Do Hiring Credits Work in Recessions? Evidence from France

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Abstract

This paper evaluates the impact of a substantial but temporary hiring credit targeted at workers paid below 1.6 times the minimum wage in firms with less than 10 employees in France from December 2008 to December 2009. Using rich administrative data covering all French firms, we find that the program has had a strong and fast impact on employment. The net cost per job created for the government was around zero. The employment effect varied across industries and was stronger in areas where recruitment was easier. Although the hiring credit was not conditional on net job creation, it did not increase churning of workers. Nevertheless, at constant budget, a credit conditional on net job creation above the employment growth threshold of −1%, would have maximized job creation, and created 1.8 times more jobs, provided that take-up had remained the same.

Keywords: JEL : C31, C93, J6

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1 Introduction

At the onset of the Great Recession, on 4 December 2008, the French President Nicolas Sarkozy announced that workers paid below 1.6 times the minimum wage hired in firms with less than 10 employees were immediately eligible to employer social contributions relief until 31 December 2009. The amount of this relief was substantial. It reduced the labor cost by 12% at the level of the minimum wage. It was linearly decreasing in the gross hourly wage level up to 1.6 times the minimum wage. This paper evaluates the impact of this hiring credit, called zéro charges.

Hiring credits have been used in the United States and in a number of European countries to counteract the employment effects of the 2008-2009 recession. New hires sometimes benefited at this occasion from very significant hiring credits at a high cost for the public purse. In some countries (e.g. Portugal, Ireland), hiring credits were targeting long-term unemployed or other disadvantaged groups and required firms to have net employment growth over the relief period. In other countries (e.g. France and Spain), the scope of hiring credits was larger and no such condition on net employment growth was required.

Although hiring credits have been widely used to foster employment during recessions, very little is known about their effects. Many economists think that they are probably useless in recessions, where aggregate demand is insufficient relative to the economy’s labor and other resources. Despite these limitations, Neumark (2013) concludes, in a survey of the literature, that hiring credits may have significant effects on employment when they are not targeted on specific demographic groups. Neumark argues that hiring credits help to circumvent short-run downward wage rigidities that prevent the economy from washing out severe aggregate demand shocks. However, as stressed by Neumark, the evidence is very scarce and we need to know more about the effects of these types of hiring credits.

The zéro charges program provides an exceptional opportunity to contribute to filling this gap. This policy has several important advantages for the evaluation purpose. First, it came as a real surprise: it was announced and implemented on the same day, and kept secret before its announcement. Second, since for fiscal reasons only firms with less than 10 employees before the announcement of the measure were eligible, the hiring credit was arbitrarily restricted to a subset of firms comparable to others that were not eligible. Third, no other new policy was targeted at small firms or at low wage workers at the same time. Fourth, there are no significant discontinuities at the 10 employees threshold in the French legislation, that would induce a change in the labor cost or in the labor regulations. These features make the implementation of zéro charges a natural experiment which allows us to evaluate (for the first time, to the best of our knowledge) the consequences of a non categorical temporary hiring credit at the firm level with a proper identification strategy. Moreover, we are able to analyze in detail the consequences of this natural experiment to the extent that we use a comprehensive database providing information about employment, hours, hires, separations and wages for all firms on a daily basis since 2005.

\footnote{For instance, in Portugal, employer social security contributions were eliminated for the first three years of employment for new hires of certain groups of jobseekers, but only within firms having a net employment growth over that period except for new hires of people older unemployed. Also, Ireland has eliminated employer social security contributions for one year for new hires (in addition to existing staff) of people unemployed for 6 months or more.}

\footnote{In Spain, a 1500 euros per year social contribution rebate applies for two years to new hires of workers with family responsibilities on permanent contracts. See OECD (2010) for a detailed presentation of hiring credit measures in 2009.}
Using a difference-in-differences strategy, we compare the evolution of small firms (between 6 and 10 employees) and medium-size firms (between 10 and 14 employees) from November 2008, just before the introduction of the hiring credit, until November 2009. The estimated elasticity of employment with respect to the drop in labor cost induced by the hiring credit is about $-4$, a very high (absolute) value which can be explained by the fact that the measure was targeted at low wage workers in the context of a high minimum wage and high unemployment, but also by the fact that the hiring credit was only for hires. If the measure had concerned all jobs instead, we show that the corresponding elasticity would have been about $-1.1$. This finding is consistent with previous estimates which showed that movements in the cost of French minimum wage workers are associated with very strong negative employment effects (Kramarz and Philippon, 2001, and Abowd et al. 2006). The impact of the hiring credit emerged quickly: hires and employment began to rise three months after the introduction of the credit. The evolution of hours worked is similar to that of employment, meaning that firms did not substitute hours of new workers benefiting from the hiring credit for that of incumbent employees. We find no increase in wages and in separation rates associated with the hiring credit.

We proceed to robustness checks — varying bandwidth, placebo analysis, search for equilibrium effects — and find that these results are stable. Accordingly, we conclude that hiring credits can be effective to boost employment of low wage workers in recessions when there is a high minimum wage.

Our contribution also sheds light on the design of hiring credits. The widespread concern that the principal effect of hiring credits may be to raise excess worker turnover without any significant impact on employment generates many debates on the optimal design of hiring credit programs. However, the lack of reliable information leads to convoluted policy decisions with very uncertain consequences. For instance, in the US the original proposal by the Carter administration in 1977 regarding what would become the New Job Tax Credit (NJTC) was a hiring credit with no conditionality on net employment growth. Following many debates, the Congress eventually passed the NJTC which targeted growing firms. This came at the cost of a considerably more complex measure. Despite this complexity, among the 147 hiring credits enacted by U.S. states from 1969 to 2012, 143 required that the number of jobs associated with hires is above specified thresholds. In 2009, the Obama administration originally set out a hiring credit targeting growing firms, while the Congress passed a measure, in the HIRE Act, which exempted all employers of their share of Social Security taxes on wages when hiring unemployed workers into new or existing positions. Like zéro charges, the HIRE Act was not restricted to growing firms and it was accessible to a rather large pool of potential candidates.

We find that zéro charges did not induce firms to increase layoffs in order to hire workers at lower cost. We could argue that this is because there is little churning in France. As shown by Abowd et al. (1999), this is not the case: the level of churning is high in particular because

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3Sunley (1980) provides a detailed description of the convoluted policy discussions and legislative history surrounding the New Job Tax Credit.

4Neumark and Grijalva (2013)

5Employers who hired unemployed workers into new or existing positions between February and December 2010, could be exempted from their share of Social Security taxes on wages paid to these workers (reduction of around 6.2% in labor cost for a full-time low-wage worker). In addition, for each worker retained for at least a year, businesses may claim an additional general business tax credit, up to $1,000 per worker. The only condition is that the new hire was unemployed during the 60 days before beginning work or, alternatively, worked no more than 40 hours for anyone during the 60-day period. Household employers were ineligible as well as are employees who earned more than $106,000 per year. See http://hireact.org/
90 percent of entries into employment are on temporary jobs. As a consequence of this high churning, the gross cost of the hiring credit per job created born by the government is significant. It amounts to about a quarter of the labor cost of one of these jobs. We estimate that about 84 percent of this cost goes to hires that would have been created absent the hiring credit, meaning that there are large windfalls for firms. This leads us to explore the potential impact of hiring credit conditional on net job creation. We find that at constant budget, a credit conditional on net job creation above the employment growth rate of $-1\%$, would have maximized job creation, creating 1.8 times more jobs than the non conditional hiring credit, provided that take-up had remained the same.

Our paper is related to empirical contributions devoted to the impact of hiring credits and job subsidies. The literature on the effect of hiring credits has been recently surveyed in Neumark (2013) who concludes that hiring credits do not have significant effects on employment when they are targeted at specific disadvantaged groups (such as long-term unemployed or disabled workers). Such targeted policies stigmatize their beneficiaries and entail substitution effects. However Neumark also concludes that non-targeted hiring credits may have significant effects on employment. The evidence, which is rather scarce, relies on two empirical evaluations of the New Job Tax Credit (NJTC), which subsidized growing firms during the late 70s. Using survey data, Perloff and Wachter (1979) compare firms declaring that they know about the NJTC and firms which do not. They conclude that employment grew 3 percent faster thanks to the NJTC. They concede that their result is an upper bound of the true effect as serious endogeneity bias affect their comparison. Using aggregate time series, Bishop (1981) concludes that the NJTC had significant positive employment effects (between 0.66 and 2.95%) and negative effects on prices. The NJTC was the only US hiring credit implemented at the federal level before the HIRE Act. At the state-level, there were many more hiring credits. Chirinko and Wilson (2010) construct a large data set documenting these policies. They conduct an event-study (difference-in-differences across US states) and estimate that the hiring credits increased employment the month when firms both know and can qualify for the hiring credit. They pay particular attention to dynamic effects and document an Ashenfelter dip between the date the legislation is signed into law and date individual employers qualify for the hiring credit. More recently, Neumark and Grijalva (2013) analyze state-level hiring credits. Using difference-in-differences across states, they do not find positive effects on job growth for many of the types of hiring credits they consider. However, some specific types of hiring credits, targeting the unemployed and those that allow states to recover credits when job creation goals are not met, appear to have succeeded in boosting job growth. Moreover Neumark and Grijalva (2013) point out that inefficiencies for certain types of hiring credits seem related to churning behaviors. In European countries, most evaluations are focused on tax exemptions that reduce labor costs. These exemptions are not targeted to hires. They apply to the stock of jobs. It is found that tax exemptions targeted to low wage jobs have positive employment effects in countries where there are high statutory minimum wages, such as France and Belgium. In countries without statutory minimum wage,

6 Crepon and Desplat (2001), Kramarz and Philippon (2001), Cheron et al (2010) and Barlet et al (2010) find positive employment effects of payroll tax exemptions for low wage workers implemented in the early 90s in France. Givord et al. (2013) find that the Zone Franche Urbaine program, comparable to US enterprise zones, which exempts businesses from taxes for a period of at least five years, had significant effects on both business creation and employment but also had significant negative spillovers on neighboring areas.

7 Goos and Konings (2007) find positive employment effect of payroll tax subsidies in Belgium.
such as Finland\textsuperscript{8} or Sweden\textsuperscript{9}, results are more mixed. We add to the literature by providing the first empirical evaluation of a temporary hiring credit in Europe using firm data. We use a much richer information than previous studies that allows us to analyze the impact of hiring credit on a wide scope of outcomes including employment, hours of work, labor turnover, wages, firms survival and cost per job created. We show that the impact of hiring credits depends on labor market conditions and in particular on hiring difficulties.

The paper is organized as follows. Section 2 describes the hiring credit scheme implemented in France in 2009 (\textit{zéro charges}). Section 3 presents the data, descriptive statistics and the empirical strategy. The results are presented in section 4. Section 5 is devoted to the cost per job created of \textit{zero charges} and to alternative hiring credit designs. Robustness checks are presented in section 6. The last section concludes.

\section{Institutional background}

The measure \textit{zéro charges} (zero contributions) was announced by the French President on the 4th of December 2008. According to the original announcement, any hire (or fixed-term contract renewal) of a low-wage worker in a firm with less than 10 employees occurring from the date of the announcement until the 31st of December 2009 could benefit during the same year from an employer social contribution relief\textsuperscript{10}. The relief is maximal for workers with an hourly wage at the minimum wage level (1,338 euros in 2009). With \textit{zéro charges} (zero contributions), employers do not pay any social contribution at the minimum wage level. The relief then decreases in the hourly wage level up to 1.6 times the minimum wage. Figure 1 shows how the hiring credit reduces the labor cost. The hiring credit reduces the labor cost by 12\% for a full-time worker paid at the minimum wage. The maximum amount of the hiring credit over 12 months represents 2,400 euros. When the wage is 30\% above the minimum wage, the subsidy rate represents 4\% of the labor cost.

Before the first announcement, the policy was not anticipated. It was kept as a secret before its announcement\textsuperscript{11}. This is illustrated by Figure 2 which shows that Google search on the item “hiring subsidy” (\textit{aide embauche}) and other related expressions started to increase in December 2008, once the announcement for the program was made. There is very little Google search for this policy before early 2009.

The practical details were quickly published by decree on the 20th of December 2008.

\textsuperscript{8}Huttunen et al. (2013) do not find any positive employment effect of wage subsidies targeted at older, full-time, low-wage workers in Finland.

\textsuperscript{9}Sianesi (2008) finds that entering a temporary job subsidy program rather than searching further in open unemployment increases employment rates soon after the program ends in Sweden in the 1990s. Bennmarker et al. (2009) do not find any positive employment effects in permanent firms of a 10\% payroll tax reduction introduced in the Northern regions of Sweden in 2002.

\textsuperscript{10}The new relief would come on top of the existing general social contribution reduction on low wages called the \textit{Fillon reduction}, which has prevailed since the 1990s and concerns all firms in the private sector.

\textsuperscript{11}See for instance the newspaper \textit{Les Echos}, that describes in a paper entitled “Le gouvernement envisage d’accélérer ses paiements et remboursements aux entreprises”, published on 27 November 2008, all potential measures that the President Sarkozy was supposed to announce in the Press conference of the 4 December 2008. The hiring credit is not mentionned in this paper. On 4 December 2008, the paper entitled, “Sarkozy dévoile un plan de 26 milliards d’euros pour relancer l’économie”, that summarizes the contents of the press conference, mentions the hiring credit.
Figure 1: The continuous line displays the labor cost without the hiring credit. The dotted line shows the labor cost with the hiring credit. The horizontal axis reports the monthly wage (in euros) of a full time worker.

Figure 2: Results of google search for the policy name. Source: google trends website.
First, only firms and associations belonging to the private sector could get the hiring credit. Firms and associations should ask for the additional relief *zéro charges* for each hire separately, filling out a one-page form and attaching the labor contract. The claim had to be sent to the French public employment agency (Pôle emploi).

