Employment: An Evaluation of the 2003 French Reform", Fiscal Studies, Vol. 33, 3

Carbonnier C., Direr A., Slimani-Houti I. (2014) "Do Savers Respond to Tax Incentives? The Case of Retirement Savings", Annals of Economics and Statistics, Vol. 113-114, pp. 225-256.

Carbonnier C., Fredon S., Gautier B., Malgouyre C., Mayer T., Py L., Rot G., Urvoy C. (2016) "Evaluation interdisciplinaire des impacts du CICE en matière de compétitivité internationale, d'investissement, d'emploi, de résultat net des entreprises et de salaires", Rapport du Laboratoire Interdisciplinaire d'Evaluation des Politiques Publiques (LIEPP) de Sciences Po en réponse à l'appel à évaluation de *France Stratégie*.
[https://spire.sciencespo.fr/hdl:/2441/4v8c8tnfgu8g392dpm6uaefuus]

Chéron A., Hairault J.-O., Langot F. (2008) "A quantitative evaluation of payroll tax subsidies for low-wage workers: An equilibrium search approach", Journal of Public Economics, vol. 92, pp. 817-843.

Chirinko R.S., Fazzari S.M., Meyer A.P. (2011) "A New Approach to Estimating Production Function Parameters: The Elusive Capital-Labor Substitution Elasticity", Journal of Business & Economic Statistics Vol. 29(4), pp. 587-594.

Crépon, B. and Deplatz, R. (2001) "Une nouvelle évaluation des effets des allègements de charges sociales sur les bas salaires", Economie et statistique, vol. 348, pp. 3-24.

Dwenger N., Rattenhuber P., Steiner V. (2011) "Sharing the burden: Empirical evidence on corporate tax incidence", Discussion Papers 2011/19, Free University Berlin, School of Business & Economics.

Gilles F., Bunel M., L'Horty Y., Mihoubi F., Yang X. (2016) "Les effets du CICE sur l'emploi, les salaires et la R&D : une évaluation ex post", Rapport de recherche du TEPP, n^o 16.9.

Gruber J. (1994) "The incidence of mandated maternity benefits", American Economic Review, vol. 84, pp. 622-641.

Huttunen K., Pirttilä J., Uusitalo R. (2013) "The employment effects of low-wage subsidies", Journal of Public Economics, vol. 97, pp. 49-60.

Karabarbounis L., Neiman B. (2013) "The Global Decline of the Labor Share", The Quarterly Journal of Economics, vol. 129(1), pp. 61-103.

Korkeamäki O., Uusitalo R. (2009) "Employment and wage effects of a payroll-tax cut - evidence from a regional experiment", International Tax and Public Finance, vol. 16, pp. 753-772.

Kramarz, F. and Philippon, T. (2001) "The impact of differential payroll tax subsidies on minimum wage employment", Journal of Public Economics, vol. 82, pp. 115-146.

Kremp E., Sevestre P. (2013) "Did the crisis induce credit rationing for French SMEs?", Journal of Banking & Finance, vol. 37(10), pp. 3757-3772.

Liu L., Altshuler R. (2013) "Measuring The Burden Of The Corporate Income Tax Under Imperfect Competition", National Tax Journal, vol. 66(1), pp. 215-37.

Murphy K.J. (2007) "The impact of unemployment insurance taxes on wages", Labour Economics, vol. 14, pp. 457-484.

Saez E. (2010) "Do Taxpayers Bunch at Kink Points?", American Economic Journal: Economic Policy, Vol. 2, pp. 180-212.

Solon G., S. J. Haider, and J. M. Wooldridge (2015) "What are we weighting for?" Journal of Human resources 50(2), pp. 301-316.

A Complementary results on employment

A.1 All workers

Table 6: Impact of the CETC on employment (DADS)

	(1)	(2)	(3)	(4)
Placebo test	-0.689***	-0.0758	-0.0578	-0.0585
	(0.0907)	(0.0505)	(0.0523)	(0.0537)
Double difference				
Intention to treat intensity 2013	-0.0713	-0.517***	-0.524***	-0.499***
	(0.0858)	(0.0536)	(0.0542)	(0.0547)
Intention to treat intensity 2014	-0.132*	-0.430***	-0.517***	-0.490***
	(0.0633)	(0.0450)	(0.0454)	(0.0470)
Observations	1918585	1918584	1789248	1788824
R^2	0.685	0.971	0.973	0.973
Triple difference				
Intention to treat intensity 2013	-0.236**	-0.303***	-0.348***	-0.322***
	(0.0721)	(0.0727)	(0.0750)	(0.0779)
Intention to treat intensity 2014	0.164	-0.0841	-0.680***	-0.595***
	(0.145)	(0.148)	(0.154)	(0.161)
Observations	1438938	1438938	1348159	1347902
R^2	0.006	0.300	0.305	0.305
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level))
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 7: Impact of the CETC on employment (DADS, weighted)

