

Do Hiring Credits Work in Recessions ? Evidence from France*

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Abstract

This paper evaluates the impact of an unexpected 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 rapid impact on employment. The net cost per job created for the government was around zero. The employment effect 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, we estimate that a credit conditional on net job creation above the employment growth threshold of -1% , would have maximized job creation, and created about 4 times more jobs, at constant budget, provided that take-up had remained the same.

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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 hired in firms with less than 10 employees at a wage less than 1.6 times the minimum wage were immediately eligible for 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, decreasing in linear fashion as the hourly wage level rose 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.¹ Despite this wide use of hiring credits, many economists think that they are probably useless during recessions, where aggregate demand is insufficient relative to the labor and other resources available in the economy.² However, very little is known about the actual effects of hiring credits, because empirical evidence is very scarce.

The *zéro charges* program provides an exceptional opportunity to contribute to filling this gap. This policy measure has several important advantages for the purpose of evaluation. 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, allowing 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, as well as the take-up of the measure.

¹See OECD (2010) for a detailed presentation of hiring credit measures in 2009.

²For instance, Becker (2010), Posner (2010) and Gali (2013).

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 smaller in absolute value, but still above one. This finding is consistent with previous estimates which have shown 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 those of incumbent employees. We find no increase in wages 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. Nevertheless, we find that the hiring credit is not always effective. We use a survey on recruitment shortages which allows us to show that the employment effects of the hiring credit decrease when recruitment difficulties increase. This suggests that hiring credits are more effective at boosting employment in downturns than in upturns, when labor markets are tight.

We find that *zéro charges* did not induce firms to increase layoffs in order to hire workers at lower cost. However, the level of churning of workers in France is high because 90 percent of entries into employment are on temporary jobs. As a consequence of this high churning, the hiring credit induces large windfalls for firms. We estimate that about 84 percent of the hires subsidized by *zéro charges* would have been created absent the hiring credit. The gross cost of the hiring credit per job created borne by the government is significant: it amounts to about a quarter of the labor cost of a job in the treatment group. To compute the cost per job created *net* of savings on social benefits, we exploit a survey that provides information about

the characteristics of the beneficiaries of *zéro charges*. We find that the hiring credit has been very effective since the net cost of the hiring credit per job created is about zero.

The importance of windfalls for firms led us to explore the potential impact of hiring credits conditional on net job creation, like the New Job Tax Credit (NJTC), which subsidized growing firms during the late 70s in the US, compared with unconditional credits like *zéro charges* or the Hiring Incentives to Restore Employment (HIRE) Act set out by the Obama administration in 2009.³ 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 4.4 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 hiring credits has been recently surveyed in Neumark (2013) who concludes that they do not have significant effects on employment when they are targeted at specific disadvantaged groups (such as 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 during recessions. The evidence comes from two empirical evaluations of the NJTC (Perloff and Wachter, 1979, Bishop 1981). The NJTC was the only US hiring credit implemented at the federal level before the HIRE Act. But at the state level, there were many more hiring credits. Using difference-in-differences across states, Chirinko and Wilson (2010) estimate that these hiring credits increased employment during the month when firms both know about, and can qualify for, the hiring credit. Using a similar identification strategy on a wider set of hiring credits, Neumark and Grijalva (2013) find that hiring credits targeting the unemployed, and those that allow US states to recover credits when job creation goals are not met, appear to have succeeded in boosting job growth. Neumark and Grijalva also point out that the inefficiencies that arise with certain types of hiring credits are related to churning behaviors. In European countries, most evaluations are focused on tax exemptions that reduce labor costs. These exemptions are not targeted at hires: they

³Like *zéro charges*, the HIRE Act was not restricted to growing firms and it was accessible to a rather large pool of potential candidates. However, in 2009 the Obama administration originally set out a hiring credit targeting growing firms only. Eventually, the Congress passed a measure which exempted all employers from their share of Social Security taxes on wages when hiring unemployed workers into new or existing positions, on top of a general business tax credit for each worker retained for at least a year (see <http://hireact.org/>). These convoluted policy discussions and legislative debates also surrounded the New Job Tax Credit in the 70s. Despite the complexity of conditionalities, among the 147 hiring credits enacted separately by U.S. states from 1969 to 2012, 143 required that the number of jobs associated with hires is above specified thresholds (Neumark and Grijalva, 2013).

apply to the stock of jobs. They have positive employment effects in countries where there are high statutory minimum wages, such as France⁴ and Belgium.⁵ In countries without a statutory minimum wage, such as Finland⁶ or Sweden,⁷ results are more mixed.

We add to the literature by providing the first empirical evaluation of an unexpected temporary hiring credit using sufficiently rich data at the firm level to analyze its effects on a wide range of outcomes including employment, hours of work, labor turnover, wages, firms survival and the cost per job created. We show empirically that the impact of hiring credits depends on labor market conditions and in particular on hiring difficulties. We also provide a theoretical framework that clarifies the impact of different types of hiring credits and employment subsidies.

The paper is organized as follows. Section 2 describes the hiring credit scheme implemented in France in 2009 (*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 of *zero charges* per job created and to alternative hiring credit designs. Robustness checks are presented in section 6. The last section concludes.

2 Institutional background

The *zéro charges* (zero contributions) measure was announced by the French President on 4 December 2008. According to the original announcement, any hire (or temporary contract renewal) of a low-wage worker in a firm with less than 10 employees occurring from the date of the announcement until 31 December 2009 could benefit during the same year from an employer social contribution relief.⁸ The relief is maximal for workers with an hourly wage at the minimum wage level (1,338 euros in 2009). With *zéro charges*, employers do not pay any social contribution

⁴Crepon and Desplatz (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 1990s 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.

⁵Goos and Konings (2007) find a positive employment effect of payroll tax subsidies in Belgium.

⁶Huttunen et al. (2013) do not find any positive employment effect of wage subsidies targeted at older, full-time, low-wage workers in Finland.

⁷Sianesi (2008) finds that entering a temporary job subsidy program rather than searching further in open unemployment increases employment rates soon after the program ended in Sweden in the 1990s. Benmarker 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. Egebark and Kaunitz (2013) and Skedinger (2014) find that payroll tax cuts for young workers implemented in Sweden in 2007 and 2009 had weak positive effects on youth employment.

⁸The new relief is in addition to the existing general social contribution reduction on low wages called the *Fillon reduction*, which has prevailed since the 1990s and concerns all firms in the private sector.

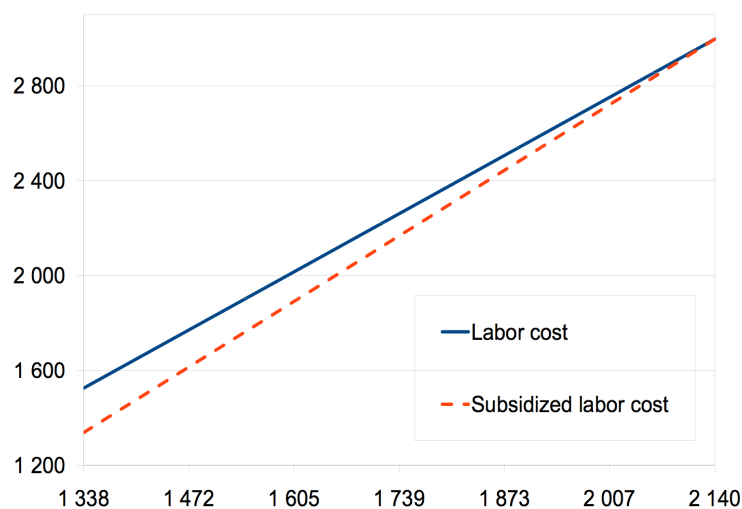


Figure 1: The hiring credit schedule.