Second, to be sponsored, hires should concern jobs lasting at least one month, and not otherwise sponsored by other targeted special measures, such as even more generous and pre-existing subsidies for some disadvantaged groups (e.g. long-term unemployed) or apprentices; household jobs were also excluded on the ground of their specific and pre-existing subsidies. The hiring credit was not restricted to firms with net employment growth, and it was not limited to the hiring of long-term unemployed or any other disadvantaged groups.

Third, only entities with less than 10 full-time equivalent employees on average between January and November 2008 could apply. Hence, the period used to define the size criteria ends before the announcement of the policy, on 4 December 2008. This means that the size criteria cannot be manipulated by firms wishing to benefit from the hiring credit. Moreover a growing firm reaching 10 or more employees over the year 2009 can still continue to receive subsidies or apply for new hires until the end of 2009.

Fourth, applying firms should not have fired any workers for economic reasons on the same job over the 6 months preceding the hiring date, nor should they have fired this particular worker on the same period on any other jobs, and they should have paid all their previous social contributions. Apart from the payment of social contributions, we believe that those criteria are not really credible as they are costly to verify.

On 16 November 2009, the policy was extended to hires occurring up to 30 June 2010. At this occasion, the duration of the hiring credit was extended for up to 12 months from the hiring date, instead of 31 December 2009 for the initial scheme. This new rule was also applicable to hires made in 2009 before the announcement and which already benefited from *zéro charges*. Firms below the average of 10 full-time equivalent employees from January 2009 to December 2009 were eligible to the extended program for their new hires in 2010. Hence, studying effects of the policy in 2010 is more challenging as some firms treated in 2009 may not be able to apply in 2010, because eligibility for the extended period was then based on the average size over 2009.

The hiring credit was initially part of a larger set of policies designated to cope with the 2008-2009 crisis. In that set, this is the only item specifically targeted at small firms, and the only item directly altering the labor cost. The hiring credits were targeted at small firms because of budget constraints. Broadly speaking, there is no other legal changes in this period that explicitly affect differently firms with less or more than 10 employees.

Moreover there are no significant discontinuities at the 10 employees threshold in the French legislation, inducing a change in the labor cost or in the labor regulations (see Ceci-Renaud and Chevalier, 2010). As a consequence we do not see any accumulation of firms just below

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12The size criteria is very precise (see cerfa n° 13838-01). Only ordinary employees are kept in the computation of the size (excluding apprentices and sponsored employees). The size is computed as the average of the end-of-month number of employees from January to November 2008. Employees contributes pro rata temporis, excluding overtime hours. This means that wage earners hired on the 15th of the month and working full-time represent 0.5 employees, as do wage-earners hired at th ebeginning of the month but working part-time (only mornings). The rule is slightly different for workers on permanent contract, then regular hours are considered to compute the time ratio (even if they were hired in the middle of the month).
the threshold (see Figure 3). We can thus be confident on the absence of sorting around the
size threshold (such a sorting could have meant that firms below and over the threshold react
differently to the business cycles).  

3 Data and empirical strategy

3.1 Data

We use administrative data from two distinct sources:

- the Déclarations Administratives de Données Sociales ("DADS") built by the French Sta-
tistical Institute (Insee) from firm social contributions declarations. Firms declare each
the year employment spells, the number of hours worked, and the associated wages for
each worker.

- and the administrative file produced by the French Public Employment Agency (Pôle
emploi) which administered the payment of the subsidy, designated as the "hiring credit"

\[\text{This contrasts with the findings of Gourio and Roys (2012) who report large accumulation at the 50 employees}
\text{threshold and moderate at the 10 employees threshold. Their evidence on small firms is less reliable than ours,}
\text{as they do not observe the whole universe of small firms (their data is based on a specific fiscal declaration that}
\text{does not cover all small firms).}\]
file. It contains information on the firms which took up \textit{zéro charges}, the level of the hiring wage, and the exact amount and duration of the subsidy received.

The DADS cover around 85\% of French wage earners. Civil servants from the French central administration (ministries) and workers from the public health care sector or employed by household employers (e.g. for house-keeping or child care) do not appear in this employment register (until 2009). We append the employment registers from 2005 to 2009, creating a panel of firms.\footnote{There is no permanent identifier for individual worker. Our data is not a panel of individual worker.} We restrict the sample to firms in the for-profit private sector and drop the agriculture sector as well as associations. We also drop workers in temporary help agencies, as we do not know in which firms they actually work, as well as the 1\% of firms with the highest employment growth rates in the sample.\footnote{Further information on the data sets and on data is available in table \ref{table:14} in the appendix.} All relevant information for firm size, the number of hires, separations, the wage levels and the duration of contracts are taken from the DADS which describes the universe of firms relevant to our evaluation. The eligibility condition based on the size threshold (Full Time Equivalent) is also computed from the employment register.

Both data sets can be matched using the firm identifier. This enables us to compute the take-up rate, which corresponds to the fraction of small firms actually benefiting from the hiring credit in 2009. The take up rate amounts to 24\%. This low Figure is the product of the hiring rate of low-wage workers and the take-up rate conditional on hiring low-wage workers, which we define as the attention rate. The attention rate (the share of subsidized hires among hires with wage below 1.6 times the minimum wage and contract duration above one month) amounts to 47\%. Figure \ref{figure:4} display the take-up rate and the attention rate by firm size in 2008 (i.e. by the eligibility criteria). The take-up rate sharply decreases for firms with 8 employees or more and goes to zero for firms larger than 12 employees. Similarly the attention rate drops before the threshold and it is positive, around 3\% for firms from 10 to 12 employees.

To the extent that, as discussed above, firms were not able to manipulate their size to meet the eligibility criteria, the drop in the attention rate before the threshold of 10 employees and the positive fraction of firms from 10 to 12 employees benefiting from the hiring credit are likely the consequence of measurement error. The eligibility criterion is difficult to measure precisely in the employment register at our disposal. In particular, according to the legal rules, workers hired on permanent contracts are considered to be present in the firm from the beginning of the month, even if they have been hired during the month. Since we only observe the type of contract at the end of the year for every worker, we are unable to know whether workers have been hired on permanent or temporary contracts because temporary contracts may be converted into permanent contracts. Another reason could be that computing the eligibility criterion is a complex task especially for small firms. Only ordinary employees are kept in the size computation, excluding apprentices and diverse categories of employees benefiting from other subsidies; employees contribute pro rata temporis but overtime hours are not taken into account. These features of the eligibility criterion may induce firms to overestimate their size and to refrain from claiming \textit{zéro charges}. The resulting absence of discontinuity in the take-up rate prevents us from using a regression discontinuity design.
Figure 4: Fraction of firms and of hires that benefited from the hiring credit by firm’s size in 2009. Note: the take-up rate is the share of firms below ten employees benefiting from the hiring credit in 2009; the attention rate is the share of hires with wages below 1.6 times the minimum wage that have been subsidized in 2009. The firms’ size is the number of full time equivalent employees as measured over the first 11 months of 2008.
3.2 Empirical strategy

We aim at evaluating the impact of the hiring credit on employment and ultimately the cost per job created. This depends, as we now show, on labor turnover.

The hiring credit can influence employment through its impact on hires and on separations. To see this, let us consider the law of motion of employment which determines the level of employment at the end of the current period

\[ L = L_{-1} + H - S, \]

(1)

where \( L_{-1} \) stands for employment inherited from the previous period, \( H \) denotes the number of entries and \( S \) is the number of separations.

Hiring credit aim at increasing employment through their effect on hires. However, it is possible that firms benefit from important amounts of hiring credits while the effects on net employment are negligible. Becker (2010), reacting to the Hiring Incentives to Restore Employment (HIRE) Act enacted in 2010 in the US, argued that it will increase churning and wages with very little employment effects. Posner (2010) stated that disregarding the abundant possibilities of gaming the program, stressed by Becker, the HIRE Act was unlikely to be effective because it violated the economic principles that ought to guide stimulus programs. The main argument of Posner is that a stimulus is designed to stimulate demand, not supply, because the economic problem for which a stimulus program is a solution in a recession is insufficient demand. This reasoning is in line with new-keynesian macroeconomists who claim that employment subsidies are ineffective during recessions because low employment is the consequence of an insufficient aggregate demand (Gali, 2013). If low employment is the consequence of an insufficient demand for the products of the firms, hiring credits can induce firms to fire some employees, and then to replace them with workers for whom they can collect the subsidy. The main impact of the hiring credit may be to increase churning and wages with very little employment effects. In our context, churning is potentially an important concern to the extent that worker flows in excess of those strictly necessary to achieve a given change in employment are large in France (Abowd et al., 1999).

If the hiring credit increases employment, it is nevertheless possible that its effects on hours worked is limited, because firms have incentives to substitute hours of subsidized employees, for those of non subsidized employees. Therefore, it is important to also analyze the reply of hours of work.

In what follows, we estimate the impact of the hiring credit on employment, wages, hours of work, hires and separations. We analyze yearly cohorts of firms. We select, for each cohort \( t \), firms whose size criteria in year \( t - 1 \) is around the cut-off (that is 10 full-time-equivalent employees, calculated at the average of end-of-month pro-rata temporis headcounts between January and November of year \( t - 1 \)) and estimate the following difference-in-differences model:

\[ Y_{it} = \alpha + \beta Z_{it} + \gamma D_{it} + \delta Z_{it}D_{it} + X_{it}b + u_{it} \]

(2)

where \( Y_{it} \) is the outcome of firm \( i \) in period \( t \), \( Z_{it} \) an eligibility dummy equal to 1 if the firm size in period \( t - 1 \) is below 10, \( D_{it} \) a dummy for year 2009 when subsidies can be claimed, \( X_{it} \) a vector of covariates. \( \delta \) is our parameter of interest. It captures the differential evolution of the group targeted by the hiring credit. It can be interpreted as an Intention-To-Treat parameter. Accordingly, we refer, from now on, to firms with less than 10 employees in year \( t - 1 \) as our "treatment" group, even if they do not claim the hiring credit.
In the benchmark estimations, we chose a bandwidth between 6 (included) and 14 (excluded) full-time employees in previous year. In Table 1, we report characteristics of our 2009 cohort. These characteristics are measured in 2008. In the first three columns, we compare small and medium firms. Small (eligible) firms are less frequently operating in the manufacturing industry and slightly more often in the retail, transport and merchant services than larger firms. They are slightly more frequently located in the Parisian area and the South-Eastern part of France, and less frequently in the North-West part of France. Almost half of small firms have sales lower than 2 millions euros, while one medium-size firms out of four does. Small firms are also younger: 13 percent have less than 5 year old vs. 10 percent for medium-size firms. The composition of the workforce (in 2008) differs between small and medium firms. Small firms have more white collar employees, while medium firms have more blue collar workers. Finally, the share of low-paid workers and that of part-time workers are higher in small firms. These variables are included in the regressions to control for these differences.

4 Results

We now turn to our main results on the effect of the hiring credit on employment, hours worked, wages, hires, separations and churning. These results allow us to evaluate the cost per job created and the windfalls for firms. Since we find that the windfalls represent a very large share of the total cost of the hiring credit, about 84 percent, we explore the consequence of alternative hiring credit schemes conditional on net job creation. Various robustness checks are presented in the next section.

4.1 Employment and hours

Let us start providing some graphical illustrations of our results and of the relevance of our identification strategy.

The validity of difference-in-differences estimations heavily relies on the common trend assumption. We describe the common trend for treated firms with previous size between 6 and 10 (excluded) and control firms with previous size from 10 to 14. The outcome is average employment growth in each group. Employment is computed at the firm level. Employment in year \( t \) is equal to employment on 30 November of year \( t \). This ensures that employment in 2008 is not influenced by the hiring credit that was announced on 4 December 2008. Let \( L_{i,t} \) denote employment in firm \( i \) on 30 November of year \( t \), average employment growth for each group is \[ \frac{1}{N_{i,t}} \sum L_{i,t} - L_{i,t-1} \] where \( N_{i,t} \) is the number of firms in the group. Figure 5 shows that the

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We focus on the effect of the hiring credit on employment growth rate rather than on employment level for the following reason. The common trend assumption on the employment level requires identical differences in employment levels between year \( t \) and year \( t-1 \) for the control and the treatment group before 2009, i.e. \( L_{C,t} - L_{C,t-1} = L_{T,t} - L_{T,t-1} \) where \( L_{j,t} \) stands for average employment of group \( j \) (\( j = C \) for the control group and \( j = T \) for the treatment group) in year \( t < 2009 \). We checked that this assumption is not fulfilled. This is not surprising to the extent that the impact of productivity shocks or labor costs shocks on the employment level are expected to increase with the size of the firm. This is the case, for instance, when the wage elasticity of labor demand is constant. To see this, consider a simple static model, where the production function is \( F(L) \) and the labor cost is equal to the net wage \( w \) times the labor wedge \( \phi \). The optimal level of employment satisfies \( F'(L) = \omega \phi \). This equation implies that a one percent change in labor cost induces a change in employment level that is proportional to the initial employment level of the firm, i.e. \( dL = \varepsilon d\phi / \phi \), where \( \varepsilon = F'(L) / LF''(L) \) denotes the elasticity of labor demand with respect to the labor cost \( w \phi \).
### Figure 5: Average growth rate of employment in firms in the treated and control groups.

Growth rate of employment between 30 November of year $t-1$ and year $t$. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November).

The difference in employment growth rates between the treatment group and the control group is negative and constant from 2006 to 2008. In 2009, this difference becomes positive: the growth rate of the treatment group drops by 0.9 percentage points while that of the control group drops by 1.6 percentage points. Figure 6 shows that the same phenomenon arises for hours of work: the average growth rate of total hours of work per firm of the treatment group is below that of the control group from 2006 to 2008 and becomes larger than that of the control group in 2009. This points to positive treatment effects, that we estimate below.