	(1)	(2)	(3)	(4)
Placebo test	-5.144***	-1.314**	-0.784	-0.740
	(1.378)	(0.422)	(0.438)	(0.435)
Double difference				
Intention to treat intensity 2013	-4.408***	-0.121	-0.177	-0.185
	(0.915)	(0.280)	(0.277)	(0.277)
Intention to treat intensity 2014	-2.969***	-0.208	-0.135	-0.178
	(0.786)	(0.412)	(0.351)	(0.347)
Observations	1918585	1918584	1789248	1788824
R^2	0.905	0.997	0.998	0.998
Triple difference				
Intention to treat intensity 2013	2.296**	1.099*	0.938	0.963
	(0.751)	(0.557)	(0.559)	(0.549)
Intention to treat intensity 2014	2.776	0.132	0.0342	0.0584
	(1.769)	(1.552)	(1.400)	(1.363)
Observations	1438938	1438938	1348159	1347902
R^2	0.059	0.396	0.398	0.398
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level))
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 8: Impact of CETC on employment (FARE)

	(1)	(2)	(3)	(4)
Placebo test	-0.153*	-0.0565	-0.0138	0.0391
	(0.0762)	(0.0397)	(0.0403)	(0.0438)
Double difference				
Intention to treat intensity 2013	0.273***	-0.180***	-0.206***	-0.180***
	(0.0729)	(0.0434)	(0.0435)	(0.0444)
Intention to treat intensity 2014	0.115*	-0.168***	-0.187***	-0.144***
	(0.0536)	(0.0377)	(0.0376)	(0.0400)
Observations	1918368	1918360	1789102	1788684
R^2	0.758	0.978	0.979	0.979
Triple difference				
Intention to treat intensity 2013	-0.425***	-1.076***	-1.135***	-1.075***
	(0.0604)	(0.0598)	(0.0575)	(0.0612)
Intention to treat intensity 2014	0.359**	-1.172***	-2.893***	-2.657***
	(0.119)	(0.116)	(0.119)	(0.135)
Observations	1438658	1438578	1347980	1347723
R^2	0.012	0.332	0.411	0.411
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 9: Impact of CETC on employment (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-2.072	-2.138***	-1.603**	-1.415*
	(1.267)	(0.615)	(0.575)	(0.576)
Double difference				
Intention to treat intensity 2013	-2.937***	-0.390	-0.404	-0.354
	(0.758)	(0.276)	(0.288)	(0.289)
Intention to treat intensity 2014	-1.844**	-0.0924	-0.0813	-0.0824
	(0.603)	(0.242)	(0.237)	(0.230)
Observations	1918368	1918360	1789102	1788684
R^2	0.882	0.995	0.996	0.996
Triple difference				
Intention to treat intensity 2013	1.824*	-0.0159	-0.0299	0.0203
	(0.834)	(0.628)	(0.557)	(0.563)
Intention to treat intensity 2014	4.982***	0.540	-1.228	-1.242
	(1.499)	(0.942)	(0.855)	(0.852)
Observations	1438658	1438578	1347980	1347723
R^2	0.032	0.366	0.479	0.479
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 10: Impact of CETC on number of hours worked

	(1)	(2)	(3)	(4)
Placebo test	-0.103	-0.0370	-0.0133	0.0705
	(0.0732)	(0.0372)	(0.0386)	(0.0467)
Double difference				
Intention to treat intensity 2013	0.195**	-0.242***	-0.259***	-0.222***
	(0.0699)	(0.0397)	(0.0398)	(0.0416)
Intention to treat intensity 2014	0.0913	-0.176***	-0.196***	-0.141***
	(0.0519)	(0.0360)	(0.0360)	(0.0386)
Observations	1918584	1918583	1789247	1788823
R^2	0.773	0.981	0.982	0.982
Triple difference				
Intention to treat intensity 2013	-0.506***	-1.194***	-1.264***	-1.215***
	(0.0552)	(0.0534)	(0.0525)	(0.0684)
Intention to treat intensity 2014	0.466***	-1.128***	-3.238***	-2.811***
	(0.113)	(0.108)	(0.113)	(0.156)
Observations	1438936	1438935	1348157	1347900
R^2	0.014	0.363	0.407	0.408
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 11: Impact of CETC on number of hours worked (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-2.982**	-2.416***	-1.856***	-1.601**
	(1.156)	(0.587)	(0.554)	(0.558)
Double difference				
Intention to treat intensity 2013	-4.183***	-1.147***	-1.181***	-1.147***
	(0.765)	(0.322)	(0.333)	(0.331)
Intention to treat intensity 2014	-2.724***	-0.793**	-0.793**	-0.796**
Observations	1918584	1918583	1789247	1788823
R^2	0.908	0.997	0.997	0.997
Triple difference				
Intention to treat intensity 2013	1.249	-0.817	-0.878	-0.914
	(0.782)	(0.547)	(0.527)	(0.554)
Intention to treat intensity 2014	4.761***	0.162	-2.783***	-2.679**
	(1.430)	(0.809)	(0.824)	(0.840)
Observations	1438936	1438935	1348157	1347900
R^2	0.033	0.374	0.425	0.425
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

A.2 By socio-professional category

Table 12: Impact of CETC on employment for executives and higher intellectual professions