Note: The horizontal axis reports the monthly wage (in euros) net of employer social contributions of a full time worker (1 338 corresponds to the minimum wage in 2009, 1 472 is 1.1 times that amount, 1 605 is 1.2 times and so on). The vertical axis reports the monthly labor cost. The continuous line displays the labor cost without the hiring credit. The dotted line shows the labor cost with the hiring credit.

at the minimum wage level. The relief then decreases as the hourly wage level rises up to 1.6 times the minimum wage. Figure 1 shows that 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 percent above the minimum wage, the subsidy rate represents only 4 percent of the labor cost.

Before the first announcement, the policy was not anticipated, because it was kept secret.⁹ This is illustrated by Figure 2 which shows that Google searches for the item “hiring subsidy” (*aide embauche*) started to increase in December 2008, once the announcement for the program was made. There is no Google search for the item *zéro charges* before early 2009.

The practical details of the hiring credit were rapidly make known by decree on 20 December 2008. To start with, only firms and associations belonging to the private sector could get the

⁹See for instance the newspaper *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.

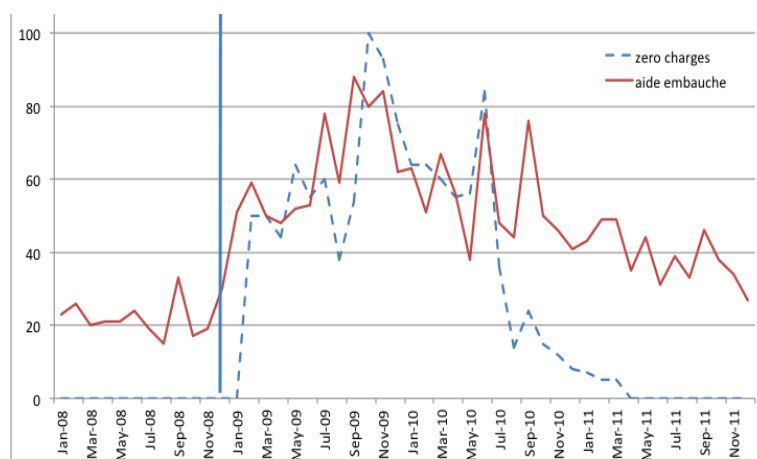


Figure 2: Results of Google search for the policy name.

Note: The vertical axis reports the monthly number of searches for one term relative to the highest point on the figure; "aide embauche" means hiring subsidy. Variations in spellings (e.g. "zéro charges", "zero charge") yield similar patterns as "zero charges". Source: Google Trends website.

hiring credit. Firms and associations had to request the additional *zéro charges* relief 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 had to be for 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. the 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 employees¹⁰ 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. A growing firm reaching 10 or

¹⁰The size criteria are very precise and follow the usual rules set in the labor code (see cerfa n° 13838-01). Only ordinary employees are kept in the computation of the size (thus excluding apprentices and sponsored employees and those hired as part of a labor market program). The size is computed as the average of the end-of-month number of employees from January to November 2008. Temporary workers contribute *pro rata temporis* their number of days present in the firm over the month. This means that temporary workers hired on the 15th of the month working full-time represent 0.5 employees. However, workers hired on permanent contracts are counted as 1 employee during the month no matter what day of the month they were hired on. All wage-earners working part-time, either on temporary or permanent contracts, are accounted *pro rata temporis* their regular number of hours during the month, excluding overtime hours. For instance, wage-earners working mornings only are counted as 0.5 employee.

more employees over the year 2009 could still continue to receive subsidies and apply for new hires until the end of 2009. This meant that the size criteria could not be manipulated by firms wishing to benefit from the hiring credit.

Fourth, applying firms must not have fired any workers for economic reasons on the same job over the 6 months preceding the hiring date, nor must they have fired this particular worker over the same period from any other job, and they must have paid all their previous social contributions.

On 16 November 2009, the policy was extended to hires occurring up to 30 June 2010. On this occasion, the duration of the hiring credit was extended for up to 12 months from the hiring date, instead of the cutoff date 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 also eligible for the extended program for their new hires in 2010. Hence it is more challenging to study the effects of the policy in 2010, as some firms treated in 2009 may not have been able to apply in 2010, because eligibility for the extended period was then based on the average size over 2009. Moreover, the period after the termination of *zéro charges* cannot be studied yet, since *zéro charges* provided subsidies until 30 June 2011 and the data for year 2011 are not yet available.

The hiring credit was initially part of a wider array of policies designed to cope with the 2008-2009 crisis. Within that array, 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 government budget constraints. Broadly speaking, there were no other explicit legal changes in this period that exerted a varying impact on firms with less or more than 10 employees.

As well, there are no significant discontinuities at the 10-employee threshold in the French legislation that might induce 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 the threshold (see Figure 3). We can thus be confident that there is an absence of sorting around the size threshold. Such a sorting might have meant that firms below and above the threshold were reacting differently to the business cycles.¹¹

¹¹This contrasts with the findings of Gourio and Roys (2012) who report large accumulation at the 50 employees threshold and moderate at the 10 employees threshold. Their evidence on small firms is less reliable than ours,

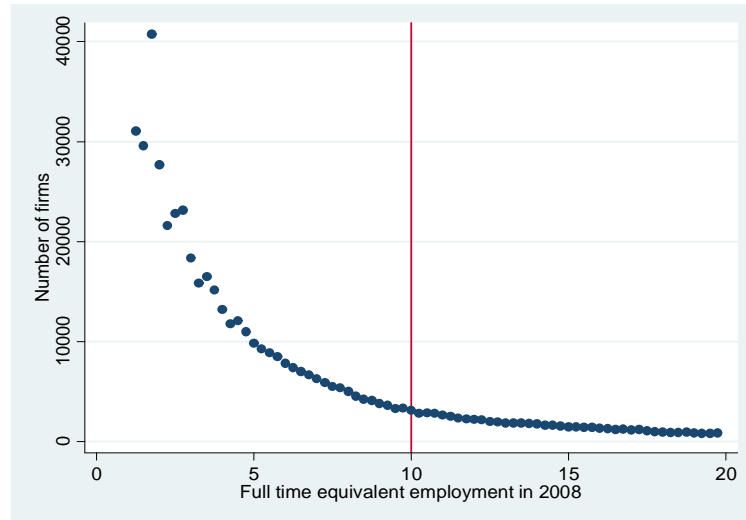


Figure 3: Firm size density. Source DADS.