In Table 2, we present our difference-in-differences estimates for different outcomes (in lines) and specifications (in columns). In column 1, our baseline sample comprises all cohorts from 2006 to 2009 without covariates. In column 2, we add covariates control. In column 3, we restrict the sample to cohorts 2008 and 2009 (to avoid potential specification errors about underlying trends). The results are very stable. They indicate that the hiring credit increased the employment growth rate of the treatment group by 0.8 percent (column 2, line 4 of Table 2). Table 2 shows that the impact of the hiring credit on the growth of hours of work is similar to that on employment, indicating that firms did not reduce working hours on existing jobs to compensate for new hires. The last row of Table 2 shows that the hiring credit had no impact on the survival of firms, meaning that the hiring credit raised employment in surviving firms. Indeed, estimates

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\[
\begin{array}{c|c|c|c|c|}
\text{Year} & \text{Small firms (treated)} & \text{Medium firms (control)} \\
\hline
2006 & -0.04 & -0.03 \\
2007 & -0.03 & -0.02 \\
2008 & -0.02 & -0.01 \\
2009 & -0.01 & \\
\end{array}
\]
Figure 6: Average growth rate of hours in firms in the treated and control groups. Growth rate of the number of hours worked within each firm between November of year $t$ and November of year $t - 1$. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November).
on the subsample of surviving firms are identical to that of all firms, as shown in table 15 in appendix.

Figure 7 displays the difference-in-differences estimates for the effect on employment month by month over the year 2009. The estimated impact of the hiring credit increases steadily over the year. The same is true for hours worked, as shown on Figure 8. In line with the literature on dynamic labor demand, our results indicate that employment may react quickly to shocks on labor costs, in a delay that is clearly infra annual (Hamermesh, 2013).

Our estimates are not weighted by firm size. This could bias our results if, for instance, the elasticity of labor demand depends on the size of firms. We checked that estimates provided along the paper yield similar results as weighted estimates. This is illustrated by table 16 in appendix which shows the weighted estimates corresponding to those displayed table 2.

The number of observations in table 3 is smaller than in table 2 because there are firms without job either below or above 1.6 times the minimum wage. The last column of table 3 displays the difference-in-differences estimates for all jobs with this smaller sample. Results are identical to those displayed in table 2 corresponding to the full sample comprising firms without job either below or above 1.6 times the minimum wage.
Figure 8: Difference-in-differences estimates of the impact of the hiring subsidy on the growth rate of hours of work. The outcome is $(h_{m,t} - h_{t-1})/h_{t-1}$ where $h_{m,t}$ denotes hours of work in month $m$ of year $t$ and $h_{t-1}$ hours of work in November of year $t - 1$. Estimations include years and covariates presented in table 2, column 2.

Since the hiring credit decreased the total labor cost of firms of the treatment group by 0.2 percent\footnote{In November 2009, firms of the treatment group got 3.6 million euros from \textit{zéro charges} while their labor cost during that month was 1.75 billion euros, which corresponds to a decrease of 0.21\% in labor cost. Over all the year 2009, \textit{zéro charges} decreased the labor cost of firms of the treatment group by 0.14\%. The amount of subsidies paid by \textit{zéro charges} increased progressively during 2009.} and increased total employment by 0.8 percent, our estimates point to an employment elasticity with respect to the change in labor cost induced by the hiring credit around $-4$.

The strong employment impact of \textit{zéro charges} relies on the absence of wage increase and on the absence of increase in churning, as shown below. Even if wages and labor turnover did not increase, this figure may seem incredibly high, at first sight, compared to usual estimates of labor demand elasticities. For instance, Kramarz and Philippon (2001) and Abowd et al. (2006) found that the elasticity of employment with respect to the minimum wage is about $-2$ for men and $-1.5$ for women in France.

It is important to remark, however, that the strong employment impact of \textit{zéro charges} also relies on the fact that a temporary decrease in average labor cost can have stronger employment effects when it is induced by a hiring credit than by wage changes that apply to all employees. To show this, let $\varepsilon$ stand for the elasticity of contemporaneous employment, $L$, when the change in average labor cost per worker is due to a temporary change in the wage cost $w$ of all incumbent and entrant workers. This is the standard definition of labor demand elasticity when the payroll equals $wL$. Let $\varepsilon_n$ stand for the elasticity of employment when the change in average labor cost per worker is due to \textit{zéro charges}. Remind that the hiring credit changes the cost of entrants only. The relation between employment and hires is given by the law of motion of employment (1). Let us assume that $\varepsilon$ is identical in all firms and that the hiring credit does not increase...
churning and wages, which is the case for *zero charges* as shown below. We get (see appendix A.2):

$$
\varepsilon_\sigma = \varepsilon_\pi / \eta
$$

(3)

where $\pi$ is the number of jobs of firms that benefit from the hiring credit during the period (i.e. year 2009 in our context) over total employment, and $\eta$ is the number of hires that benefit from the hiring credit over total employment. The difference between the two elasticities is explained by two terms. The first term is the share of jobs of firms that benefit from the hiring credit. This term implies that the employment elasticity induced by the hiring credit is *smaller*, in absolute value, than that induced by a proportional change in the wage of all workers. The reason is that the hiring credit has no impact on firms that do not recruit or do not take advantage of the hiring credit while a drop in wage costs would increase employment in those firms. The second term, which is the number of subsidized hires divided by total employment, implies that the employment elasticity induced by the hiring credit is *larger*, in absolute value, than that induced by a proportional change in the wage cost of all workers. The reason is that subsidizing the jobs of incumbent workers in firms that recruit has no employment effects. This only creates windfalls for firms. Using hiring credits is a means to target subsidies to marginal jobs, that have positive employment effects, without providing subsidies to incumbent workers, that have no employment effects.

All in all, we find that $\varepsilon$, the elasticity of employment with respect to labor cost induced by a change in wage, is smaller, in absolute value, than when the labor cost is modified by the hiring credit. We find that $\varepsilon = -1.1^{21}$ which is in line with previous estimates obtained for France.

### 4.2 Wages

The hiring credit may raise individual net wages. It may also induce firms to hire less skilled workers, with lower wages, since the hiring credit decreases with the wage as shown on Figure 1. To evaluate the impact of the hiring credit on wages, we use our difference-in-differences approach where the dependent variable is the difference in log wages. Let $w_{it}$ be the average hourly wage of workers in firm $i$ in year $t$ and $\tilde{w}_{it-1}$ their average hourly wage in previous year, if they worked, either in firm $i$, or in any other firm. Workers who did not work in the previous year are excluded. For each firm $i$ and year $t$, the dependent variable is $\ln w_{it} - \ln \tilde{w}_{it-1}$ for all workers present in firm $i$ on 30 November of year $t$. This variable allows us to compare the evolution of wages changes in small and medium firms controlling for the individual past wages. If the hiring credit has had an impact on wages, it should be apparent for the entrants eligible to the hiring credit – i.e. workers hired during the current year, paid below 1.6 times the minimum wage and who worked in the firm at least one month. Figure 9 shows the evolution of the wages of these workers in the small and medium firms over the years 2006-2009. Contrary to what happens for employment and hours of work, there is no break in the common trend in 2009. This suggests that the hiring credit has had no impact on wages. This is confirmed by table 4 which displays the difference-in-differences estimates for the wages of all workers, for the wages of incumbent workers paid below 1.6 times the minimum wages and for the wages of entrants eligible to the hiring credit. In all cases, the estimates point to a null effect of the hiring credit.

---

21 In the treatment group, the number of jobs of firms that benefit from the hiring credit is equal to 183,921 and the number of subsidized hires is equal to 48,192, so that $\mu / \pi$, equal to 48,192/183,921, amounts to 0.26, which implies that $\varepsilon = -4 \times 0.26 = -1.06$. 

17
Figure 9: Average log wage difference of entrants eligible to the hiring credit in firms in the treated and control groups. Eligible entrants are workers hired during the current year, paid below 1.6 times the minimum wage and who worked at least one month in the firm. The average log wage difference for each group is

\[
\frac{1}{N_i} \sum \ln w_{it} - \ln \bar{w}_{it-1}
\]

where \(w_{it}\) is the average hourly wage of eligible entrants in firm \(i\) in year \(t\) and \(\bar{w}_{it-1}\) their average hourly wage in previous year, if they worked, either in firm \(i\), or in any other firm; \(N_i\) is the number of firms in the group. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November).

on wages. This result is not surprising in the French context, where there is a high minimum wage and collective agreements that cover more than 90 percent of employees.

4.3 Churning and separations

Table 2 shows that the hiring credit has a positive, although non significant, impact on the separation rate. Consistent with this result, the hiring credit has a bigger impact on the hiring rate than on employment growth, although the difference is not significantly different from zero. It may be suspected that this result reflects some strategic behavior of firms which could replace incumbent workers by new workers to benefit from the hiring credit. Below, we provide evidence which suggests that this is not the case.

Using French data over the period 1987-1990, Abowd et al. (1999) estimate that each job created in a given year is associated with 3 hires and 2 separations. Davis et al. (2012) also find that hires rise more than one-for-one with job creation in the US. This relation indicates that a higher incidence of recently formed matches at more rapidly growing firms generates higher separation rates. There are two reasons for this. One is purely mechanical: at given quit rate, the
The separation rate, equal to the number of separations during the period divided by employment at the beginning of the period (or by the average of employment at the beginning and at the end of the period), increases when employment grows faster. Another reason is due to the fact that filling a job requires to find the good match with the good worker, which is not always the case with the first hire. Accordingly, if the hiring credit fosters job creation, it may also increase churning, even if firms to do not strategically raise their separations to hire new workers at lower cost.\footnote{Assume that each hire induces $s$ separations. If $s$ remains constant, the separation rate, defined as $S/L_{t-1}$ increases with $H$. This is also the case if the separation rate is defined as $2S/(L + L_{t-1})$, as in Davis et al. (1996).}

The upper chart of Figure 10 shows the relation between the hiring rate and the employment growth rate in small-size and medium-size firms over the period 2006-2008. The vertical axis displays the average annual hiring rates\footnote{The hiring rate of year $t$ is the number of hires from 1 December of year $t - 1$ to 30 November of year $t$ divided by employment on 30 November of year $t - 1.$} by growth rate bins. Hires increase more than one-for-one with job creation in all firms. Over the period 2006-2008, the relation between hires and employment growth is similar in small-size and in medium-size firms.

If the hiring credit had induced employers to replace incumbent workers with new workers to benefit from the subsidy, in 2009, the hiring rate, at a given employment growth rate, would have been higher in small firms, eligible to the hiring credit, than in medium-size firms, not eligible to the hiring credit. The bottom panel of Figure 10 shows that this is not the case. The relation between hires and employment growth is similar in small-size and medium-size firms before and after 2009. This means that the hiring credit did not induce firms to increase labor turnover in order to benefit from the subsidy.

4.4 Heterogeneous effects

The employment effect of the hiring credit may differ across sectors and regions depending on the local labor market conditions or sector idiosyncrasies. For one thing, the impact of the hiring credit on employment depends on the hiring cost which is likely to differ among firms depending on the nature of main activity and where they operate. Where it is particularly difficult to recruit, hiring costs should be higher and the hiring credit might not be enough to boost employment.

To check for the possibility of heterogeneous employment effects across regions and sectors, we use a survey on recruitment shortages (survey Besoins de Main d’Oeuvre - BMO). The survey comprises information on recruitment difficulties in 388 local employment pools and 24 sectors, providing each year 8,622 estimates (some industries are not always present in all employment pools). Every year, firms are requested to provide, for the coming year, the number of recruitments they plan, and how many of these recruitment projects are considered difficult (see appendix A.5 for a detailed presentation of this source). The difficulties are self-assessed and merely expected, not realized. This source allows us to calculate the percentage of difficult hiring plans at the sector-areas level. The resulting estimates of hiring difficulties are then matched with the DADS based on the employment pool and industry to which firms belong. This matched database is used to evaluate the influence of hiring difficulties on the effects of the hiring credit. To do so, we estimate the difference-in-differences model\footnote{23} separately for the four quartiles of the hiring difficulties distribution. To control for the potential endogeneity...
Figure 10: Hiring rate and employment growth rate in small size and medium size firms. The upper chart displays the average of the mean hiring rate by employment growth rate bins over 2006-2008. The bottom chart displays the average hiring rate by employment growth rate bins in 2009. Dots represent 6-bin moving averages. Small size firms have 6-10 (excluded) full time equivalent employees in previous year. Medium size firms have 10-14 full time equivalent employees in previous year. Source: DADS.
of hiring difficulties in 2009, the share of difficult hires in 2009 is instrumented by the average share of difficult hires in 2006-2008 (at a time when the hiring credit was not yet implemented).

Table 5 presents the estimates by quartile of hiring difficulties. It shows that the employment effect decreases when recruitment difficulties increase. The differences across quartiles are large. There are no significant employment effects in the quartile with the bigger hiring difficulties while the effect is very large and significant in the quartile with the smaller hiring difficulties. This suggests that the hiring credit is not effective to create jobs in sectors and areas where workers are hard to find. This might be the case, for instance, in booming areas and sectors, or when there is a permanent lack of suitable manpower. This might also be more frequently the case in booms than in recessions, suggesting that hiring credits may be more effective to boost employment when the economy slowdowns.