	(1)	(2)	(3)	(4)
Placebo test	-0.923***	0.668***	0.673***	0.709***
	(0.142)	(0.104)	(0.110)	(0.111)
Double difference				
Intention to treat intensity 2013	-0.764***	0.777***	0.840***	0.869***
	(0.132)	(0.0981)	(0.101)	(0.102)
Intention to treat intensity 2014	-0.102	1.008***	0.871***	0.932***
	(0.0962)	(0.0773)	(0.0801)	(0.0871)
Observations	789800	756315	707047	706869
R^2	0.630	0.948	0.951	0.951
Triple difference				
Intention to treat intensity 2013	0.605***	1.446***	1.616***	1.632***
	(0.158)	(0.169)	(0.175)	(0.177)
Intention to treat intensity 2014	-0.219	0.929**	1.910***	1.707***
	(0.288)	(0.314)	(0.331)	(0.348)
Observations	533708	505723	475912	475824
R^2	0.005	0.218	0.224	0.224
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 13: Impact of CETC on employment for executives and higher intellectual professions (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-4.013*	0.112	0.338	0.468
	(1.824)	(0.841)	(0.814)	(0.789)
Double difference				
Intention to treat intensity 2013	-6.125***	0.360	0.548	0.519
	(1.487)	(0.521)	(0.495)	(0.490)
Intention to treat intensity 2014	-3.396**	0.912*	0.762	0.793
	(1.067)	(0.429)	(0.424)	(0.417)
Observations	789800	756315	707047	706869
R^2	0.815	0.990	0.991	0.991
Triple difference				
Intention to treat intensity 2013	0.326	0.826	1.506	1.423
	(1.012)	(0.962)	(0.956)	(0.941)
Intention to treat intensity 2014	0.256	1.144	1.217	0.855
	(1.905)	(1.777)	(1.717)	(1.686)
Observations	533708	505723	475912	475824
R^2	0.021	0.219	0.229	0.229
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 14: Impact of CETC on employment for intermediate professions

	(1)	(2)	(3)	(4)
Placebo test	-0.488**	0.199	0.259*	0.247
	(0.165)	(0.124)	(0.132)	(0.132)
Double difference				
Intention to treat intensity 2013	0.322*	0.204	0.159	0.144
	(0.153)	(0.115)	(0.119)	(0.120)
Intention to treat intensity 2014	0.193	-0.0165	-0.0108	-0.0118
	(0.110)	(0.0896)	(0.0933)	(0.0953)
Observations	902289	858572	804799	804631
R^2	0.541	0.931	0.932	0.932
Triple difference				
Intention to treat intensity 2013	0.0302	-0.0920	-0.192	-0.161
	(0.190)	(0.203)	(0.211)	(0.213)
Intention to treat intensity 2014	-0.909**	-1.137**	-0.730	-0.577
	(0.344)	(0.373)	(0.392)	(0.400)
Observations	600607	568091	534994	534902
R^2	0.003	0.216	0.218	0.218
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 15: Impact of CETC on employment for intermediate professions (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-2.051	0.138	0.628	0.629
	(1.979)	(1.327)	(1.269)	(1.243)
Double difference				
Intention to treat intensity 2013	-1.438	1.710	1.301	1.103
	(1.451)	(0.876)	(0.873)	(0.866)
Intention to treat intensity 2014	0.0426	2.228**	1.910**	1.834*
	(1.124)	(0.740)	(0.728)	(0.730)
Observations	902289	858572	804799	804631
R^2	0.821	0.988	0.989	0.989
Triple difference				
Intention to treat intensity 2013	0.651	-0.675	-0.904	-0.947
	(1.628)	(1.480)	(1.510)	(1.495)
Intention to treat intensity 2014	2.806	-0.115	0.293	0.885
	(3.318)	(3.108)	(3.134)	(3.107)
Observations	600607	568091	534994	534902
R^2	0.025	0.233	0.237	0.238
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Ecarts-types robustes reportés entre parenthèses (cluster au niveau des entreprises)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 16: Impact of CETC on employment of employees

	(1)	(2)	(3)	(4)
Placebo test	-0.0552	-0.451***	-0.427***	-0.386***
	(0.126)	(0.0947)	(0.101)	(0.103)
Double difference				
Intention to treat intensity 2013	0.0897	-0.675***	-0.703***	-0.685***
	(0.116)	(0.0881)	(0.0917)	(0.0935)
Intention to treat intensity 2014	-0.135	-0.683***	-0.587***	-0.526***
	(0.0841)	(0.0680)	(0.0711)	(0.0743)
Observations	1575088	1555338	1443379	1443032
R^2	0.601	0.944	0.946	0.946
Triple difference				
Intention to treat intensity 2013	-0.193	-0.455**	-0.452**	-0.449**
	(0.144)	(0.153)	(0.159)	(0.161)
Intention to treat intensity 2014	0.145	-0.276	-0.613*	-0.437
	(0.260)	(0.275)	(0.291)	(0.301)
Observations	1137222	1114875	1038707	1038494
R^2	0.003	0.203	0.206	0.206
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 17: Impact of CETC on employment of employees (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-1.467	-2.610*	-2.332*	-2.169
	(1.778)	(1.198)	(1.188)	(1.162)
Double difference				
Intention to treat intensity 2013	-4.835***	-1.627*	-1.710*	-1.644*
	(1.176)	(0.728)	(0.759)	(0.749)
Intention to treat intensity 2014	-3.009**	-1.558*	-1.427*	-1.452*
	(0.998)	(0.680)	(0.662)	(0.654)
Observations	1575088	1555338	1443379	1443032
R^2	0.859	0.992	0.992	0.992
Triple difference				
Intention to treat intensity 2013	1.585	-0.239	-0.399	-0.249
	(1.518)	(1.266)	(1.283)	(1.265)
Intention to treat intensity 2014	4.680	0.725	-1.088	-0.904
	(2.904)	(2.374)	(2.318)	(2.279)
Observations	1137222	1114875	1038707	1038494
R^2	0.026	0.249	0.257	0.257
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 18: Impact of CETC on employment of blue-collars