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 Statistical Institute (INSEE) from the social contributions declarations of firms. Each year firms declare the employment spells, the number of hours worked, and the associated wages for each worker.
- the administrative file produced by the French Public Employment Agency (*Pôle emploi*) which administered the payment of the subsidy, designated as the “hiring credit” file. It contains information on the firms which enrolled in the *zéro charges* program, 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 (government ministries) and workers from the public health care sector or employed by householders (e.g. for house-keeping or child care) do not appear in this employ-

as they do not observe the whole universe of small firms. Their data is based on a specific fiscal declaration that does not cover all small firms.

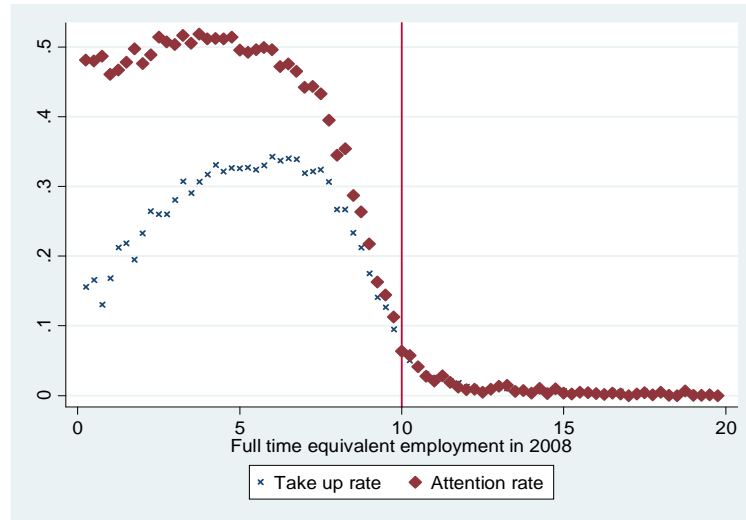


Figure 4: Fraction of firms and of hires that benefited from the hiring credit by firm size in 2009. Note : The take-up rate is the share of firms below ten employees benefiting from the hiring credit in 2009; the attention rate is the share of hires with wages below 1.6 times the minimum wage that have been subsidized in 2009. The firms' size is the number of full time equivalent employees as measured over the first 11 months of 2008.

ment register (until 2009). We append the employment registers from 2005 to 2009,¹² creating a panel of firms.¹³ We restrict the sample to firms in the for-profit private sector and we drop the agricultural 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.¹⁴ All relevant information pertaining to firm size, the number of hires, separations, the wage levels and the duration of contracts are taken from the DADS data set 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.

Our two 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

¹²The specification about the type of labor contract, either temporary or permanent, is not available before 2005. Since the type of contract is used to compute the number of full time equivalent workers, as explained in footnote 10, we cannot use the DADS before 2005.

¹³There is no permanent identifier for individual workers. Our data is not a panel of individual workers.

¹⁴Further information on data is available in table 14 in the appendix.

we define as the attention rate. The attention rate (the share of subsidized hires among eligible hires with wages below 1.6 times the minimum wage and contract duration above one month) amounts to 47%. Figure 4 displays the take-up rate and the attention rate by firm size in 2008 (i.e. according to 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 with a workforce of 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 consequences 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 have been 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 *zéro charges*. The resulting absence of discontinuity in the take-up rate prevents us from using a regression discontinuity design.

3.2 Empirical strategy

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

$$L = L_{-1} + H - S, \tag{1}$$

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 credits 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) and Posner (2010), reacting to the Hiring Incentives to Restore Employment (HIRE) Act passed in the US in 2010, argued that it will increase churning and wages with very little effect on employment. This reasoning is in line with that of 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 replace them with workers for whom they can collect the subsidy. The main impact of the hiring credit may thus be to increase churning and wages with very little effect on employment. 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 impact on hours worked is limited, because firms have incentives to substitute hours of subsidized employees for those of non subsidized employees. Therefore, it is also important to analyze the response 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:

$$Y_{it} = \alpha + \beta Z_{it} + \gamma D_{it} + \delta Z_{it} D_{it} + X_{it} b + u_{it} \quad (2)$$

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. δ 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. Note that by defining our eligibility dummy for every year, the treatment effect estimate is robust to potential mean-reversion bias that could occur if the definitions of the control and treatment groups were based

on the size of firms in 2008 only.

In the benchmark estimations, the bandwidth goes from 6 (included) to 14 (excluded) full time employees in the 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 size firms. Small (i.e. eligible) firms operate less frequently in manufacturing industry and slightly more often in retail, transport and merchant services than non-eligible medium size 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 of less than 2 million euros, while one medium-size firm out of four exceeds that mark. Small firms are also younger: 13 percent have existed for less than 5 years 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 both 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 and separations. 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, 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

The validity of difference-in-differences estimations is heavily dependent 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 in Figure 5. The outcome is average employment growth in each group.¹⁵ Employment is computed at the firm level.

¹⁵We focus on the effect of the hiring credit on the growth rate of employment rather than on the 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. $\bar{L}_t^C - \bar{L}_{t-1}^C = \bar{L}_t^T - \bar{L}_{t-1}^T$ where \bar{L}_t^j 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

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 $\frac{1}{N_t} \sum_i \frac{L_{i,t} - L_{i,t-1}}{L_{i,t-1}}$ where N_t is the number of firms in the group. Figure 5 shows that 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.¹⁶ 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 rows) 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 related to underlying trends). The results are very stable. They indicate that the hiring credit increased the employment growth rate of the treatment group by about 0.8 percentage points (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 on the subsample of surviving firms are identical to that of all firms, as shown in Table 15 in appendix.¹⁷

not surprising inasmuch as 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 ϕ . The optimal level of employment satisfies $F'(L) = w\phi$. 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. $dL = L\varepsilon d\phi/\phi$, where $\varepsilon = F'(L)/LF''(L)$ denotes the elasticity of labor demand with respect to the labor cost $w\phi$.

¹⁶The average employment growth is negative for the treatment group and the control group all along the period. This is because new entrants, which typically account for a significant share of employment growth, are excluded from the sample. Bear in mind that, by construction, we cannot include new entrants since we study the behavior of firms that had between 6 and 14 full time equivalent employees the previous year.

¹⁷Our 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 in the course of the paper yield results similar to weighted estimates. This is illustrated by Table 16 in appendix which shows the weighted estimates corresponding to those displayed Table in 2.

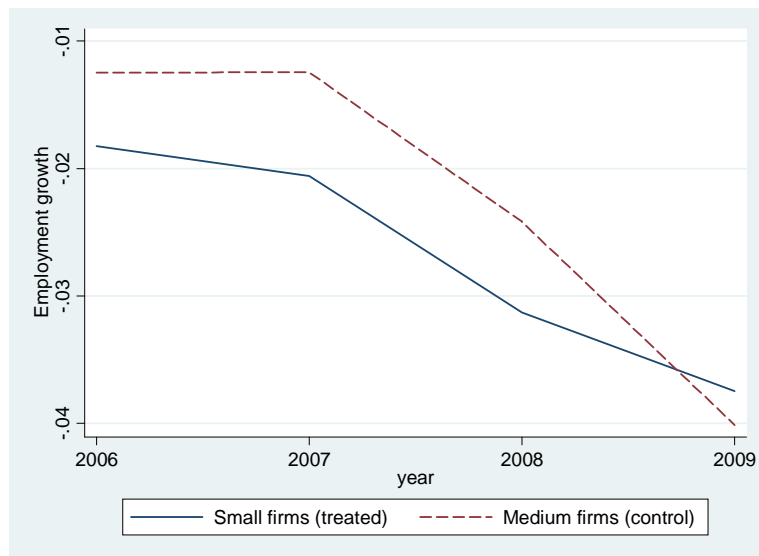


Figure 5: Average employment growth rate in firms in the treated and control groups.