Differences in the employment effect of the hiring credit do not only stem from variations in hiring difficulties. Given the temporary nature of zero charges, heterogeneity can also be the result of differences in firms’ ability to optimize their production schedule and produce more while the hiring credit is available. Obviously this would be possible for manufacturing companies which can temporarily increase inventories, but not for service sector firms which produce services only when needed by customers, or even construction companies which face long delays in launching new projects. As shown in figure 15 the effect of the hiring credit is indeed close to the overall average in the service sector, but is twice as large in the manufacturing sector and about zero in the construction sector. But such differences in the response to the hiring credit could stem from other factors than sector specificities. For instance, it is possible that sectors faced different variations in hiring difficulties in 2009. In fact, controlling for the difficulties of recruitment at the local and sector level when measuring the employment effect of the hiring credit is not sufficient to get rid of sector-specific effects, suggesting that the type of mechanism mentioned previously could be at play. To see this we regress the following equation:

\[
Y_{it} = \alpha + \beta Z_{it} + \gamma D_{it} + \delta Z_{it}D_{it} + Q_{i1}\phi_1 + Z_{it}Q_{i2}\phi_2 + D_{it}Q_{i3}\phi_3 + Z_{it}D_{it}Q_{i4}\phi_4 + S_i\theta_1 + Z_{it}S_i\theta_2 + D_{it}S_i\theta_3 + Z_{it}D_{it}S_i\theta_4 + X_{it}b + u_{it}
\]

where \(Q_i\) is a vector of 3 dummies corresponding to quartile 2, 3 or 4 of the distribution of hiring difficulties, as measured at the employment local area and for each sector separately over the period 2006-2008; \(S_i\) is a vector of 2 dummies if firm \(i\) belongs either to the service (including retail and transport, hotels and restaurants, and other merchant services) or the construction sectors. In this model, \(\delta\) measures the effect of the hiring credit in firms belonging to the manufacturing sector and the first quartile of hiring difficulties, while the vector \(\theta_4\) represents the effect of the credit in the remaining sectors controlling for the impact of hiring difficulties which are captured by the vectors of coefficients \(\phi_1\) to \(\phi_4\). Table 6 presents the estimates of \(\theta_4\) when the dependent variable is employment or hours growth rates. It shows that the impact of

---

24 Note that these differences do not rely on differences in the share of eligible hires that benefited from the hiring credit (the “attention rate”) since the correlation between hiring difficulties and the attention rate is close to zero, equal to 0.0198.

25 Note that these large differences do not rely on differences in the share of eligible hires that benefited from the hiring credit (the “attention rate”) across industries. In the treatment group, the attention rate is equal to 40% in manufacturing, 44% in construction, 38% in retail and transport, 33% in hotels and restaurants and 34% in other merchant services.
the credit still differs across sectors even after controlling for hiring difficulties\textsuperscript{29} in construction the effect is reduced by 75\% compared with manufacturing; while the service sector the effect is about half.

5 Cost analysis

5.1 Cost per created job and windfalls for firms

Based on our estimates, it is possible to compute the gross cost per created job in the treatment group. The hiring credit \textit{zéro charges} provided 3.6 million euros to the firms of the treatment group in November 2009 and created about 5,200 jobs at that date\textsuperscript{27}. Accordingly, at the end of 2009, the monthly cost of job creation amounts to $3.6 \times 10^6 / 5,200$, or 700 euros (about 25\% of the average cost of a job in the treatment group)\textsuperscript{28}. This is gross cost, because it ignores the savings generated by job creation in terms of unemployment and other social benefits that would have been paid in the absence of the measure. It also ignores the remaining social contributions paid by employees on these additional jobs. We exploit a survey, presented in appendix A.6 which allows us to precisely evaluate the savings permitted by \textit{zéro charges} on social benefits. The savings amount to about 700 euros per month. This makes the net cost of the hiring credit per created job equal to zero.

There are however large windfalls for firms associated with the hiring credit. Let $\bar{H}$ denote the number of subsidized hires and $\Delta$ the number of hires created by the hiring credit. The share or hires that have benefited from the hiring credit and that would have been created absent the hiring credit amounts to $(\bar{H} - \Delta) / \bar{H}$. According to our estimates, 84\% of the cost of hiring credit funded hires that would have occurred absent the hiring credit\textsuperscript{29}.

5.2 Evaluation of the impact of hiring credits conditional on net job creation

Given the large windfalls for firms associated with the \textit{zéro charges} scheme, it is worth looking at alternative schemes that aim at reducing the cost of hiring credits. Many schemes rely on credits conditional on net job creation. In order to shed light on the differences in the effects of credits conditional on net job creation and hiring credits, we rely on the model of labor demand with hiring and firing costs of appendix A.1. This model allows us to compare the impact of the two schemes when the hiring credit $\sigma$ per eligible hire is identical for both schemes. It shows that (see appendix A.3):

\textsuperscript{26}The attention rate is very similar across sectors and not related to hiring difficulties: the correlation between the attention rate and the hiring difficulties is only 0.02 for the firms with 6 to 10 employees in our sample.

\textsuperscript{27}As shown by table 14 there are 646,717 jobs in the treatment group at the end of 2008. According to table 2 our estimate of coefficient $\delta$ when the dependent variable is $L = L_{-1}$ in equation (2), equals 0.008. Thus, the number of jobs created in the treatment group is $0.008 \times 646,717 = 5,173$.

\textsuperscript{28}This cost per job created can also be computed from the knowledge of the elasticity of labor demand. If the hiring credit creates $\Delta L$ jobs thanks to a drop in cost equal to $\Delta \psi$ per job, the cost per job created is equal to $\Delta \psi / \Delta L$. Using the definition of the elasticity of labor demand with respect to the labor cost, equal to $(\Delta L / L) / (\Delta \psi / \psi)$, we find that the cost per job created is equal to $\psi / \varepsilon_{\sigma}$ where $\varepsilon_{\sigma} = -4$ stands for the elasticity of employment when the change in labor cost is due to \textit{zéro charges}.

\textsuperscript{29}There are 48,992 subsidized hires in the treatment group in 2009. According to table 2, the hiring credit, which increased the hiring rate $H / L_{-1}$ by 1.2 percent, created 7,760 (equal to 0.012 $\times$ 646,717) additional hires, since there are 646,717 jobs in the treatment group at the end of 2008, as shown by table 14.
1. When the hiring credit is not conditional on net job creation:

(a) In firms where firing costs and hiring costs are sufficiently low, the hiring credit induces employers to layoff incumbent workers to replace them by subsidized workers.

(b) In firms where firing or hiring costs are sufficiently high, the hiring credit does not induce layoffs in order to hire subsidized workers.

2. When the credit is conditional on net employment growth, its impact on hires and employment is the same as that of the hiring credit non conditional on net job creation in firms which are in the case 1b above and which benefit from the conditional hiring credit (i.e. for which \(L - L_{-1} / L_{-1} > \gamma\), where \(\gamma\) denotes the employment growth threshold above which firms become eligible to the hiring credit).

Since our empirical evaluation concludes that zéro charges, which is a hiring credit non conditional on net job creation, did not induce firms to layoff workers in order to hire subsidized workers, we evaluate the potential impact of credits conditional on net job creation assuming that case 1b applies. This allows us to compute the labor demand elasticity with respect to the labor cost when the change in labor cost is induced by the credit conditional on net job creation above the employment growth threshold \(\gamma\). From the knowledge of this elasticity, denoted by \(\varepsilon_\gamma\), and from the knowledge of the labor demand elasticity with respect to the labor cost when the change in labor cost is induced by the non conditional hiring credit, denoted by \(\varepsilon_\sigma\), we can compute the number of jobs created by each type of credit for an expenditure equal to one percent of the labor cost absent the credit. We assume that the elasticity of labor demand with respect to the wage, \(\varepsilon\), is identical for all firms and that the take-up rate of eligible hires is identical for both types of credit. Then, the ratio of the number of jobs created by the credit conditional on net job creation above the employment growth threshold \(\gamma\) over the number of jobs created by the non conditional hiring credit is equal to:

\[
\frac{\varepsilon_\gamma}{\varepsilon_\sigma} = \frac{\eta}{\pi} \frac{1 + \Gamma(\gamma)}{\Gamma(\gamma) - \gamma},
\]

where \(\Gamma(\gamma)\) is the average employment growth rate of firms that grow above the threshold \(\gamma\) absent the subsidy, \(\pi\) is the number of jobs of firms that benefit from the non conditional hiring credit over total employment absent the hiring credit, and \(\eta\) is the number of hires that benefit from the non conditional hiring credit over total employment absent the hiring credit.

Figure 11 displays the \(\varepsilon_\gamma/\varepsilon_\sigma\) ratio computed on the treatment group. The ratio is bigger than one, meaning that the credit conditional on net job creation is more effective to create jobs than the non conditional hiring credit. The reason is that the credit conditional on net job creation reduces the windfalls for firms. Figure A.1 also shows that the credit conditional on net job creation creates a maximum number of jobs when the employment growth rate threshold equals \(-1\)%. In that case, the credit conditional on net job creation creates 1.8 times more jobs, at given budget, than the non conditional hiring credit.

These results suggest that it could have been worth targeting the credit to net job creation and providing more generous subsidies per job created to less firms. However, this conclusion

\[30\] see equation (A28) in appendix A.4.

\[31\] see equation (A11) in appendix A.2.

\[32\] see the definitions of the two elasticities equations (A11) and (A28) in appendix A.2 and A.4 respectively.
Figure 11: Job ratio of the number of jobs created by the credit conditional on net job creation above the employment growth threshold $\gamma$ over the number of jobs created by the non conditional hiring credit. The horizontal axis displays the employment growth rate threshold $\gamma$ above which firms are eligible to the credit conditional on net job creation.

needs further investigations because credits conditional on net job creation are much more complex to implement than non conditional hiring credits. This implies that the take-up rate might be significantly lower with credits conditional on net job creation. Moreover, since firms do not know with certainty when they hire workers if they will reach the threshold above which they become eligible when credits are conditional on net job creation, their impact might be smaller than that of non conditional hiring credits.

6 Robustness checks

In this section we perform an number of additional estimations to check the robustness of our baseline results. We vary the bandwidth. We run placebo tests to confirm the validity of the common trend assumption. We also control for any potential equilibrium effect that could bias our previous estimates.

6.1 Changing the bandwidth

Our benchmark estimates are based on a sample which includes some treated firms featuring a lower take-up than others (between 8 and 10 employees), and a residual take-up among control firms (between 10 and 12 employees). Table 7 presents the estimates for different bandwidths. The difference-in-differences estimates are higher when the treatment group includes firms with
higher take-up rates. This is the case in column 3, as shown by Figure 4. Column 4 shows that the estimates are also higher when the control group excludes firms with residual take-up. This suggests that our benchmark estimates, which rely on firms from 6 to 14 full time equivalent employees in previous year, are lower bounds of the impact of the subsidy. The corresponding estimates of wage elasticity of labor demand equal 2.7 and 3.7 when the bandwidth goes from 5 to 15 employees and belongs to [5,8]-[13,16] employees respectively. All in all, these results suggest that our benchmark estimate of the elasticity of employment with respect to the hiring credit is conservative: it is likely a lower bound for the elasticity that might be larger than 2 in absolute value.

6.2 Year placebo tests

We perform a series of placebo tests using the data from 2005 to 2008. We use the specification of column 3 in Table 2 as if the policy had been implemented in December 2006 (using cohorts 2006 and 2007) or December 2007 (using cohorts 2006, 2007 and 2008). Table 8 shows that employment, hours, hires and separations of the treatment and the control group did not evolve differently neither in 2007 nor in 2008, contrary to 2009 when zéro charges was introduced. Figures 12 and 13, which display the month-by-month difference-in-differences estimates for employment in year 2007 (if the shock had been in December 2006) and in year 2008 (if the shock had been in December 2007) respectively, show that the month-by-month evolution of employment was similar in the control and the treatment group over these 2 years. These results comfort the relevance of the common trend assumption.
Figure 13: Placebo test as if the policy had been implemented in December 2007. Difference-in-differences estimates of the impact of the hiring subsidy if the subsidy had been introduced in December 2007. The outcome is the growth rate of employment \((L_{m,t} - L_{t-1})/L_{t-1}\) where \(L_{m,t}\) denotes employment at the end of month \(m\) of year \(t\) and \(L_{t-1}\) employment on 30 November of year \(t-1\). Estimations include years 2006-2008 and covariates presented in table 2.

6.3 Size threshold placebo tests

A potential concern is that our results could reflect the fact that firms of different sizes behave differently during the business cycle, especially at the beginning of recessions. Moscarini and Postel-Vinay (2012) have shown that large firms (above 500 employees) destroy proportionally more jobs in net terms relative to small firms (below 20 employees) when unemployment is above trend in France. This phenomenon is not necessarily a concern in our case, because the difference in the firm size in our control and treatment group is very small compared to that of Moscarini and Postel-Vinay. Nevertheless, we check that there are no systematic difference in the evolution of employment and hours across firms of different size in 2009.

If firms of size between 6 and 10 employees in 2008 behaved differently in 2009 from firms of size between 10 to 14 employees because of differences in sizes and not because of the hiring credit, we would expect firms with 13 to 16 employees to behave differently from firms with 16 to 19 employees. Figure 14 compares the average employment growth rate for firms with 13 to 16 (excluded) employees in previous year and firms with 16 to 19 employees in previous year. The difference in employment growth across these groups does not change in 2009. This result is confirmed by the difference-in-differences estimates for these two groups of firms. This suggests that the difference in employment growth across our treatment and control group does not stem from differences in behavior due to differences in size.

\[\text{We avoid to make comparisons using firms with 10 to 12 employees in previous year which, for a tiny fraction, have benefited from the subsidy as shown by figure 4.}\]

\[\text{We do not present these estimates to save space. The results are available on request.}\]
6.4 Equilibrium effects

The validity of difference-in-differences estimations relies on the assumption that the control group is not affected by the policy. In our context, it is possible that firms above 10 employees have been impacted by the hiring credit. The hiring credit may provide competitive advantage to small firms that expand their market share at the expense of larger firms. The supplementary hires induced by the hiring credit may increase the labor market tightness and then the recruiting costs for all firms. Potential wage increases induced by the hiring credit may affect the control group. All these mechanisms imply a potential negative impact of the hiring credit on employment and hours worked of the control group. These effects potentially bias upward the estimates of the true effects of the hiring credit on small firms.