	(1)	(2)	(3)	(4)
Placebo test	-0.458**	0.0206	0.0545	0.0772
	(0.159)	(0.113)	(0.117)	(0.119)
Double difference				
Intention to treat intensity 2013	0.435**	-0.430***	-0.520***	-0.488***
	(0.147)	(0.107)	(0.109)	(0.110)
Intention to treat intensity 2014	0.132	-0.645***	-0.523***	-0.515***
	(0.106)	(0.0808)	(0.0826)	(0.0839)
Observations	1303700	1275183	1219684	1219398
R^2	0.652	0.949	0.949	0.949
Triple difference				
Intention to treat intensity 2013	-0.637***	-1.102***	-1.146***	-1.093***
	(0.174)	(0.186)	(0.190)	(0.193)
Intention to treat intensity 2014	-1.300***	-2.199***	-2.635***	-2.484***
	(0.311)	(0.330)	(0.340)	(0.350)
Observations	924834	900648	865529	865389
R^2	0.004	0.208	0.212	0.212
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 19: Impact of CETC on employment of blue-collars (weighted)

	(1)	(2)	(3)	(4)
Placebo test	0.881	-1.179	-0.807	-0.639
	(2.415)	(1.495)	(1.442)	(1.418)
Double difference				
Intention to treat intensity 2013	-3.025	-0.882	-1.299	-1.269
	(1.873)	(1.048)	(1.061)	(1.060)
Intention to treat intensity 2014	-3.264*	-2.355*	-2.657**	-2.637**
	(1.316)	(1.025)	(1.013)	(0.994)
Observations	1303700	1275183	1219684	1219398
R^2	0.851	0.992	0.992	0.992
Triple difference				
Intention to treat intensity 2013	-1.084	-2.599	-2.629	-2.694
	(1.910)	(1.864)	(1.926)	(1.921)
Intention to treat intensity 2014	-2.766	-8.186*	-9.042**	-8.873**
	(3.673)	(3.292)	(3.305)	(3.330)
Observations	924834	900648	865529	865389
R^2	0.045	0.261	0.272	0.272
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

A.3 Par sexe

Table 20: Impact of CETC on men employment

	(1)	(2)	(3)	(4)
Placebo test	-0.641***	0.0221	-0.00194	0.00206
	(0.104)	(0.0601)	(0.0622)	(0.0635)
Double difference				
Intention to treat intensity 2013	-0.00640	-0.406***	-0.443***	-0.434***
	(0.0994)	(0.0640)	(0.0646)	(0.0652)
Intention to treat intensity 2014	0.0717	-0.195***	-0.336***	-0.316***
	(0.0738)	(0.0536)	(0.0541)	(0.0556)
Observations	1759050	1753754	1671000	1670619
R^2	0.663	0.966	0.966	0.966
Triple difference				
Intention to treat intensity 2013	-0.258**	-0.439***	-0.456***	-0.442***
	(0.0857)	(0.0872)	(0.0892)	(0.0914)
Intention to treat intensity 2014	0.395*	-0.317	-0.841***	-0.781***
	(0.174)	(0.179)	(0.185)	(0.191)
Observations	1306841	1297615	1244137	1243920
R^2	0.005	0.296	0.299	0.299
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 21: Impact of CETC on men employment (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-4.776***	-1.186*	-0.710	-0.658
	(1.363)	(0.486)	(0.509)	(0.506)
Double difference				
Intention to treat intensity 2013	-3.999***	0.104	0.0375	0.0228
	(0.974)	(0.301)	(0.298)	(0.297)
Intention to treat intensity 2014	-2.415**	0.117	0.153	0.112
	(0.791)	(0.414)	(0.352)	(0.346)
Observations	1759050	1753754	1671000	1670619
R^2	0.899	0.997	0.997	0.997
Triple difference				
Intention to treat intensity 2013	2.337**	1.103*	0.930	0.942
	(0.742)	(0.553)	(0.553)	(0.545)
Intention to treat intensity 2014	2.992	0.121	-0.000684	0.0149
	(1.768)	(1.576)	(1.419)	(1.379)
Observations	1306841	1297615	1244137	1243920
R^2	0.049	0.384	0.385	0.385
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 22: Impact of CETC on women employment