Note: 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).

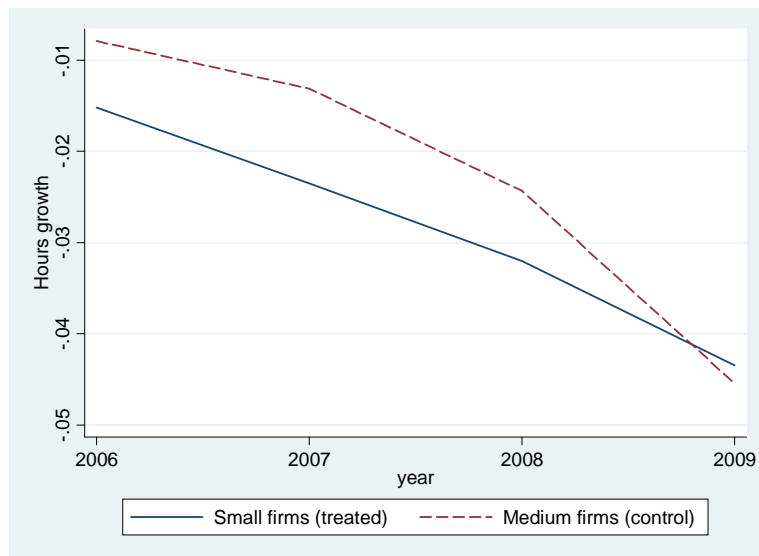


Figure 6: Average hours growth rate in firms in the treated and control groups.

Note: 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).

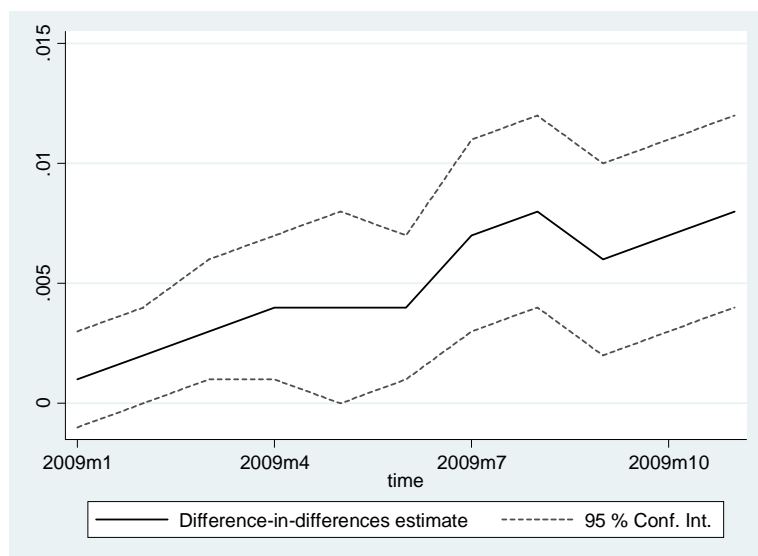


Figure 7: Difference-in-differences estimates of the impact of the hiring subsidy on the employment growth rate.

Note: The outcome is $(L_{m,t} - L_{t-1})/L_{t-1}$ 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 and covariates presented in Table 2, column 2.

Table 3 displays separately the impact of the hiring credit on eligible jobs – jobs paying below 1.6 times the minimum wage that last at least one month – and on ineligible jobs.¹⁸ The hiring credit has a strong positive and significant impact on employment and hours for eligible jobs only. The impact for non eligible jobs is rather positive, but not significantly different from zero. This means that the hiring credit has had a positive impact on total employment and total hours mainly through its impact on eligible jobs, and very marginally on ineligible jobs.

Our data set allows us to show the evolution of employment month-by-month over the year 2009. 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, with a delay that is clearly infra annual (Hamermesh, 2013).

¹⁸The number of observations in Table 3 is smaller than in Table 2 because there are firms without jobs 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 jobs either below or above 1.6 times the minimum wage.

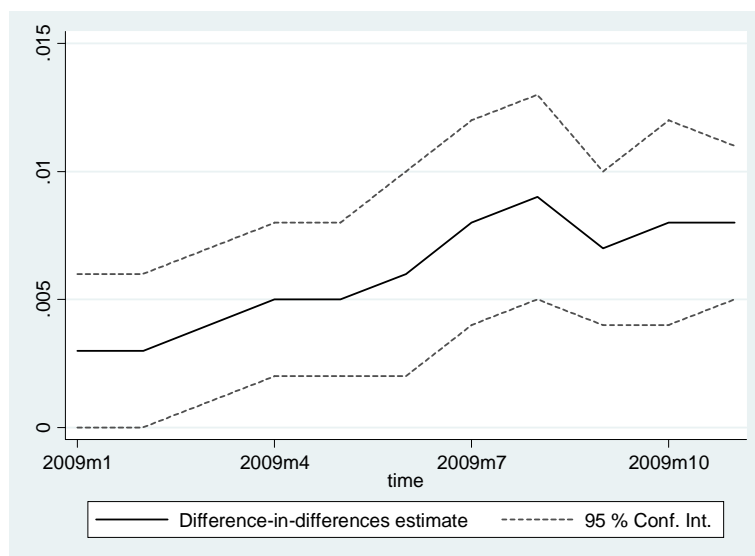


Figure 8: Difference-in-differences estimates of the impact of the hiring subsidy on the growth rate of hours of work.

Note: 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 percent¹⁹ 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 of around -4 , belonging to the 95% confidence interval $[-6, -2]$.

The strong employment impact of zero charges relies on the absence of wage increases and on the absence of increased churning of workers, as shown below. Even if wages and labor turnover did not increase, this figure may at first sight seem incredibly high, 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. Crepon and Desplatz (2001), using a different empirical strategy, found an elasticity equal to -0.8 for all workers.

It is important to remark, however, that the strong employment impact of *zéro charges*

¹⁹In November 2009, firms in the treatment group got 3.6 million euros from *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 the course of year 2009, *zéro charges* decreased the labor cost of firms in the treatment group by 0.14%. The amount of subsidies paid by *zéro charges* increased progressively during 2009.

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 ε 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 ε_σ stand for the elasticity of employment with respect to the average labor cost per worker when the change in average labor cost per worker is due to *zéro charges*. Bear in mind that the hiring credit alters the cost of entrants (i.e. new hires) only. The relation between employment and hires is given by the law of motion of employment (1). Let us assume that ε is identical in all firms and that the hiring credit does not increase churning of workers and wages, which is the case for *zéro charges* as shown below. We get (see appendix A.2):

$$\varepsilon = \eta\varepsilon_\sigma \tag{3}$$

where $\eta = H/L$ is the average hiring rate of firms with a positive number of hires eligible to *zéro charges*.²⁰ As long as $\eta < 1$, 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: all it does is to create windfalls for firms. Using hiring credits is a means to target subsidies at marginal jobs, that have positive employment effects, without providing subsidies to incumbent workers, which has no employment effects. In the limit case where $\eta = 1$, the two elasticities are identical because the entire workforce of firms that benefit from the hiring credit is subsidized.