To deal with this issue we check whether employment and hours worked of the control group have been impacted by the share of subsidized hires in their employment pool. If there are equilibrium effects, in areas with a high share of subsidized hires we should observe lower growth rates of employment or hours among non eligible firms than in areas with a low share of subsidized hires. We distinguish 348 employment pools. Within each employment pool, we compute the share of subsidized hires in 2009, and the average growth of employment and of hours worked of firms from 10 to 14 full time equivalent employees in previous year. The average share of subsidized hires in 2009 is 5.5%, and the standard deviation across commuting zones is

35We use the 348 zones d’emploi provided by INSEE, the French national statistical office. A zone d’emploi is a geographic area wherein most workers reside and work, and in which companies can find most of the labor needed for the jobs offered. The definition of zone d’emploi is based on the flow of commuting workers observed in the 2006 Census.
3.5%. We then compare the labor market outcomes in employment pools with different shares of subsidized hires. To do so, we estimate the following simple model:

\[
Y_j = \alpha + \beta Share_j + bX_j + u_j
\]

where \(Y_j\) is the average outcome (growth rate of employment or hours) of employment pool \(j\) between 1 December 2008 and 30 November 2009 among firms from 10 to 14 full time equivalent employees in previous year, and \(Share_j\) is the share of subsidized hires in employment pool \(j\) over the same period dummy and \(u_j\) is a residual. We also include a number of area-specific controls \(X_j\), such as the distribution of firms across sectors, the distribution of firm age, as well as the growth rate of employment, the hiring and separation rates in the employment pool in 2008. This aims at better controlling for the labor market dynamics specific to each pool. If the coefficient \(\beta\) is negative and significant, this indicates the presence of equilibrium effects.

Now, the share of subsidized hires in 2009 is potentially endogenous in equation (6). This share is not only influenced by potentially exogenous factors, such as the number of small firms or the prevalence of low-wage employment in one given area before the implementation of the subsidy, but also by endogenous factors such as the share of eligible hires actually benefiting from the subsidy in 2009. For instance, in areas featuring higher employment growth among medium-size firms it is likely that small firms experience a similar growth pattern. If the take-up is influenced (in any way) by economic conditions, as the share of hires actually subsidized in 2009 will change as well. This mechanism would yield a spurious correlation between \(Y\) and \(Share\) in equation (6) and bias estimates of equilibrium effects based on OLS. For this reason, the share of subsidized hires in 2009 is instrumented by the share of eligible hires among all hires in an employment zone in 2008 (when the subsidy was not yet implemented), and by the share of small firms below 10 full-time-equivalent employees in 2008 among all firms present the same year in an employment zone.

Table 9 presents the estimates based on the OLS. It shows that there is no statistically significant correlation between the share of subsidized hires and the average growth rates of employment and hours in 2009. Tables 10 shows the estimates when the share of subsidized hires in 2009 is instrumented by the share of eligible hires and the share of small firms in 2008, both for the growth rate of employment and that of hours. The lower panel of this Table presents the first step of the estimations. It shows that the share of subsidized hires in 2009 is strongly correlated with the instruments. The first panel shows the second step of the estimation. The coefficients are those of the instrumented variable. No equilibrium effects are detected, even when the full set of controls is included in the regression. When we omit weights, the coefficients are smaller than in the OLS and negative, but still insignificant. When we include weights, the coefficients are similar to those obtained with the OLS. The two instruments are not rejected by the over identification test at the 5 percent level. However, the endogeneity test suggests that the share of subsidized hires in 2009 is not endogenous both when the dependent variable is the growth rate of employment in 2009 or when it is the growth rate of hours. In that case the estimates based on the OLS are more relevant.

7 Conclusion

This paper shows that a hiring credit targeted at small firms and low wage workers had a significant impact on employment in France during the 2008-2009 recession. The hiring credit,
although non conditional on net job creation, did not induce firms to increase layoffs in order to hire workers at lower cost. These results are consistent with a standard neoclassical labor demand model with hiring and firing costs and exogenous wage.

All in all, it turns out that the hiring credit was very effective. It allowed the government to create jobs at zero net cost in a small amount of time. Our results suggest that the effectiveness of the hiring credit relied on the excess of labor supply during the recession, which implied that hiring difficulties were not stringent for a large share of firms. The low cost of job creation is also linked to the temporary nature of the hiring credit which allows the government to lower the cost of entrants but not that of incumbent workers. To the extent that employment adjusted quickly to the drop in labor cost, it is likely that the hiring credit has had a temporary impact on employment, that disappeared quickly when the hiring credit has been terminated. However, this issue is beyond the scope of this paper because the information is not yet available to study the evolution of employment after the end of zéro charges.

It should be kept in mind that our results have been obtained in a specific context. In particular, the zéro charges program was targeted to low wages, which are very rigid in France, because the minimum wage is high and almost all workers are covered by sectorial collective agreements that are binding in small firms. It is likely that the strong wage rigidity contributed to the positive employment impact of zéro charges, meaning that permanent hiring credits, or hiring credit non targeted to low wages, could have a much weaker employment impact.
References


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<th>Ineligible 6-10</th>
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<th>Treated 6-10</th>
<th>Untreated 6-10</th>
<th>Diff test p-value</th>
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<td>.100</td>
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<td>.254</td>
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<td>.351</td>
<td>.344</td>
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<td>.036</td>
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<td>.000</td>
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<td>.593</td>
<td>.000</td>
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<td>.000</td>
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<td>17,017</td>
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<td>-</td>
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</table>

Source: DADS (Insee). Note: Low-wage workers earn between the minimum wage and 1.6 times this amount (on hourly basis). Part-time workers work below 80 percent of normal working hours. The number of employees corresponds to the full-time equivalent over 2008. The number of observations corresponds to the number of firms.
Table 2: Difference-in-differences estimates

<table>
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<th></th>
<th></th>
</tr>
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<tbody>
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<td></td>
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<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Employment growth</td>
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<td>.008*** (.002)</td>
<td>.009*** (.002)</td>
</tr>
<tr>
<td>Hours growth</td>
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<td>.009*** (.002)</td>
<td>.008*** (.002)</td>
</tr>
<tr>
<td>Hiring rate</td>
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<td>.012*** (.004)</td>
<td>.019*** (.005)</td>
</tr>
<tr>
<td>Separation rate</td>
<td>.005 (.005)</td>
<td>.004 (.004)</td>
<td>.010* (.005)</td>
</tr>
<tr>
<td>Survival rate</td>
<td>.000 (.001)</td>
<td>.000 (.001)</td>
<td>.000 (.001)</td>
</tr>
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<td>Nb. Observations</td>
<td>405,376</td>
<td>405,376</td>
<td>206,854</td>
</tr>
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</table>

Source: DADS (Insee). Note: this table presents our difference-in-differences estimates for different outcomes (lines) and different specifications (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t; the growth rate of the number of hours worked between November of year t-1 and November of year t; the number of hires from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the survival rate from 30 November year t-1 to 30 November year t. As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.
Table 3: Difference-in-differences estimates for eligible and non eligible jobs

<table>
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<th>Eligible jobs</th>
<th>Non eligible jobs</th>
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<tr>
<td>Employment growth</td>
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<td>.008***</td>
</tr>
<tr>
<td></td>
<td>(.003)</td>
<td>(.004)</td>
<td>(.002)</td>
</tr>
<tr>
<td>Hours growth</td>
<td>.012***</td>
<td>.005</td>
<td>.008***</td>
</tr>
<tr>
<td></td>
<td>(.003)</td>
<td>(.004)</td>
<td>(.002)</td>
</tr>
<tr>
<td>Hiring rate</td>
<td>.011***</td>
<td>.005</td>
<td>.008**</td>
</tr>
<tr>
<td></td>
<td>(.004)</td>
<td>(.008)</td>
<td>(.004)</td>
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<tr>
<td>Separation rate</td>
<td>.001</td>
<td>.003</td>
<td>.000</td>
</tr>
<tr>
<td></td>
<td>(.004)</td>
<td>(.008)</td>
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</tr>
<tr>
<td>Nb. Observations</td>
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<td>349,996</td>
<td>349,996</td>
</tr>
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</table>

Source: DADS (Insee). Note: this table presents our difference-in-differences estimates for different outcomes (lines) and different types of jobs (columns): eligible jobs below 1.6 times the minimum wage that last at least one month; non eligible jobs above 1.6 times the minimum wage or that last less than one month; all jobs. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t; the growth rate of the number of hours worked between November of year t-1 and November of year t; the number of hires from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.
Table 4: Difference-in-differences estimates for wages

<table>
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<tbody>
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<td>Yes</td>
<td>Yes</td>
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<td>All wages</td>
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<td>.000</td>
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<tr>
<td></td>
<td>(.001)</td>
<td>(.001)</td>
<td>(.002)</td>
</tr>
<tr>
<td>Low wage incumbents</td>
<td>.000</td>
<td>-.001</td>
<td>.000</td>
</tr>
<tr>
<td></td>
<td>(.001)</td>
<td>(.001)</td>
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<tr>
<td>Eligible entrants</td>
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<td>-.001</td>
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<tr>
<td></td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.002)</td>
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<td>210,553</td>
<td>105,277</td>
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Source: DADS (Insee). Note: this table presents our difference-in-differences estimates for different outcomes (lines) and different specifications (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). We consider as outcomes the differences in log hourly wages between 30 November of year t-1 and year t; “All wages” stands for the wages of all workers present in the firm on 30 November of year t. “Low wage incumbents” stands for the wages below 1.6 times the minimum wage of workers present in the firm on 30 November of year t-1 to 30 November of year t. “Eligible entrants” stands for the wages below 1.6 times the minimum wage of workers present in the firm on 30 November of year t but not present in the firm on 30 November of year t-1 and who have been working at least one month in the firm. As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 5: Difference-in-differences estimates for different degrees of hiring difficulties

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<td>No</td>
<td>Yes</td>
<td>No</td>
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<td>.014***</td>
<td>.014***</td>
<td>.009**</td>
</tr>
<tr>
<td></td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.004)</td>
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<tr>
<td>Hours growth</td>
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<td>.013***</td>
<td>.013***</td>
<td>.008**</td>
</tr>
<tr>
<td></td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.004)</td>
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<td>92,073</td>
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</table>

Source: DADS (Insee) and BMO (Pôle Emploi). Note: this table presents our difference-in-differences estimates for different outcomes (lines) and different quartiles of hiring difficulties (columns): The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t and the growth rate of the number of hours worked between November of year t-1 and November of year t; As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.
Table 6: Difference-in-differences estimates in different industries controlling for hiring difficulties

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<th>Hours growth in manufacturing (first quartile of hiring difficulties)</th>
<th>Hours growth in construction</th>
<th>Hours growth in services</th>
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<td>δ</td>
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<tr>
<td></td>
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<td>-.011**</td>
<td>.020***</td>
<td>-.015***</td>
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<td>(.006)</td>
<td>(.005)</td>
<td>(ref)</td>
<td>(.006)</td>
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<tr>
<td></td>
<td>in construction</td>
<td>in services</td>
<td>in construction</td>
<td>in services</td>
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<tr>
<td></td>
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<td>.011**</td>
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<td>(.002)</td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.002)</td>
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Source: DADS (Insee) and BMO (Pôle Emploi). Note: this table presents our difference-in-differences estimates for different outcomes and different industries. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t and the growth rate of the number of hours worked between November of year t-1 and November of year t; As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies the quartile of hiring difficulties, for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 7: Difference-in-differences estimates varying the bandwidth.

<table>
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<tr>
<th>Size bandwidth</th>
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<th>Hiring rate</th>
<th>Separation rate</th>
<th>Survival rate</th>
<th>Nb. Observations</th>
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<td>.006***</td>
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<td>.007</td>
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<td></td>
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<td>(.002)</td>
<td>(.005)</td>
<td>(.004)</td>
<td>(.001)</td>
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<tr>
<td>6-14</td>
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<td>.009***</td>
<td>.012***</td>
<td>.004</td>
<td>.000</td>
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<td>(.002)</td>
<td>(.002)</td>
<td>(.007)</td>
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<td></td>
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<td>5-15</td>
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<td>.012***</td>
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<td>(.002)</td>
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<td>.015***</td>
<td>.015***</td>
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<td>.000</td>
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<td>(.002)</td>
<td>(.005)</td>
<td>(.004)</td>
<td>(.001)</td>
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</table>

Source: DADS (Insee). Note: this table displays the DID estimates varying the bandwidth (in columns). The sample contains all available cohorts (2006-2009), and we include covariates presented in table 2. The 2nd column is similar to column (2) of table 2. We consider as outcomes the growth rate of employment between 30 November of year t and year t-1; the growth rate of the number of hours worked between November of year t and November of year t-1; Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.
Table 8: Difference-in-differences estimates for all firms, with placebo years

<table>
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<tr>
<th>Placebo Cohorts</th>
<th>December 2006</th>
<th>December 2007</th>
</tr>
</thead>
<tbody>
<tr>
<td>Covariates</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Employment growth</td>
<td>-.001 (0.002)</td>
<td>.001 (0.002)</td>
</tr>
<tr>
<td>Hours growth</td>
<td>-.001 (0.003)</td>
<td>.001 (0.002)</td>
</tr>
<tr>
<td>Hiring rate</td>
<td>.001 (0.003)</td>
<td>-.004 (0.003)</td>
</tr>
<tr>
<td>Separation rate</td>
<td>.002 (0.003)</td>
<td>-.005* (0.003)</td>
</tr>
<tr>
<td>Survival rate</td>
<td>.000 (0.001)</td>
<td>.001 (0.001)</td>
</tr>
<tr>
<td>Nb. Observations</td>
<td>178,603</td>
<td>270,593</td>
</tr>
</tbody>
</table>

Source: DADS (Insee). Note: this table presents our difference-in-differences estimates for different outcomes (lines) and different placebo years (columns, 12 months starting from December 2006 or 2007, instead of 2009). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t and year t-1; the growth rate of the number of hours worked between November of year t and November of year t-1; the number of hires from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t; and the number of excess reallocation from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1. As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 millions euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 9: OLS Estimates of equilibrium effects of the growth rate of employment and hours in 2009 among firm with 10-14 employees.