	(1)	(2)	(3)	(4)
Placebo test	-0.404***	-0.101	-0.0388	-0.0357
	(0.108)	(0.0637)	(0.0673)	(0.0682)
Double difference				
Intention to treat intensity 2013	-0.0713	-0.428***	-0.421***	-0.390***
	(0.103)	(0.0673)	(0.0697)	(0.0702)
Intention to treat intensity 2014	-0.0938	-0.400***	-0.432***	-0.381***
	(0.0760)	(0.0562)	(0.0582)	(0.0608)
Observations	1710076	1704058	1579937	1579566
R^2	0.625	0.964	0.965	0.965
Triple difference				
Intention to treat intensity 2013	-0.119	-0.259**	-0.316***	-0.292**
	(0.0897)	(0.0915)	(0.0956)	(0.0981)
Intention to treat intensity 2014	0.00769	-0.391*	-0.910***	-0.793***
	(0.185)	(0.190)	(0.201)	(0.211)
Observations	1269062	1259353	1172635	1172407
R^2	0.004	0.289	0.292	0.292
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 23: Impact of CETC on women employment (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-4.723**	-1.462***	-0.917*	-0.875*
	(1.511)	(0.396)	(0.407)	(0.404)
Double difference				
Intention to treat intensity 2013	-5.108***	-0.213	-0.261	-0.279
	(0.968)	(0.304)	(0.301)	(0.301)
Intention to treat intensity 2014	-3.255***	-0.319	-0.255	-0.315
	(0.857)	(0.446)	(0.385)	(0.383)
Observations	1710076	1704058	1579937	1579566
R^2	0.883	0.997	0.997	0.997
Triple difference				
Intention to treat intensity 2013	2.408**	1.211*	1.055	1.062
	(0.799)	(0.607)	(0.608)	(0.596)
Intention to treat intensity 2014	3.070	0.502	0.324	0.408
	(1.842)	(1.588)	(1.445)	(1.412)
Observations	1269062	1259353	1172635	1172407
R^2	0.044	0.380	0.382	0.382
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

A.4 By type of contract

Table 24: Impact of CETC on permanent jobs

	(1)	(2)	(3)	(4)
Placebo test	-0.578***	0.199***	0.278***	0.304***
	(0.0858)	(0.0500)	(0.0525)	(0.0534)
Double difference				
Intention to treat intensity 2013	0.319***	0.265***	0.295***	0.301***
	(0.0814)	(0.0524)	(0.0535)	(0.0541)
Intention to treat intensity 2014	0.233***	0.222***	0.320***	0.323***
	(0.0594)	(0.0426)	(0.0431)	(0.0473)
Observations	1896865	1896082	1767851	1767439
R^2	0.707	0.967	0.968	0.968
Triple difference				
Intention to treat intensity 2013	0.141	0.246**	0.246**	0.276***
	(0.0760)	(0.0771)	(0.0796)	(0.0822)
Intention to treat intensity 2014	-0.527***	-0.499***	-0.0350	0.246
	(0.139)	(0.140)	(0.146)	(0.161)
Observations	1415726	1411397	1321919	1321669
R^2	0.005	0.255	0.263	0.263
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 25: Impact of CETC on permanent jobs (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-2.965*	-1.780*	-1.474*	-1.398*
	(1.319)	(0.726)	(0.695)	(0.684)
Double difference				
Intention to treat intensity 2013	-2.723**	-0.219	-0.289	-0.191
	(0.906)	(0.401)	(0.419)	(0.412)
Intention to treat intensity 2014	-2.256***	-0.377	-0.419	-0.332
	(0.642)	(0.284)	(0.291)	(0.286)
Observations	1896865	1896082	1767851	1767439
R^2	0.859	0.994	0.994	0.994
Triple difference				
Intention to treat intensity 2013	1.978*	0.739	0.840	1.003
	(0.868)	(0.613)	(0.619)	(0.633)
Intention to treat intensity 2014	2.213	-0.539	-0.496	-0.304
	(1.717)	(1.151)	(1.025)	(1.048)
Observations	1415726	1411397	1321919	1321669
R^2	0.031	0.310	0.329	0.329
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 26: Impact of CETC on fixed term contracts

	(1)	(2)	(3)	(4)
Placebo test	0.197	-0.601***	-0.536**	-0.481**
	(0.194)	(0.174)	(0.184)	(0.185)
Double difference				
Intention to treat intensity 2013	1.797***	0.852***	1.008***	1.009***
	(0.199)	(0.197)	(0.205)	(0.205)
Intention to treat intensity 2014	1.284***	0.482***	0.657***	0.694***
	(0.141)	(0.145)	(0.151)	(0.158)
Observations	1123096	1059876	1003032	1002776
R^2	0.433	0.852	0.853	0.853
Triple difference				
Intention to treat intensity 2013	1.458***	1.922***	2.081***	2.196***
	(0.295)	(0.344)	(0.357)	(0.374)
Intention to treat intensity 2014	0.0237	0.619	0.200	0.794
	(0.509)	(0.634)	(0.664)	(0.760)
Observations	688308	605976	577498	577390
R^2	0.033	0.244	0.246	0.247
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 27: Impact of CETC on fixed term contracts (weighted)