All in all, we find that ε , 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. The 95% interval confidence of the elasticity ε is $[-3.6, -1.2]$,²¹ which is in line with previous estimates obtained for France.

²⁰Note that the hiring rate η is defined here as the ratio of the number of hires over employment, i.e. H/L , whereas the hiring rate is defined as H/L_{-1} in the rest of the paper.

²¹The hiring rate is computed over the period during which *zéro charges* was implemented (i.e. from 4 December 2008 to 30 November 2009). It amounts to 0.63 which implies that $\varepsilon = -4 \times 0.63 = -2.52$ with a 95% confidence interval equal to $[-3.6, -1.2]$.

4.2 Wages

The hiring credit may raise individual net wages. It may also induce firms to hire workers with fewer skills at 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.²² 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 the 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 wage changes in small and medium firms controlling for individual past wages. If the hiring credit did indeed have an impact on wages, that should be apparent for the entrants eligible for 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 we see for employment and hours of work, there is no break in the common trend in 2009. This suggests that the hiring credit 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 wage, and for the wages of entrants eligible for the hiring credit. In all cases, the estimates point to a null effect of the hiring credit on wages. This result is not surprising in the French context, where there is a high minimum wage and collective agreements that are most often binding for small firms and 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 might replace incumbent workers with new workers to benefit from the hiring credit.

Let us provide evidence which suggests that this is not the case. Using French data over

²²Note that although the DADS is not a panel, it does provide the wage in the previous year for each worker.

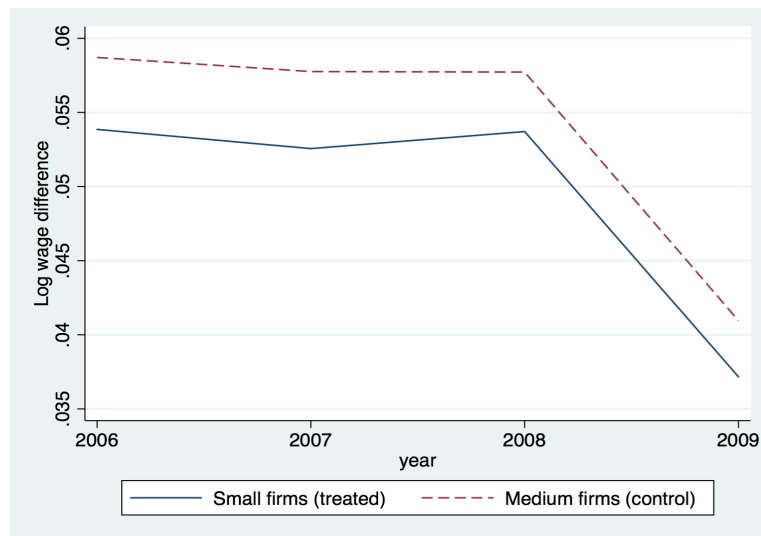


Figure 9: Average log wage difference of entrants eligible for the hiring credit in firms in the treated and control groups.

Note: 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 $\frac{1}{N_i} \sum_i \ln w_{it} - \ln \tilde{w}_{it-1}$ where w_{it} is the average hourly wage of eligible entrants in firm i in year t and \tilde{w}_{it-1} their average hourly wage in the 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 the 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 the previous year (average from 1 December to 30 November).

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 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 flows from the fact that filling a job requires finding the right match with the right 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 in the case where firms do not strategically raise their separations in order to hire new workers at lower cost.²³

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 rate²⁴ 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 for the hiring credit, than it was in medium-size firms not eligible for 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 local labor market conditions. Where it is particularly difficult to recruit, hiring costs should be higher and the hiring credit might not be enough to boost employment.

²³ Assume that each hire induces s separations. If s remains constant, the separation rate, defined as S/L_{-1} increases with H . This is also the case if the separation rate is defined as $2S/(L + L_{-1})$, as in Davis et al. (1996).

²⁴ 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 $t-1$.

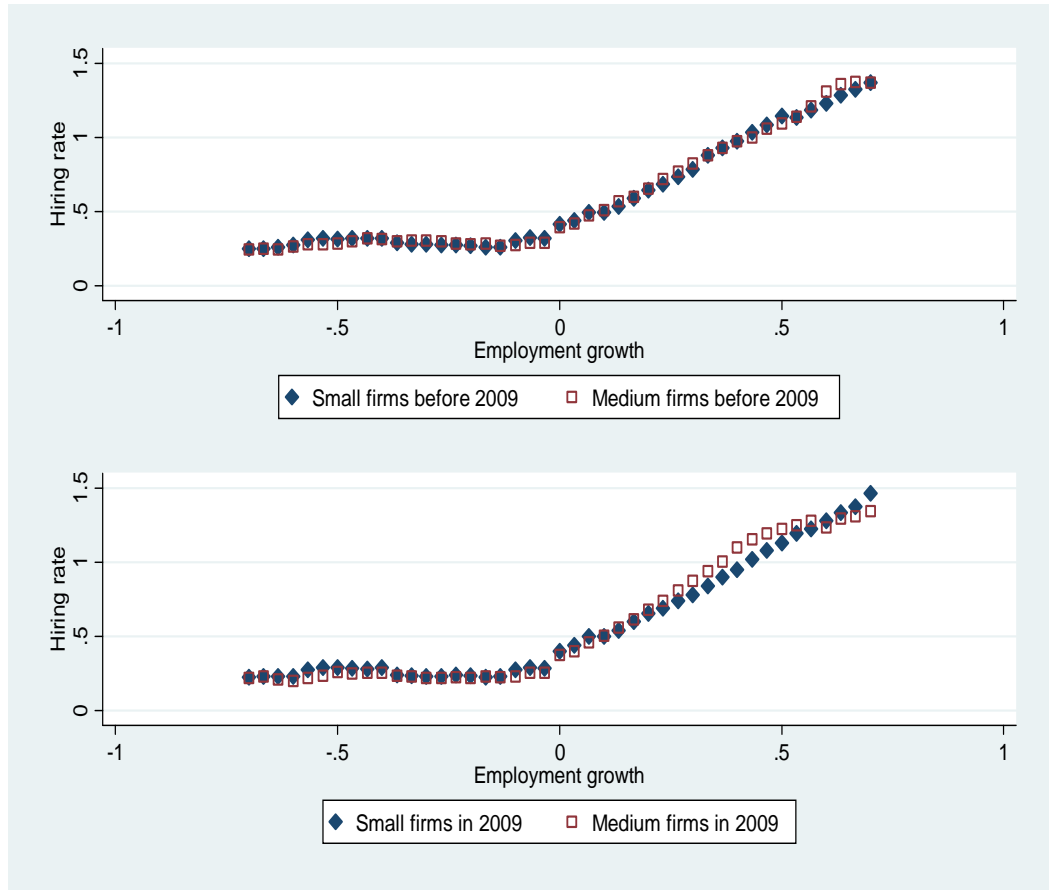


Figure 10: Hiring rate and employment growth rate in small size and medium size firms.