<table>
<thead>
<tr>
<th>OLS</th>
<th>Area Weights</th>
<th>No</th>
<th>Yes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Covariates</td>
<td></td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Employment growth</td>
<td></td>
<td>.056 (.067)</td>
<td>.107* (.063)</td>
</tr>
<tr>
<td>Hours growth</td>
<td></td>
<td>.042 (.065)</td>
<td>.099 (.061)</td>
</tr>
<tr>
<td>Nb. Observations</td>
<td></td>
<td>348</td>
<td>348</td>
</tr>
</tbody>
</table>

Source: DADS (Insee). Note: The dependent variable is either the average growth rate of employment or that of hours over 12 months from 1 December 2008 to 30 November 2009 in each employment pool. The independent variables is the share of subsidized hires in 2009, which is the ratio of all subsidized hires and the sum of all hires in the employment pool in 2009. Robust standard errors in parentheses.
Table 10: Instrumental Variables Estimates of Equilibrium effects on the growth rates of employment and hours among firms with 10-14 employees.

<table>
<thead>
<tr>
<th></th>
<th>IV 2SLS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No</td>
</tr>
<tr>
<td><strong>Area Weights</strong></td>
<td></td>
</tr>
<tr>
<td>Covariates</td>
<td></td>
</tr>
<tr>
<td>Employment growth</td>
<td>−.013</td>
</tr>
<tr>
<td></td>
<td>(−.100)</td>
</tr>
<tr>
<td>Endogeneity test (p-value)</td>
<td>.4228</td>
</tr>
<tr>
<td></td>
<td>(−.099)</td>
</tr>
<tr>
<td>Hours growth</td>
<td>−.017</td>
</tr>
<tr>
<td></td>
<td>(.099)</td>
</tr>
<tr>
<td>Endogeneity test (p-value)</td>
<td>.132</td>
</tr>
<tr>
<td></td>
<td>(.080)</td>
</tr>
<tr>
<td>Overidentifying restriction test (p-value)</td>
<td>.265</td>
</tr>
<tr>
<td></td>
<td>(.070)</td>
</tr>
<tr>
<td><strong>First stage coefficients</strong></td>
<td></td>
</tr>
<tr>
<td>Share of eligible hires in 2008</td>
<td>.269***</td>
</tr>
<tr>
<td></td>
<td>(.036)</td>
</tr>
<tr>
<td>Share of small firms in 2008</td>
<td>.534***</td>
</tr>
<tr>
<td></td>
<td>(.046)</td>
</tr>
<tr>
<td>p-value</td>
<td>.0000</td>
</tr>
<tr>
<td>Nb. Observations</td>
<td>348</td>
</tr>
</tbody>
</table>

Source: DADS (Insee). Note: The dependent variable is either the average growth rate of employment or that of hours over 12 months from 1 December 2008 to 30 November 2009 in each employment pool. The independent variable of interest is the share of subsidized hires in 2009, which is the ratio of all subsidized hires and the sum of all hires in the employment pool in 2009. This share is instrumented by the share of eligible hires in 2008 and the share of small firms in 2008. The share of eligible hires in 2008 is the ratio hires with a wage less than 1.6 times the minimum wage in 2008 in firms with less than 10 employees at the end of 2007 over the sum of all hires in the employment pool in 2009. The share of small firms in 2008 is the ratio the number of firms with less than 10 employees at the end of 2007 present in 2008 over the total number of firms present in the employment pool in 2008. Robust standard errors in parentheses.
Table 11: Supplementary table: sample description

<table>
<thead>
<tr>
<th>Year</th>
<th>2005</th>
<th>2006</th>
<th>2007</th>
<th>2008</th>
<th>2009</th>
<th>2010</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nb (in thousands) of firms with size (Full time equivalent)...</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>less than 6 employees (1)</td>
<td>857</td>
<td>881</td>
<td>892</td>
<td>907</td>
<td>938</td>
<td>931</td>
</tr>
<tr>
<td>between 6 and 10 employees (2)</td>
<td>120</td>
<td>122</td>
<td>124</td>
<td>124</td>
<td>126</td>
<td>126</td>
</tr>
<tr>
<td>less than 10 employees (3)=(1)+(2)</td>
<td>977</td>
<td>1003</td>
<td>1,005</td>
<td>1,031</td>
<td>1,064</td>
<td>1,057</td>
</tr>
<tr>
<td>between 10 and 14 employees</td>
<td>53</td>
<td>54</td>
<td>56</td>
<td>56</td>
<td>57</td>
<td>56</td>
</tr>
<tr>
<td>Total</td>
<td>1,180</td>
<td>1,209</td>
<td>1,225</td>
<td>1,243</td>
<td>1,276</td>
<td>1,268</td>
</tr>
<tr>
<td>Nb of employees (in thousands) in firms with size (Full time equivalent)...</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>less than 6 employees (1)</td>
<td>1,720</td>
<td>1,777</td>
<td>1,790</td>
<td>1,812</td>
<td>1,856</td>
<td>1,847</td>
</tr>
<tr>
<td>between 6 and 10 employees (2)</td>
<td>925</td>
<td>944</td>
<td>953</td>
<td>954</td>
<td>965</td>
<td>964</td>
</tr>
<tr>
<td>less than 10 employees (3)=(1)+(2)</td>
<td>2,645</td>
<td>2,721</td>
<td>2,743</td>
<td>2,766</td>
<td>2,821</td>
<td>2,812</td>
</tr>
<tr>
<td>between 10 and 14 employees</td>
<td>629</td>
<td>644</td>
<td>658</td>
<td>659</td>
<td>666</td>
<td>663</td>
</tr>
<tr>
<td>Total</td>
<td>11,710</td>
<td>11,846</td>
<td>11,992</td>
<td>12,126</td>
<td>12,129</td>
<td>12,121</td>
</tr>
<tr>
<td>Growth rates à la Davis et. al. ??</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>less than 6 employees (1)</td>
<td>1,720</td>
<td>1,777</td>
<td>1,790</td>
<td>1,812</td>
<td>1,856</td>
<td>1,847</td>
</tr>
<tr>
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<td>944</td>
<td>953</td>
<td>954</td>
<td>965</td>
<td>964</td>
</tr>
<tr>
<td>less than 10 employees (3)=(1)+(2)</td>
<td>2,645</td>
<td>2,721</td>
<td>2,743</td>
<td>2,766</td>
<td>2,821</td>
<td>2,812</td>
</tr>
<tr>
<td>between 10 and 14 employees</td>
<td>629</td>
<td>644</td>
<td>658</td>
<td>659</td>
<td>666</td>
<td>663</td>
</tr>
<tr>
<td>Total</td>
<td>11,710</td>
<td>11,846</td>
<td>11,992</td>
<td>12,126</td>
<td>12,129</td>
<td>12,121</td>
</tr>
</tbody>
</table>

Source: DADS (Insee). Note: cohorts 2008-2009, permanent firms, no covariates. We sort the 88 different sub industries according to their exposure to the 2008 crisis and divide them in 2 sub groups. Turnover is computed as the mean turnover from 2005-2008.
A Appendix

A.1 The model with hiring and firing costs

We consider a discrete time partial equilibrium model of a firm that produces a single good with labor. The revenue function, denoted $R(A, L)$, is increasing with respect to the productivity parameter $A$ and increasing and concave with respect to labor $L$. The productivity parameter increases the marginal productivity of labor: $R_{AL}(A, L) > 0$.

There are hiring and firing costs. The hiring cost is an increasing and convex function of the number of hires. This function is denoted by $c_H(H)$, $c_H(0) = c'_H(0) = 0, c'_H(H) \geq 0, c''_H(H) > 0$, where $H \geq 0$ stands for the number of hires. Similarly, firing costs are equal to $c_F(F)$, $c_F(0) = c'_F(0) = 0, c'_F(F) \geq 0, c''_F(F) > 0$ where $F$ denotes the number of firings. An exogenous proportion $q$ of workers quit the firm during each period. The number of separations is equal to the sum of quits and layoffs.

Let us denote by $V(A, w, L_{-1})$ the value function of the firm, where $w$ is the wage in the current period and $L_{-1}$ is employment in the previous period. Let $\beta$ denote the discount factor and $E$ the expectation operator. The value function of the firm satisfies

$$V(A, w, L_{-1}) = \max_{(H,F)} R(A, L) - wL - c_H(H) - c_F(F) + \beta E \left[ V(A', w', L) \right]$$

subject to the law of motion of employment:

$$L = (1-q)L_{-1} + H - F$$

(A1)

and subject to $H \geq 0, F \geq 0$ and $F \leq (1-q)L_{-1}$.

Let us assume that there is a temporary hiring credit, that reduces the labor cost of entrants during the current period by an amount denoted $\sigma$. We analyze the impact of the hiring credit in the neighborhood of $\sigma = 0$. In the period where the hiring credit is implemented, the expression of the profit is

$$R(A, L) - wL - \sigma H - c_H(H) - c_F(F)$$

Profit maximization yields the first order conditions

$$R_L(A, L) - w + \sigma - c'_H(H) + \beta E V_L(A', w', L) + \lambda_H = 0$$  \hspace{1cm} (A2)

$$-R_L(A, L) + w - c'_F(F) - \beta E V_L(A', w', L) + \lambda_F - \bar{\lambda}_F = 0$$  \hspace{1cm} (A3)

where $\lambda_H, \lambda_F$ and $\bar{\lambda}_F$ are the multipliers associated with constraints $H \geq 0$, $F \geq 0$ and $F \leq (1-q)L_{-1}$ respectively. We now solve for the different cases.

- **Case 1:** $H > 0, F > 0, F < (1-q)L_{-1}$. This case corresponds to the interior solution. We get from the first order conditions, with $\lambda_H = \lambda_F = \bar{\lambda}_F = 0$:

$$c'_F(F) + c'_H(H) = \sigma$$  \hspace{1cm} (A4)

Then the current values of $L, H$ and $F$ are defined by equation (A4) above and the following equations:

$$R_L(A, L) = w - \sigma + c'_H(H) - \beta E V_L(A', w', L)$$  \hspace{1cm} (A5)

$$L = (1-q)L_{-1} + H - F$$  \hspace{1cm} (A6)
Case 2: $H > 0$, $F = 0$. In this case, there are no firings. Then, the first order conditions, together with $\lambda_H = 0$, imply

$$R_L(A, L) = w - \sigma + c_H'(H) - \beta EV_L(A', w', L)$$

$$L = (1 - q) L_{-1} + H.$$  \hfill (A7)

Case 3: $H > 0$, $F = (1 - q)L_{-1}$. In this case, the firm replaces all its incumbent workers by new workers. The law of motion of employment implies that $L = H$. Then, using the first order conditions and the fact that $\lambda_H = 0$, we find that hires (and employment) is defined by

$$R_L(A, H) = w - \sigma + c_H'(H) - \beta EV_L(A', w', H)$$

$$L = (1 - q)L_{-1} - F.$$  \hfill (A8)

The amount of subsidy paid to the firm is maximum and equal to $\sigma L$.

Case 4: $H = 0$, $F > 0$, $F < (1 - q)L_{-1}$. In this case, there are no hires, but layoffs. The first order conditions and the fact that $\lambda_F = 0$ and $\lambda_F = 0$, imply that:

$$R_L(A, L) = w - c_F'(F) - \beta EV_L(A', w', L)$$

$$L = (1 - q)L_{-1} - F.$$  \hfill (A9)

Case 5: $H = 0$, $F = (1 - q)L_{-1}$. In this case, the firm disappears as there is no more current employment.

A.2 Labor demand elasticity

In this appendix, we use the model presented in appendix A.1 to compute the elasticity of labor demand in the current period with respect to its contemporaneous labor cost. In order to account for the heterogeneity of firms, let us suppose that the productivity parameter $A$ is distributed across firms according to the cdf $G$ defined on the support $[0, \infty)$.

We compute the elasticity of labor demand in the current period with respect to its contemporaneous labor cost in two situations. First, when the wage $w$ changes. Second, when the change in labor cost is due to a non conditional hiring credit (as zero charges).

A.2.1 Changes in wage

Let us study the impact of a change in wage on labor demand when $\sigma = 0$. When $\sigma = 0$, only cases 2, 4 and 5 can exist in the solutions of the model of appendix A.1. For the sake of simplicity, we focus on surviving firms, so that we consider cases 2 and 4 only. When cases 2 and 4 exist, there exists a threshold value $\bar{A} > 0$ such that case 4 arises if $A \leq \bar{A}$ and case 2 arises if $A > \bar{A}$ because the marginal productivity of labor, $R_L(A, L)$, increases with $A$. In case 2, where $H > 0$ and $F = 0$, equations (A7) and (A8) define $L$ and $H$ as functions of $A$ and $w$ that are denoted by $l_{H}(A, w)$ and $h(A, w)$ respectively. In case 4, where $F > 0$ and $H = 0$, employment is defined as a function of $A$ and $w$ that is denoted $l_{F}(A, w)$. Remark that $l_{F}(\bar{A}, w) = l_{H}(\bar{A}, w)$.