	(1)	(2)	(3)	(4)
Placebo test	0.479	-1.188	-0.746	-0.434
	(1.888)	(0.836)	(0.808)	(0.805)
Double difference				
Intention to treat intensity 2013	1.195	1.261	1.295	1.140
	(1.687)	(1.485)	(1.531)	(1.515)
Intention to treat intensity 2014	0.111	0.574	0.802	0.609
	(1.303)	(1.183)	(1.210)	(1.181)
Observations	1123096	1059876	1003032	1002776
R^2	0.751	0.965	0.966	0.966
Triple difference				
Intention to treat intensity 2013	1.998	1.667	1.923	1.344
	(1.765)	(1.736)	(1.799)	(1.803)
Intention to treat intensity 2014	0.578	0.0498	0.570	-0.000286
	(2.678)	(2.413)	(2.340)	(2.314)
Observations	688308	605976	577498	577390
R^2	0.239	0.450	0.454	0.455
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

B Complementary results on wages

B.1 All workers

Table 28: Impact of CETC on average wage

	(1)	(2)	(3)	(4)
Placebo test	0.308***	1.695***	1.786***	1.838***
	(0.0739)	(0.0555)	(0.0583)	(0.0629)
Double difference				
Intention to treat intensity 2013	0.413***	1.821***	1.775***	1.775***
	(0.0691)	(0.0553)	(0.0568)	(0.0570)
Intention to treat intensity 2014	0.942***	1.924***	1.797***	1.842***
	(0.0517)	(0.0444)	(0.0457)	(0.0480)
Observations	1918585	1918584	1789248	1788824
R^2	0.478	0.904	0.907	0.907
Triple difference				
Intention to treat intensity 2013	-0.117	0.885***	0.943***	0.951***
	(0.0840)	(0.0861)	(0.0872)	(0.0959)
Intention to treat intensity 2014	-1.811***	0.0958	3.034***	3.386***
	(0.165)	(0.167)	(0.174)	(0.206)
Observations	1438938	1438938	1348159	1347902
R^2	0.005	0.220	0.250	0.251
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Table 29: Impact of CETC on average wage (weighted)

	(1)	(2)	(3)	(4)
Placebo test	1.697*	0.733*	0.644	0.823*
	(0.774)	(0.356)	(0.347)	(0.349)
Double difference				
Intention to treat intensity 2013	-0.0936	0.375	0.400	0.405
	(0.545)	(0.247)	(0.249)	(0.253)
Intention to treat intensity 2014	0.360	0.625*	0.508*	0.572*
	(0.414)	(0.275)	(0.258)	(0.258)
Observations	1918584	1918583	1789247	1788823
R^2	0.908	0.997	0.997	0.997
Triple difference				
Intention to treat intensity 2013	-1.396**	-0.386	-0.0781	-0.176
	(0.519)	(0.542)	(0.554)	(0.567)
Intention to treat intensity 2014	-2.053	-0.224	1.358	1.436
	(1.348)	(1.348)	(1.197)	(1.175)
Observations	1438938	1438938	1348159	1347902
R^2	0.036	0.215	0.240	0.240
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 30: Impact of CETC on wage growth

	(1)	(2)	(3)	(4)
Placebo test	-0.197***	0.538***	0.800***	0.788***
	(0.0310)	(0.0307)	(0.0294)	(0.0303)
Double difference				
Intention to treat intensity 2013	0.0499*	0.604***	0.723***	0.715***
	(0.0250)	(0.0249)	(0.0247)	(0.0258)
Intention to treat intensity 2014	-0.284***	0.0873***	0.778***	0.776***
	(0.0159)	(0.0157)	(0.0168)	(0.0175)
Observations	1781333	1776155	1658600	1658204
R^2	0.020	0.266	0.354	0.354
Triple difference				
Intention to treat intensity 2013	-0.0516	0.560***	0.632***	0.625***
	(0.0508)	(0.0522)	(0.0486)	(0.0496)
Intention to treat intensity 2014	-4.141***	-2.882***	1.353***	1.392***
	(0.0885)	(0.0884)	(0.0880)	(0.0917)
Observations	1273838	1236017	1160251	1160022
R^2	0.011	0.138	0.298	0.298
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 31: Impact of CETC on wage growth (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-0.375**	0.254	0.277*	0.334
	(0.139)	(0.146)	(0.138)	(0.173)
Double difference				
Intention to treat intensity 2013	0.0681	0.529***	0.620***	0.529***
	(0.121)	(0.114)	(0.118)	(0.134)
Intention to treat intensity 2014	-0.245***	0.0653	0.498***	0.456***
	(0.0715)	(0.0668)	(0.0786)	(0.0833)
Observations	1781333	1776155	1658600	1658204
R^2	0.045	0.286	0.326	0.327
Triple difference				
Intention to treat intensity 2013	0.153	0.787**	0.898***	0.726*
	(0.237)	(0.240)	(0.245)	(0.303)
Intention to treat intensity 2014	-3.488***	-2.169***	0.767	0.471
	(0.376)	(0.370)	(0.407)	(0.466)
Observations	1273838	1236017	1160251	1160022
R^2	0.032	0.177	0.259	0.259
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