Note: 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 the previous year. Medium size firms have 10-14 full time equivalent employees in the previous year. Source: DADS.

To check for the possibility of heterogeneous employment effects across regions and sectors, we use a survey carried out 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; each year it supplies 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 foresee, 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 this end, we estimate the difference-in-differences model (2) separately for the four quartiles of the hiring difficulties distribution. To control for the potential endogeneity 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 greatest amount of difficulty in hiring, while the effect is very large and significant in the quartile with the least amount of hiring difficulty.²⁵ This suggests that the hiring credit is not effective at creating 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 at boosting employment when the economy slows down.

5 Cost analysis

5.1 Cost per job created and windfalls for firms

Based on our estimates, it is possible to compute the gross cost per job created in the treatment group. The *zéro charges* hiring credit provided 3.6 million euros to the firms of the treatment

²⁵Note 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.

group in November 2009 and created about 5,200 jobs at that date.²⁶ Accordingly, at the end of 2009, the monthly cost of job creation amounts to 700 euros (about 25% of the average cost of a job in the treatment group).²⁷ 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 *zéro charges* on social benefits. To this end we rely on two key assumptions. First, consistent with our estimation of the impact of *zéro charges* on net job creation, we assume that the number of jobs created by *zéro charges* reduced non-employment by the same amount. Second, we assume that social benefits would have been paid to individuals identical to the beneficiaries of *zéro charges* if they had remained on the dole. We find that 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 \tilde{H} denote the number of hires subsidized by *zéro charges* and H_σ the number of hires created by *zéro charges*. The share of hires that have benefited from the hiring credit and that would have been created absent the hiring credit amounts to $(\tilde{H} - H_\sigma)/\tilde{H}$. According to our estimates, 84 percent of the hires subsidized by *zéro charges* would have occurred absent the hiring credit.²⁸ This large share implies that firms have benefited from important windfalls associated with *zéro charges*.

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

Given the large windfalls for firms entailed by *zéro charges*, 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 without this condition, we rely on the model of labor demand with hiring and firing costs presented in appendix A.1. This model allows us to compare the

²⁶As 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 δ when the dependent variable is $\Delta L/L_{-1}$ in equation (2), equals 0.008. Thus, the number of jobs created in the treatment group is $0.008 \times 646,717 = 5,173$.

²⁷700 euros are equal to $3.6 \times 10^6/5,200$.

²⁸There 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.

impact of the two schemes when the hiring credit σ per eligible hire is identical for both schemes. It shows that (see appendix A.3):

1. When the hiring credit is non-conditional on net job creation:
 - (a) In firms where firing costs and hiring costs are sufficiently low, the hiring credit induces employers to lay off incumbent workers in order 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 firms for which growth is high enough to benefit from the 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 lay workers off 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 γ .²⁹ From the knowledge of this elasticity, denoted by ε_γ , 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 ε_σ ,³⁰ we can compute the number of jobs created by each type of credit for an expenditure equal to one percent of the labor cost. We assume that the elasticity of labor demand with respect to the wage, ε , 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 γ over the number of jobs created by the non-conditional hiring credit is equal to³¹

$$\frac{\varepsilon_\gamma}{\varepsilon_\sigma} = \eta \frac{1 + \Gamma(\gamma)}{\Gamma(\gamma) - \gamma}, \quad (4)$$

²⁹see equation (A13) in appendix A.4.

³⁰see equation (A11) in appendix A.2.

³¹see the definitions of the two elasticities equations (A11) and (A13) in appendix A.2 and A.4 respectively.

where $\Gamma(\gamma)$ is the average employment growth rate of firms that grow above the threshold γ absent the subsidy, and η is the average hiring rate of firms with a positive number of hires eligible to the non-conditional hiring credit.

The $\varepsilon_\gamma/\varepsilon_\sigma$ ratio can be smaller or larger than one. It is smaller than one if the non-conditional hiring credit is relatively effective. This comes about if the hiring rate of firms subsidized by the non-conditional hiring credit is small (i.e. η small) and if the conditional credit targets firms that would have had high employment growth absent the hiring credit (i.e. $\Gamma(\gamma) - \gamma$ large). In this situation, the conditional credit creates large windfalls for firms, because it targets firms that would have created many jobs absent the hiring credit.

The $\varepsilon_\gamma/\varepsilon_\sigma$ ratio can either increase or decrease with γ . When γ increases, the conditional credit targets firms that would have created more jobs absent the subsidy, i.e. $\Gamma'(\gamma) > 0$. But the impact of increases in γ on the term $[1 + \Gamma(\gamma)] / [\Gamma(\gamma) - \gamma]$ depends on the shape of the conditional average $\Gamma(\gamma)$.³²

Figure 11 displays the $\varepsilon_\gamma/\varepsilon_\sigma$ ratio computed on the treatment group for various values of the growth threshold γ . The ratio is bigger than one, meaning that the credit conditional on net job creation is more effective at creating jobs than the non-conditional hiring credit. This means that the credit conditional on net job creation would reduce the windfalls for firms. Figure 11 also shows that the credit conditional on net job creation would create a maximum number of jobs when the employment growth rate threshold γ equals -1% . In that case, the credit conditional on net job creation would create 4.4 times more jobs, at given budget, than the non-conditional hiring credit.

These results suggest that it might have been worthwhile to target the credit at net job creation and provide more generous subsidies per job created to fewer firms. However, this conclusion needs further investigation 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.

³²The derivative of $[1 + \Gamma(\gamma)] / [\Gamma(\gamma) - \gamma]$ with respect to γ has the same sign as the expression $1 + \Gamma(\gamma) - (\gamma + 1)\Gamma'(\gamma)$, where $\gamma \geq -1$ and $\Gamma(\gamma) > \gamma$.

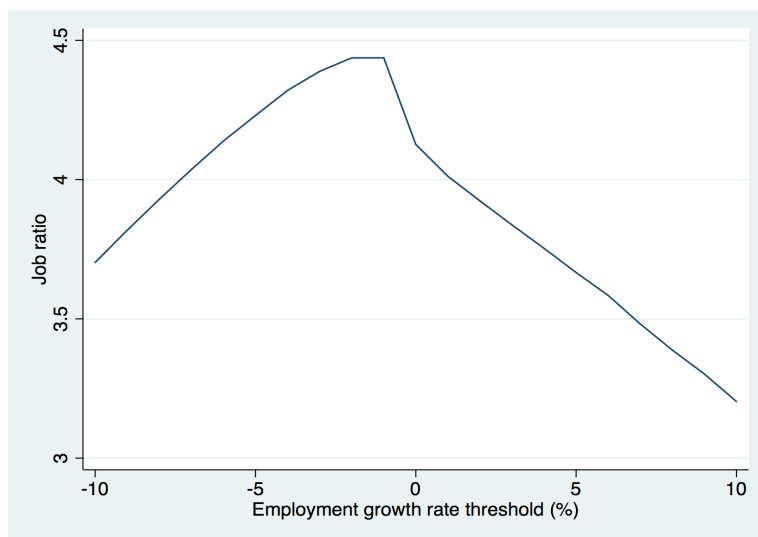


Figure 11: Ratio of the number of jobs created by the credit conditional on net job creation above the employment growth threshold γ over the number of jobs created by the non-conditional hiring credit.