Total employment is defined by:

$$\mathcal{L} = \int_{0}^{\bar{A}} l_{F}(A, w) dG(A) + \int_{\bar{A}}^{\infty} l_{H}(A, w) dG(A)$$
The derivative of total employment with respect to \( w \) is

\[
\frac{dL}{dw} = \int_0^\Lambda \frac{\partial l_F(A, w)}{\partial w} dG(A) + \int_\Lambda^\infty \frac{\partial l_H(A, w)}{\partial w} dG(A)
\]

or

\[
\frac{wdL}{dw} = \int_0^\Lambda l_F(A, w) \varepsilon_F(A, w) dG(A) + \int_\Lambda^\infty l_H(A, w) \varepsilon_H(A, w) dG(A)
\]

where \( \varepsilon_F(A, w) \) and \( \varepsilon_H(A, w) \) denote the wage elasticity of functions \( l_F(A, w) \) and \( l_H(A, w) \) respectively. Let us denote by

\[
\bar{\varepsilon}_F = \frac{1}{\int_0^\Lambda l_F(A, w) dG(A)} \int_0^\Lambda l_F(A, w) \varepsilon_F(A, w) dG(A)
\]

\[
\bar{\varepsilon}_H = \frac{1}{\int_\Lambda^\infty l_H(A, w) dG(A)} \int_\Lambda^\infty l_H(A, w) \varepsilon_H(A, w) dG(A)
\]

the average elasticities of functions \( l_F(A, w) \) and \( l_H(A, w) \).

Then, we get the expression for the elasticity of labor demand in the current period with respect to its contemporaneous labor cost when the change in labor cost is due to a change in the wage \( w \):

\[
\varepsilon_w = \frac{wdL}{L dw} = (1 - \pi) \bar{\varepsilon}_F + \pi \bar{\varepsilon}_H
\]  \hspace{1cm} (A10)

where \( \pi \) stands for the share of jobs of firms that hire workers during the period in the total number of jobs.

**A.2.2 Non conditional hiring credit**

Assume now that there is a non conditional hiring credit that provides a subsidy \( \sigma > 0 \) per hire. The average cost per worker in a firm with employment \( L \) is

\[
\psi = w - \sigma \max \left( \frac{H}{L}, 0 \right)
\]

Since the hiring credit \( \text{zéro charges} \) did not induce firms to fire workers to replace them by less costly entrants, as shown in section [1.3], we neglect cases 1 and 3 of the solutions of the model of appendix [A.1].

This implies that employment and hires are still defined by cases 2 and 4. Let us denote by \( \rho \) the share of eligible firms with positive take up to the hiring credit. Equations [A7] and [A8] imply that total employment can be written

\[
L = \int_0^\Lambda l_F(A, w) dG(A) + (1 - \rho) \int_\Lambda^\infty l_H(A, w) dG(A) + \rho \int_\Lambda^\infty l_H(A, w - \sigma) dG(A)
\]

where \( \check{\Lambda} \) satisfies, as previously, \( l_F(\check{\Lambda}, w) = l_H(\check{\Lambda}, w) \).

We look for the impact of a change in the average cost of labor among all firms, that we denote by \( \check{\psi} \). By definition, we have

\[
\check{\psi} = w - \sigma \rho \int_\Lambda^\infty h(A, w - \sigma) dG(A)
\]

The derivative of the labor cost \( \check{\psi} \) with respect to \( \sigma \) in the neighborhood of \( \sigma = 0 \) (where \( \check{\psi} = w \)) is

\[
\frac{d\check{\psi}}{d\sigma} = -\rho \int_\Lambda^\infty h(A, w) dG(A)
\]
Accordingly, the derivative of employment with respect to \( \psi \) induced by a change in \( \sigma \) in the neighborhood of \( \sigma = 0 \) is

\[
\frac{dL}{d\sigma} \frac{d\sigma}{d\psi} = \int_A^\infty h(A, w) dG(A) \int_A^\infty \frac{\partial l_H(A, w)}{\partial w} dG(A)
\]

With this formula, we can write the elasticity of labor demand in the current period with respect to its contemporaneous labor cost when the change in labor cost is due to a hiring credit non conditional on net job creation as

\[
\varepsilon_{\sigma} = \frac{\psi}{\bar{\psi}} \frac{dL}{d\sigma} \frac{d\sigma}{d\psi} = \frac{\pi \bar{\varepsilon}_H}{\bar{\eta}}
\]

where \( \pi = \frac{\bar{\psi}}{\bar{L}} \int_A^\infty l_H(A, w) dG(A) \) stands for the number of jobs of firms that benefit from the hiring credit divided by total employment absent the hiring credit and \( \eta = \frac{\bar{\psi}}{\bar{L}} \int_A^\infty h(A, w) dG(A) \) is the number of hires that benefit from the hiring credit divided by total employment absent the hiring credit.

From equations (A10) and (A11) we get the relation between the elasticities \( \varepsilon_{\sigma} \) and \( \varepsilon_{w} \):

\[
\varepsilon_{w} = \varepsilon_{\sigma} + (1 - \pi) \bar{\varepsilon}_F + \pi \bar{\varepsilon}_H \frac{\eta - 1}{\eta}
\]

This relation takes a simple form if we assume that \( \varepsilon \) is identical in all firms, i.e. \( \varepsilon_F = \varepsilon_H = \varepsilon = \varepsilon_w \). Formally, this assumption is a correct approximation if, for instance, the revenue function \( R \) is homogeneous with respect to \( L \) and the hiring and firing cost functions have little curvature, which means that employment adjusts quickly to its target. If \( \varepsilon \) is identical in all firms we get

\[
\varepsilon_{\sigma} = \frac{\pi \varepsilon}{\bar{\eta}}
\]

A.3 Conditional and non conditional hiring credit

In this appendix we show that

1. When the hiring credit is not conditional on net job creation:

   (a) In firms where firing costs and hiring costs are sufficiently low, the hiring credit induces employers to layoff incumbent workers to replace them by subsidized workers.

   (b) In firms where firing or hiring costs are sufficiently high, the hiring credit does not induce layoffs in order to hire subsidized workers.

2. When the hiring credit is conditional on net employment growth, its impact on hires and employment is the same as that of the hiring credit non conditional on net employment growth in firms which are in the case 1b above and which benefit from the conditional hiring credit (i.e. for which \( (L - L_{-1}) / L_{-1} > \gamma \), where \( \gamma \) denotes the employment growth threshold above which firms become eligible to the hiring credit).

To show 1a and 1b we use the model in cases where the hiring credit plays a role, i.e. in cases where the firm hires workers. When the hiring credit is introduced, the firm may layoff some workers to replace them by entrants whose cost is lower because they benefit from the hiring credit, which corresponds to case 1. To show 1a and 1b we describe the frontier between cases 1 and 2 in the space of firing costs and hiring costs. More precisely, we assume that the parameter values pin down the model in case 1. We consider an increase in the hiring and firing costs and derive the conditions when firms stop to fire any workers (and switch to case 2). We follow a perturbation method and assume that there exist \( \Delta_i, i = H, F \),
close to 0, such that the cost functions \( c_i \) are multiplied by \((1 + \Delta_i)\). Let us denote by \( H(\Delta_H, \Delta_F) \geq 0 \), \( F(\Delta_H, \Delta_F) \geq 0 \) and \( L(\Delta_H, \Delta_F) \geq 0 \) the solutions of the perturbed system characterized by \((\Delta_H, \Delta_F)\). They verify:

\[
R_L(A, L(\Delta_H, \Delta_F)) = w - \sigma + (1 + \Delta_H)c'_H(H(\Delta_H, \Delta_F)) - \beta E V_L(A', w', L(\Delta_H, \Delta_F)) \\
\sigma = (1 + \Delta_H)c'_H(H(\Delta_H, \Delta_F)) + (1 + \Delta_F)c'_F(F(\Delta_H, \Delta_F)) \\
L(\Delta_H, \Delta_F) = (1 - q)L_{-1} + H(\Delta_H, \Delta_F) - F(\Delta_H, \Delta_F)
\]

Note that the perturbation does not affect the derivative of the value function with respect to past employment. Because the perturbation is small, we can express the solutions of the perturbed system as deviations from the solutions of the initial system: \( L(\Delta_H, \Delta_F) = L + dL, H(\Delta_H, \Delta_F) = H + dH \) and \( F(\Delta_H, \Delta_F) = F + dF \). Then the perturbed system can be approximated at the first order as follows:

\[
dL \left[ R_{LL}(A, L) + \frac{\beta(1 - q)}{1 - \beta(1 - q)} E R_{LL}(A', L) \right] = \Delta_H c'_H + dH c'_H \\
0 = \Delta_H c'_H + dH c'_H + \Delta_F c'_F + dF c'_F (A16)
\]

where we use the envelop theorem to derive \( V_L \) in case 1. We have:

\[
V_L(A, w, L_{-1}) = (1 - q) [R_L(A, L) - w] + (1 - q) \beta E V_L(A', w', L)
\]

which yields in a stationary environment:

\[
V_L(A, \sigma, L) = \frac{(1 - q) [R_L(A, L) - w]}{1 - \beta(1 - q)}
\]

Let us denote \( B = R_{LL}(A, L) + \frac{\beta(1 - q)}{1 - \beta(1 - q)} E R_{LL}(A', L) \). Because of the concavity of the revenue function, \( B \) is negative. We consider the case when \( \Delta_F = 0 \). Then we can solve the above system and obtain:

\[
\Delta_H = -\frac{c'_H(c'_F - B)}{c'_H c'_F - B(c'_H + c'_F)} (A18) \\
\Delta_L = -\frac{c'_H c'_F}{c'_H c'_F - B(c'_H + c'_F)} (A19) \\
\Delta_F = \frac{c'_F c'_H - B(c'_H + c'_F)}{c'_H c'_F - B(c'_H + c'_F)} (A20)
\]

This shows that hires, layoffs and employment decrease with the hiring costs. Similarly we can consider the case where \( \Delta_H = 0 \). Noticing that \( B dL = -c'_F \Delta_F - c'_L dF \) we get:

\[
\Delta_H = \frac{c'_F B}{c'_H c'_F - B(c'_H + c'_F)} (A21) \\
\Delta_L = \frac{c'_F c'_H}{c'_H c'_F - B(c'_H + c'_F)} (A22) \\
\Delta_F = -\frac{c'_F(c'_H - B)}{c'_H c'_F - B(c'_H + c'_F)} (A23)
\]

We can conclude that hires and layoffs decrease when firing costs increase. The resulting effect on employment is positive.

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In case 1, the derivatives of $L$ with respect to $\Delta_F$ and $\Delta_H$ are negative. From equations (A4) and (A5) we can write the equation that defines $F(\Delta_F, \Delta_H)$ as a function of $L(\Delta_F, \Delta_H)$:

$$(1 + \Delta_F)c_F(F(\Delta_F, \Delta_H)) = w - R_L(A, L(\Delta_F, \Delta_H)) - \beta \mathbb{E}V_L(A', w', L(\Delta_F, \Delta_H))$$  \hspace{1cm} (A24)

We find the slope of the frontier $F(\Delta_F, \Delta_H) = 0$ in the $(\Delta_F, \Delta_H)$ plane by differentiating this equation with respect to $\Delta_F$ and $\Delta_H$. We get:

$$\frac{d\Delta_F}{d\Delta_H} = - \frac{c_F(0) + [R_{LL}(A, w', L) + \beta \mathbb{E}V_{LL}(A', w', L)]}{c_F(0) + [R_{LL}(A, w', L) + \beta \mathbb{E}V_{LL}(A', w', L)]} < 0$$

Thus, the slope in the $(\Delta_F, \Delta_H)$ plane is negative. Since equation (A24) implies that $F$ decreases with $\Delta_H$, case 1, where $F > 0$ and $H > 0$ lies below the frontier and case 3, where $F = 0$ and $H > 0$ lies above the frontier. This proves 1a and 1b in section 5.2.

Let us now show claim 2. Assume that the hiring credit is conditional on net job creation for all jobs created above the threshold employment growth rate $\gamma$. The value function of the firm reads

$$V(A, w, L_{-1}) = \max_{(H,F)} R(A, L) - wL + \sigma \max [L - (1 + \gamma)L_{-1}, 0] - c_H(H) - c_F(F)$$

subject to the law of motion of employment (A1) and to $H \geq 0$, $F \geq 0$ and $F \leq (1 - q)L_{-1}$.

Assume that $L > (1 + \gamma)L_{-1}$, the first order conditions are:

$$R_L(A, L) - w + \sigma - c_H(H) + \beta \mathbb{E}V_L(A', w', L) + \lambda_H = 0$$  \hspace{1cm} (A25)

$$-R_L(A, L) + w - c_F(F) - \beta \mathbb{E}V_L(A', w', L) + \lambda_F - \tilde{\lambda}_F = 0$$  \hspace{1cm} (A26)

When $H > 0$, $H$ and $L$ are determined by the first order condition:

$$R_L(A, L) - w + \sigma - c_H(H) + \beta \mathbb{E}V_L(A', w', L) = 0$$

and by the law of motion of employment (A1). This is the same system of equations as in case 2 where the hiring credit is not conditional on net employment growth. This proves 2 in section 5.2.

A.4 Labor demand elasticity with respect to conditional hiring credit on net job creation

In this appendix, we compute the elasticity of labor demand with respect to labor cost when the change in labor cost is induced by a conditional hiring credit on net job creation above the employment growth rate threshold $\gamma$. The hiring credit obtained by a firm with employment $L$ amounts to $\sigma \max [L - (1 + \gamma)L_{-1}, 0]$.