B.2 By socio-professional category

Table 32: Impact of CETC on wage growth for intermediate professions

	(1)	(2)	(3)	(4)
Placebo test	-0.335***	0.369***	0.553***	0.563***
	(0.0551)	(0.0539)	(0.0554)	(0.0565)
Double difference				
Intention to treat intensity 2013	-0.191***	0.376***	0.473***	0.410***
	(0.0451)	(0.0441)	(0.0452)	(0.0520)
Intention to treat intensity 2014	-0.408***	-0.0154	0.528***	0.491***
	(0.0280)	(0.0286)	(0.0308)	(0.0366)
Observations	690170	653108	613057	612927
R^2	0.013	0.306	0.350	0.350
Triple difference				
Intention to treat intensity 2013	-0.147	0.486***	0.577***	0.466***
	(0.0883)	(0.0926)	(0.0924)	(0.110)
Intention to treat intensity 2014	-3.565***	-2.239***	1.076***	0.860***
	(0.151)	(0.159)	(0.166)	(0.220)
Observations	440461	403630	380194	380126
R^2	0.009	0.157	0.247	0.247
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 33: Impact of CETC on wage growth for intermediate professions (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-0.552**	-0.109	0.0143	0.146
	(0.194)	(0.221)	(0.206)	(0.255)
Double difference				
Intention to treat intensity 2013	-0.291	0.255	0.348*	0.181
	(0.156)	(0.138)	(0.140)	(0.194)
Intention to treat intensity 2014	-0.281**	0.0784	0.498***	0.448***
	(0.0944)	(0.0964)	(0.0951)	(0.105)
Observations	690170	653108	613057	612927
R^2	0.049	0.318	0.344	0.346
Triple difference				
Intention to treat intensity 2013	0.305	0.890**	0.892**	0.553
	(0.334)	(0.330)	(0.345)	(0.491)
Intention to treat intensity 2014	-2.187***	-1.046*	1.918***	1.498*
	(0.533)	(0.527)	(0.580)	(0.714)
Observations	440461	403630	380194	380126
R^2	0.042	0.200	0.262	0.263
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 34: Impact of CETC on growth wage of employees

	(1)	(2)	(3)	(4)
Placebo test	-0.0959*	0.390***	0.558***	0.539***
	(0.0445)	(0.0434)	(0.0451)	(0.0463)
Double difference				
Intention to treat intensity 2013	-0.0794*	0.272***	0.349***	0.344***
	(0.0386)	(0.0358)	(0.0373)	(0.0381)
Intention to treat intensity 2014	-0.272***	-0.0298	0.503***	0.490***
	(0.0221)	(0.0222)	(0.0241)	(0.0252)
Observations	1206404	1173810	1083046	1082776
R^2	0.010	0.278	0.322	0.322
Triple difference				
Intention to treat intensity 2013	-0.225**	0.0940	0.189*	0.198**
	(0.0703)	(0.0738)	(0.0750)	(0.0760)
Intention to treat intensity 2014	-2.593***	-1.919***	1.450***	1.529***
	(0.122)	(0.122)	(0.130)	(0.137)
Observations	815067	767166	710571	710414
R^2	0.006	0.134	0.219	0.220
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 35: Impact of CETC on growth wage of employees (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-0.437*	0.0427	0.0577	0.0139
	(0.208)	(0.192)	(0.183)	(0.182)
Double difference				
Intention to treat intensity 2013	-0.0255	0.217	0.310*	0.286
	(0.172)	(0.148)	(0.158)	(0.159)
Intention to treat intensity 2014	-0.235	-0.0229	0.371*	0.335*
	(0.169)	(0.154)	(0.154)	(0.160)
Observations	1206404	1173810	1083046	1082776
R^2	0.037	0.313	0.337	0.337
Triple difference				
Intention to treat intensity 2013	0.145	0.563*	0.647*	0.631*
	(0.292)	(0.286)	(0.302)	(0.303)
Intention to treat intensity 2014	-2.283***	-1.566*	0.962	0.807
	(0.602)	(0.641)	(0.639)	(0.664)
Observations	815067	767166	710571	710414
R^2	0.029	0.189	0.242	0.243
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 36: Impact on growth of wages of employees

	(1)	(2)	(3)	(4)
Placebo test	-0.277***	0.319***	0.662***	0.609***
	(0.0545)	(0.0509)	(0.0499)	(0.0532)
Double difference				
Intention to treat intensity 2013	-0.0684	0.327***	0.366***	0.304***
	(0.0451)	(0.0438)	(0.0432)	(0.0493)
Intention to treat intensity 2014	-0.352***	-0.0944***	0.629***	0.620***
	(0.0285)	(0.0273)	(0.0280)	(0.0292)
Observations	974180	946838	932376	932179
R^2	0.018	0.285	0.353	0.353
Triple difference				
Intention to treat intensity 2013	-0.110	0.270**	0.102	0.0144
	(0.0843)	(0.0884)	(0.0851)	(0.0970)
Intention to treat intensity 2014	-3.018***	-2.093***	1.867***	2.050***
	(0.145)	(0.146)	(0.147)	(0.167)
Observations	661532	626244	618627	618534
R^2	0.008	0.125	0.248	0.248
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 37: Impact on growth of wages of blue-collars (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-0.679*	-0.171	-0.125	-0.189
	(0.278)	(0.275)	(0.264)	(0.264)
Double difference				
Intention to treat intensity 2013	-0.164	0.244	0.298	0.270
	(0.181)	(0.175)	(0.183)	(0.184)
Intention to treat intensity 2014	-0.393**	-0.0893	0.410**	0.336**
	(0.150)	(0.133)	(0.127)	(0.128)
Observations	974180	946838	932376	932179
R^2	0.053	0.327	0.358	0.358
Triple difference				
Intention to treat intensity 2013	0.353	1.048*	0.978*	1.001*
	(0.402)	(0.418)	(0.414)	(0.415)
Intention to treat intensity 2014	-2.690***	-1.506	1.248	0.904
	(0.759)	(0.828)	(0.787)	(0.802)
Observations	661532	626244	618627	618534
R^2	0.039	0.164	0.228	0.228
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