Note: The horizontal axis displays the employment growth rate threshold γ above which firms are eligible for the credit conditional on net job creation.

6 Robustness checks

In this section we perform a 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 and rule out the possibility that our estimates are driven by reversion to the mean. 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, see Figure 4). Table 6 presents the estimates for different bandwidths. The difference-in-differences estimates are higher when the treatment group includes firms with higher take-up rates (column 1 to 3). 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 that have from 6 to 14 full time equivalent em-

employees in the previous year, are lower bounds of the impact of the subsidy. The corresponding estimates of elasticity of employment with respect to the change in labor cost induced by *zéro charges* equal -5.5 and -7.5 when the bandwidth goes from 5 to 15 employees and falls in the range $[5,8]$ - $[13,16]$ employees respectively.³³ All in all, these results suggest that our benchmark estimate of the elasticity of employment with respect to the change in labor cost induced by the hiring credit is conservative: it is likely a lower bound for the elasticity that might be larger than 4 in absolute value.

6.2 Year placebo tests

We perform a series of placebo tests using cohorts from 2006 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 7 shows that employment, hours, hires and separations of the treatment and the control groups did not evolve differently either in 2007 or 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 groups over these 2 years. These results reinforce the relevance of the common trend assumption. They also rule out the possibility that our estimates of the impact of *zéro charges* are driven by reversion to the mean.

6.3 Size threshold placebo tests

A potential concern is that our results may 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 groups is very small compared to the situation studied by Moscarini and Postel-Vinay. Nevertheless, we check that there is no systematic difference in the evolution of employment and hours across firms of different size in

³³As explained in footnote 21 these elasticities imply labor demand elasticity with respect to the wage equal to $-3.5 = -5.5 \times 0.63$ and $-4.7 = -7.5 \times 0.63$ respectively.

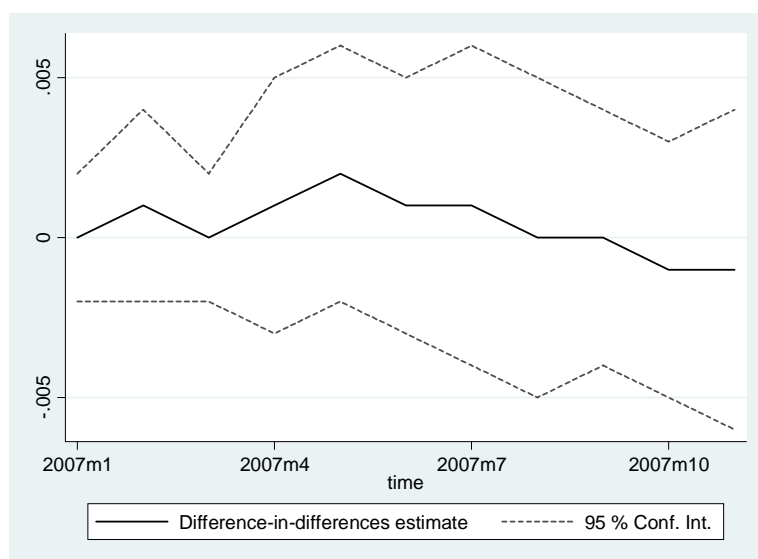


Figure 12: Placebo test as if the policy had been implemented in December 2006.
 Note: Difference-in-differences estimates of the impact of the hiring subsidy if the subsidy had been introduced in December 2006. The outcome is the growth rate of employment $(L_{m,t} - L_{t-1})/L_{t-1}$ where $L_{m,t}$ 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 2006-2007 and covariates presented in table 2.

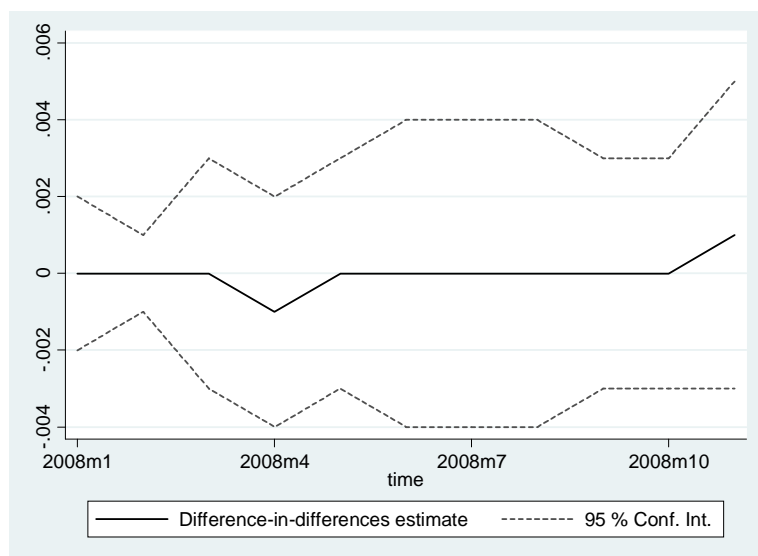


Figure 13: Placebo test as if the policy had been implemented in December 2007.
 Note: Difference-in-differences estimates of the impact of the hiring subsidy if the subsidy had been introduced in December 2007. The outcome is the growth rate of employment $(L_{m,t} - L_{t-1})/L_{t-1}$ where $L_{m,t}$ 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.

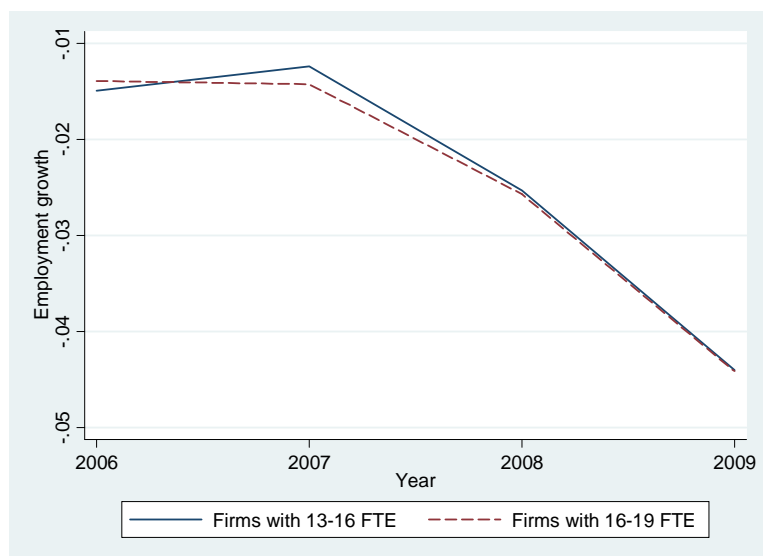


Figure 14: Average employment growth rates in placebo groups.

Note: Growth rate of employment between 30 November of year $t - 1$ and year t . One group comprises firms of size between 13 (included) and 16 (excluded) full time equivalent employees in the previous year (average from 1 December to 30 November). The other group comprises firms of size from 16 (included) to 19 full time equivalent employees in the previous year.

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 size 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.³⁴ 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.³⁵ This indicates that the difference in employment growth across our treatment and control groups does not stem from differences in behavior due to differences in size.

³⁴We 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.