We assume that the economy is in situation 1b of appendix A.3 where the non conditional hiring credit does not induce layoffs in order to hire subsidized workers. Using the result 2 of appendix A.3 according to which a conditional hiring credit on net employment growth has the same impact on hires and employment as that of a non conditional hiring credit in firms eligible to both types of credit, we can use equations (A7) and (A8) to define total employment as:

$$\mathcal{L} = \int_0^A l_F(A, w)dG(A) + \int_A^\infty l_H(A, w)dG(A) + (1 - \rho) \int_A^\infty l_H(A, w)dG(A) + \rho \int_A^\infty l_H(A, w - \sigma)dG(A)$$
where $\bar{A}$ satisfies $l_E(\bar{A}, w) = l_H(\bar{A}, w)$; $\bar{A}$ satisfies $l_H(\bar{A}, w) = (1 + \gamma)L_{-1}$ and $\rho$ is the share of eligible firms that benefit from the hiring credit.

The average labor cost per employee is

$$\bar{\psi} = w - \frac{\rho}{E} \int_{A}^{\infty} [l_H(A, w - \sigma) - (1 + \gamma)L_{-1}] dG(A)$$

The derivative of the labor cost $\bar{\psi}$ with respect to $\sigma$ in the neighborhood of $\sigma = 0$ (where $\bar{\psi} = w$) is

$$\frac{d\bar{\psi}}{d\sigma} = -\frac{\rho}{E} \int_{A}^{\infty} [l_H(A, w) - (1 + \gamma)L_{-1}] dG(A)$$

Accordingly, the derivative of employment with respect to $\bar{\psi}$ induced by a change in $\sigma$ in the neighborhood of $\sigma = 0$ is

$$\frac{dL}{d\sigma} \frac{d\sigma}{d\bar{\psi}} = \frac{L}{\int_{A}^{\infty} [l_H(A, w) - (1 + \gamma)L_{-1}] dG(A)} \int_{A}^{\infty} \frac{\partial l_H(A, w)}{\partial w} dG(A)$$

With this formula, we can write the elasticity of labor demand in the current period with respect to its contemporaneous labor cost when the change in labor cost is due to a conditional hiring credit on net job creation above the employment growth rate threshold $\gamma$ as

$$\varepsilon_H \equiv \frac{\bar{\psi} dL \sigma}{L \sigma d\bar{\psi}} = \frac{1 + \Gamma(\gamma)}{\Gamma(\gamma) - \gamma} \varepsilon_H$$

where $\varepsilon_H = \frac{1}{\int_{A}^{\infty} l_H(A, w) dG(A)} \int_{A}^{\infty} l_H(A, w) \varepsilon_H(A, w) dG(A)$ is the average elasticity of labor demand with respect to the wage $w$ of firms that grow above the threshold $\gamma$ absent the subsidy; and $\Gamma(\gamma) = \frac{1}{1 - G(A)} \int_{A}^{\infty} \left( \frac{l_H(A, w) - L_{-1}}{L_{-1}} \right) dG(A)$ is the average employment growth of firms that grow above the threshold $\gamma$ absent the hiring credit. If we assume that the elasticity of labor demand with respect to the wage $\varepsilon$ is identical in all firms, i.e. $\varepsilon_H = \varepsilon$ we get

$$\varepsilon_H = \frac{1 + \Gamma(\gamma)}{\Gamma(\gamma) - \gamma} \varepsilon$$

A.5 The BMO survey

Recruitment shortages are surveyed each year by the public employment service thanks to a questionnaire called Besoins de Main d’Oeuvre (BMO). This survey provides annual measures of recruitment difficulties in 388 local employment pools, and 24 industries providing each year 8,622 estimates (some industries are not systematically present in all employment pools). Firms are requested to provide, for the coming year, the number of recruitments they plan, how many relate to seasonal needs, and how many of these recruitment projects are considered difficult. It covers all private firms as well as some publicly-owned firms and organizations, or a total of 2.3 million plants. The majority of questionnaires are sent by post, and the questionnaire features a response rate of about 24%. Answers are then appropriately weighted so that the survey is representative.

Each employment pool is made of one or several municipalities, which are coded according to a national classification. The industry taxonomy used in this survey can be linked with the detailed classification used by INSEE in the DADS. This permits to match each firm in our DADS sample with the ratio of difficult recruitments to the total number of recruitments planned in the employment zone and the industry to which the firm belongs. This ratio is presented in table [12] for the years used in our sample (based on 363 employment zones for which we have observations in the sample).
A.6 Cost-benefit analysis

In order to evaluate the savings permitted by zéro charges on social benefits, we use a survey conducted by the public employment service Pôle Emploi in November - December 2009 on the beneficiaries of zéro charges. Pôle Emploi interviewed 3,083 firms and a total of 3,996 employees who benefited from zéro charges between January 1 and June 30, 2009 out of 270,755 beneficiaries recorded during that period. The survey collected the gender, age, education of the recruits, the main reason for recruitment (creation of a new job, replacement of another worker, contract renewal, temporary needs, etc.), as well as the type of contract (permanent of temporary) offered, the profession, the monthly wage and the sector of firms. More interestingly, it also included a question on the personal situation of workers immediately before the recruitment took place: employed, registered or unregistered unemployed, in training or at school, on sick or maternal leave, or inactive. The corresponding breakdown is presented in table 13 for workers less than 26 years old (64% of the recruits) and those 26 years old or more (36%).

We use this information to estimate the social cost of the counterfactual situation without zéro charges. To do so, we look for the typical social benefits attached to these situations. In 2009, the average unemployment insurance benefit (called Allocation de Retour à l’Emploi) was 970 euros per month, but only 50% of the registered unemployed received it (DARES, 2012). About 10% received the unemployment assistance (called Allocation de Solidarité Spécifique, a means tested scheme) which amounted to 450 euros. Another 10% received the minimum income (called Revenu de Solidarité Active, also about 450 euros for a single without children), and 30% did not receive any benefit. This gives a (weighted) average cost of 575 euros for the registered unemployed. As for those not registered, they do not receive unemployment benefits as registration is a prior condition. But they are eligible to the minimum income of 450 euros per month, as well as inactive people, for which studies show a typical take-up rate of 2/3. This provides an average cost of 300 euros per month for the unregistered unemployed and the other inactive, but only for those 26 years old or older, since younger unemployed / inactive people are not eligible to this minimum income scheme. Students may be eligible to scholarship but there are rather rare. The main benefit for students is one of the three main housing benefits schemes, which average amount is about 200 euros per month. We apply the same take-up of 2/3, as for the minimum income, which gives an average benefit of 133 euros per month for students. For trainees, there is a specific benefit (called ARE formation) for those unemployed and eligible to the insurance benefit which was 975 euros on average in 2009. Since about only half of the unemployed are eligible to the insurance benefit, we apply a take-up rate of 50% which gives a monthly cost of about 485 euros. There might be other benefits for non-employed trainees but they are scarcer and we neglect them. Finally, as a counterfactual we consider that, in the absence of the jobs created by zéro charges, those employed immediately before being hired on these jobs would have been unemployed otherwise, and would then have received the same average benefit as the registered unemployed (since they would just end an employment period they would probably register rather than not to receive job search support and unemployment benefits). Adding all these benefits, and using the weights of the various populations (less or more than 26 years old, and by status), as provided in table 13 gives an average benefit per worker of 460 euros per month. To these savings one must add the social contributions paid by the additional employees hired on jobs created by zéro charges, which amount to 23% of gross wages, or about 235 euros per month on average given observed the hiring wages. All in all, each job created by zéro charges generates monthly net savings of 695 euros. This estimates excludes the cost of social in-kind services (such as counselling, case-management and health services) typically more important for unemployed and inactive than the employed people.

36See http://www.social-sante.gouv.fr/IMG/pdf/1_Le_non-recours au_rSa_et_ses_motifs.pdf
It also takes in account only the basic amount of the minimum income, excluding all supplements for couples and children.
Figure 15: Average growth rate of employment in firms in the treated and control groups in different sectors. Growth rate of employment between 30 November of year $t-1$ and year $t$. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November).

A.7 Supplementary Figures and Tables
Table 12: Ratio of the number of difficult recruitments to the total number of planned recruitments by year

<table>
<thead>
<tr>
<th></th>
<th>2006</th>
<th>2007</th>
<th>2008</th>
<th>2009</th>
</tr>
</thead>
<tbody>
<tr>
<td>Average ratio</td>
<td>.427 (.121)</td>
<td>.436 (.119)</td>
<td>.532 (.079)</td>
<td>.459 (.093)</td>
</tr>
<tr>
<td>Min</td>
<td>.136</td>
<td>.128</td>
<td>.304</td>
<td>.161</td>
</tr>
<tr>
<td>Max</td>
<td>.851</td>
<td>.799</td>
<td>.817</td>
<td>.916</td>
</tr>
<tr>
<td>Nb. Observations</td>
<td>363</td>
<td>363</td>
<td>363</td>
<td>363</td>
</tr>
</tbody>
</table>

Source: BMO (Pole Emploi). Note: Standard deviations in parentheses.

Table 13: The situation of workers hired with zero charges, immediately before recruitment

<table>
<thead>
<tr>
<th>Employed</th>
<th>Registered unemployed</th>
<th>Unregistered unemployed</th>
<th>Training</th>
<th>Education</th>
<th>Other</th>
<th>Inactive</th>
</tr>
</thead>
<tbody>
<tr>
<td>Less than 26 years old</td>
<td>29%</td>
<td>36%</td>
<td>5%</td>
<td>5%</td>
<td>18%</td>
<td>7%</td>
</tr>
<tr>
<td>26 years old or more</td>
<td>42%</td>
<td>39%</td>
<td>5%</td>
<td>4%</td>
<td>4%</td>
<td>8%</td>
</tr>
</tbody>
</table>

Source: Pole Emploi.

Table 14: Number of eligible/ineligible firms in the sample in 2008

<table>
<thead>
<tr>
<th>Nb employees</th>
<th>Number of firms</th>
<th>Number of employees (end of 2008)</th>
</tr>
</thead>
<tbody>
<tr>
<td>all</td>
<td>below 10</td>
<td>above 10</td>
</tr>
<tr>
<td></td>
<td>832,910</td>
<td>146,811</td>
</tr>
<tr>
<td>+ excl. temporary temp, associations &amp; agriculture</td>
<td>654,047</td>
<td>123,177</td>
</tr>
<tr>
<td>+ trimming extreme values</td>
<td>647,230</td>
<td>120,075</td>
</tr>
<tr>
<td>+ Excluding missing control variables</td>
<td>499,381</td>
<td>112,442</td>
</tr>
<tr>
<td>6-10 and 10-14 empl. only</td>
<td>70,998</td>
<td>30,912</td>
</tr>
</tbody>
</table>

Source: DADS (Insee). Note: The number of employees is the total number of employees per firm at the end of November 2008.
Table 15: Difference-in-differences estimates for surviving firms

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Employment growth</td>
<td>.009***</td>
<td>.008***</td>
<td>.009***</td>
</tr>
<tr>
<td></td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.002)</td>
</tr>
<tr>
<td>Hours growth</td>
<td>.010***</td>
<td>.009***</td>
<td>.009***</td>
</tr>
<tr>
<td></td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.002)</td>
</tr>
<tr>
<td>Hiring rate</td>
<td>.014***</td>
<td>.012***</td>
<td>.019***</td>
</tr>
<tr>
<td></td>
<td>(.005)</td>
<td>(.004)</td>
<td>(.005)</td>
</tr>
<tr>
<td>Separation rate</td>
<td>.005</td>
<td>.004</td>
<td>.010*</td>
</tr>
<tr>
<td></td>
<td>(.005)</td>
<td>(.004)</td>
<td>(.005)</td>
</tr>
<tr>
<td>Nb. Observations</td>
<td>399,412</td>
<td>399,412</td>
<td>203,889</td>
</tr>
</tbody>
</table>

Source: DADS (Insee). Note: this table presents our difference-in-differences estimates for different outcomes (lines) and different specifications (columns) for surviving firms. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t and year t-1; the growth rate of the number of hours worked between November of year t and November of year t-1; the number of hires from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; and the number of excess reallocation from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1. As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 millions euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.
Table 16: Difference-in-differences estimates with weighted observations

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Covariates</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Employment growth</td>
<td>.009***</td>
<td>.008***</td>
<td>.008***</td>
</tr>
<tr>
<td></td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.002)</td>
</tr>
<tr>
<td>Hours growth</td>
<td>.010***</td>
<td>.009***</td>
<td>.008***</td>
</tr>
<tr>
<td></td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.002)</td>
</tr>
<tr>
<td>Hiring rate</td>
<td>.014***</td>
<td>.0121***</td>
<td>.018***</td>
</tr>
<tr>
<td></td>
<td>(.005)</td>
<td>(.004)</td>
<td>(.005)</td>
</tr>
<tr>
<td>Separation rate</td>
<td>.004</td>
<td>.003</td>
<td>.010*</td>
</tr>
<tr>
<td></td>
<td>(.005)</td>
<td>(.004)</td>
<td>(.005)</td>
</tr>
<tr>
<td>Survival rate</td>
<td>.000</td>
<td>.000</td>
<td>.000</td>
</tr>
<tr>
<td></td>
<td>(.001)</td>
<td>(.001)</td>
<td>(.001)</td>
</tr>
<tr>
<td>Nb. Observations</td>
<td>405,376</td>
<td>405,376</td>
<td>206,845</td>
</tr>
</tbody>
</table>

Source: DADS (Insee). Note: this table presents our difference-in-differences estimates for different outcomes (lines) and different specifications (columns) when firms are weighted according to their size as measured by the number of full time equivalent employees in the previous year. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t; the growth rate of the number of hours worked between November of year t-1 and November of year t; the number of hires from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.