B.3 By type of contract

Table 38: Impact on growth of wages for permanent contracts

	(1)	(2)	(3)	(4)
Placebo test	-0.197***	0.532***	0.792***	0.777***
	(0.0308)	(0.0304)	(0.0292)	(0.0303)
Double difference				
Intention to treat intensity 2013	0.0514*	0.605***	0.721***	0.711***
	(0.0246)	(0.0245)	(0.0244)	(0.0261)
Intention to treat intensity 2014	-0.288***	0.0861***	0.779***	0.774***
	(0.0159)	(0.0156)	(0.0168)	(0.0179)
Observations	1765100	1758896	1642561	1642175
R^2	0.020	0.263	0.358	0.358
Triple difference				
Intention to treat intensity 2013	-0.0458	0.555***	0.621***	0.615***
	(0.0503)	(0.0517)	(0.0482)	(0.0497)
Intention to treat intensity 2014	-4.118***	-2.872***	1.385***	1.447***
	(0.0877)	(0.0877)	(0.0873)	(0.0945)
Observations	1257523	1217018	1142419	1142195
R^2	0.012	0.144	0.317	0.317
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 39: Impact on growth of wages for permanent contracts (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-0.446*	0.188	0.254	0.326
	(0.181)	(0.161)	(0.166)	(0.195)
Double difference				
Intention to treat intensity 2013	0.119	0.593***	0.688***	0.567***
	(0.146)	(0.141)	(0.146)	(0.171)
Intention to treat intensity 2014	-0.266**	0.0838	0.508***	0.451***
	(0.0820)	(0.0779)	(0.0925)	(0.0994)
Observations	1765100	1758896	1642561	1642175
R^2	0.055	0.289	0.324	0.325
Triple difference				
Intention to treat intensity 2013	0.372	1.041***	1.151***	0.912*
	(0.304)	(0.307)	(0.324)	(0.388)
Intention to treat intensity 2014	-3.387***	-2.077***	0.752	0.410
	(0.508)	(0.517)	(0.520)	(0.575)
Observations	1257523	1217018	1142419	1142195
R^2	0.042	0.186	0.260	0.260
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 40: Impact on wage growth for fixed-term contracts

	(1)	(2)	(3)	(4)
Placebo test	-0.175	0.411	0.688**	0.672*
	(0.235)	(0.248)	(0.257)	(0.261)
Double difference				
Intention to treat intensity 2013	-0.666**	0.294	0.290	0.304
	(0.233)	(0.268)	(0.280)	(0.282)
Intention to treat intensity 2014	-0.750***	-0.0149	0.422*	0.454*
	(0.158)	(0.193)	(0.203)	(0.204)
Observations	164183	116926	111674	111625
R^2	0.044	0.487	0.493	0.493
Triple difference				
Intention to treat intensity 2013	-0.264	-0.274	-0.295	-0.272
	(0.456)	(0.602)	(0.620)	(0.624)
Intention to treat intensity 2014	-2.674***	-3.020*	0.335	0.445
	(0.811)	(1.206)	(1.265)	(1.269)
Observations	65167	41856	40108	40097
R^2	0.019	0.196	0.219	0.219
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.

Table 41: Impact on wage growth for fixed term contracts (weighted)

	(1)	(2)	(3)	(4)
Placebo test	-0.274	0.675	0.715	0.649
	(0.801)	(0.871)	(0.897)	(0.897)
Double difference				
Intention to treat intensity 2013	0.430	1.630	1.723	1.767
	(0.885)	(0.994)	(1.025)	(1.004)
Intention to treat intensity 2014	-0.00402	0.703	0.727	0.733
	(0.516)	(0.612)	(0.600)	(0.599)
Observations	164183	116926	111674	111625
R^2	0.184	0.554	0.558	0.558
Triple difference				
Intention to treat intensity 2013	1.498	1.783	1.932	2.024
	(1.683)	(2.017)	(2.099)	(2.053)
Intention to treat intensity 2014	-4.656*	-4.397	-2.439	-2.389
	(2.362)	(3.263)	(3.386)	(3.360)
Observations	65167	41856	40108	40097
R^2	0.128	0.296	0.309	0.309
Sectoral × year fixed effects	√	√	√	√
Size × year fixed effects	√	√	√	√
Firm fixed effects		√	√	√
LIEPP controls			√	√
SMIC controls				√

Robust standards errors in parentheses (clustered at the firm-level)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Source : DADS, FARE, MVC 2010-2014.