³⁵We do not present these estimates to save space. The results are available upon 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 may have been impacted by the hiring credit. The control group can be affected negatively. The hiring credit could 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 might increase labor market tightness and thus the recruiting costs for all firms. Firms of the control group can also be impacted positively. The rise in production of small firms could increase the demand directed at their suppliers. The lower labor cost might allow small firms to sell their products at a lower price, boosting the production of firms that buy these products. All these mechanisms imply a potential impact of the hiring credit on employment and hours worked of the control group that can be either positive or negative.

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 and in their sector. If there are equilibrium effects that reduce the impact of the hiring credit, we should observe lower growth rates of employment or hours among non-eligible firms in areas with a higher share of subsidized hires. We distinguish 348 employment pools,³⁶ and 5 sectors (manufacturing, construction, retail and transport, hotels and restaurants, and other merchant services) and we restrict our sample to employment pool \times sector units with at least 50 firms present in a given year. We first check that our baseline results remain unchanged if we regress the difference-in-differences equation (2) on the sample made of the employment pool \times sector units obtained this way. To do so, for each unit and each year we compute the average growth rates of employment and hours worked separately for the two groups of firms (treatment or control). We weight each employment pool \times sector unit by its employment size among firms with less than 14 full-time equivalent employees in the previous year. The results are shown in Table 8. They are similar to those in Table 2.

Within each of the 1,512 employment pool \times sector units obtained for 2009, we compute the ratio of subsidized hires in 2009 to all hires observed in 2008 among firms with 0 to 14 full

³⁶We 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.

time equivalent employees in 2008, denoted S_{jk} , where j stands for the employment pool and k for the sector. We also compute for each unit the average growth rate of employment and of hours worked from December 2008 to November 2009 among firms having from 10 to 14 full time equivalent employees in the previous year. The average ratio of subsidized hires in 2009 among firms with 0 to 14 employees, S_{jk} , is 0.210, and its standard deviation is 0.087. We then compare the labor market outcomes across units with different shares of subsidized hires. To achieve this, we estimate the following model :

$$Y_{jk} = \alpha + \beta_1 S_{jk} + \beta_2 S_{j(-k)} + \beta_3 S_{(-j)k} + bX_{jk} + u_{jk} \quad (5)$$

where Y_{jk} stands for the average growth rate of employment or of hours worked in firms belonging to employment pool j and sector k , $S_{j(-k)}$ is the number of subsidized hires in 2009 divided by all hires in 2008 in firms with 0 to 14 employees operating in employment pool j and belonging to sectors other than k ; and $S_{(-j)k}$ is the number of subsidized hires in 2009 divided by all hires in 2008 in firms with 0 to 14 employees operating in employment pools other than j but belonging to sector k . The term u_{jk} is a residual. In this setting S_{jk} and $S_{j(-k)}$ together account for the equilibrium effects that may occur within the employment pool j whatever the sector, while $S_{(-j)k}$ accounts for the equilibrium effects that could arise from interactions with firms in the same sector as the unit under consideration but outside the employment pool j . We also include a number of unit-specific controls X_{jk} , such as the distribution of firm age, the composition of the workforce, as well as the growth rate of employment, and the hiring rates in the employment pool in 2008. The aim is to achieve a better control for the specificities of labor market dynamics in each pool. To better account for the labor market situation, the set of control variables X_{jk} also includes the change in the survival rate of firms within the unit between 2008 and 2009, as well as the employment growth rate in 2009 observed in the same sector as the unit but in employment zones located nearby. If the sum of coefficients β is significantly different from zero, this indicates the presence of equilibrium effects.

Now, the number of subsidized hires in 2009 might be affected by unobserved shocks that also affect employment and hours of the control group, meaning that the ratios S of subsidized hires in 2009 are potentially endogenous in equation (5). For this reason, in each employment pool \times sector unit, the ratios S of subsidized hires in 2009 are instrumented by the corresponding shares of eligible hires in 2008 among all hires the same year (when the subsidy was not yet implemented). This amounts to substituting the number of subsidized hires in 2009 at the

numerator of S by the number of eligible hires in 2008 in firms with less than 10 employees. The resulting instruments are denoted Z .

Table 9 presents the result for the OLS estimation of equation (5). 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, when estimations include the set of controls. Tables 10 and 11 show the estimates when the share of subsidized hires in 2009 is instrumented by the share of eligible hires in 2008, both for the growth rate of employment and that of hours. Table 10 presents the first step of the estimations. It shows that the shares of subsidized hires in 2009 are strongly correlated with the instruments Z . Table 11 shows the second step of the estimations. No significant equilibrium effects are detected, even when the full set of controls is included in the regression. For the specification using controls, the Wu-Hausman endogeneity test suggests that the ratio of subsidized hires in 2009 is not a valid instrument, either 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. These results remain unchanged if we enlarge our sample to all employment pool \times sector units with at least 30, or if we restrict it to units with at least 100 firms in 2009 (instead of 50).

7 Conclusion

This paper shows that a hiring credit targeted at small firms and low wage workers did have 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, 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 had a temporary impact on employment, that disappeared at least in part when the hiring credit was 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 at low wages, which are very rigid in France, because the minimum wage is very high and almost all workers are covered by sectoral collective agreements that are binding on 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 credits not targeted at low wages, could have a much weaker employment impact.

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Table 1: The characteristics of eligible/ineligible and treated/untreated firms in 2008

	Eligible	Ineligible	Diff test	Treated	Untreated	Diff test
Nb employees in 2008	6-10	10-14	p-value	6-10	6-10	p-value
Manufacturing	.159	.195	.0000	.138	.166	.0000
Construction	.184	.185	.6620	.191	.182	.0460
Retail and transport	.308	.294	.0000	.325	.302	.0000
Hotels and restaurants	.097	.087	.0000	.148	.081	.0000
Merchant services	.252	.239	.0000	.199	.269	.0000
Parisian area	.238	.232	.0360	.153	.265	.0000
North-West	.243	.254	.0000	.261	.238	.0000
North-East	.121	.125	.0600	.129	.118	.0000
South-East	.268	.261	.0260	.307	.255	.0000
South-West	.130	.128	.2700	.150	.124	.0000
Sales below 2 millions euros	.473	.218	.0000	.534	.453	.0000
Young firm (age below 5 years)	.133	.100	.0000	.131	.134	.4820
Mean share of...						
... male managers	.207	.218	.0000	.161	.222	.0000
... female managers	.120	.116	.0000	.101	.126	.0000
... male white-collar	.080	.074	.0020	.096	.075	.0000
... female white-collar	.209	.184	.0000	.254	.195	.0000
... male blue-collar	.346	.365	.0000	.351	.344	.0140
... female blue-collar	.037	.043	.0000	.036	.037	.5350
Mean share of ...						
...low-wage workers	.610	.594	.0000	.709	.593	.0000
...part-time workers	.263	.214	.0000	.255	.239	.0000
Nb. of obs.	70,998	30,912	-	17,017	53,981	-

Source : DADS (Insee). Note : Low-wage workers earn between the minimum wage and 1.6 times this amount (on an hourly basis). Part-time workers work below 80 percent of normal working hours. The number of employees corresponds to the full time equivalent in 2008 (average from 1 January to 30 November). The number of observations corresponds to the number of firms.

Table 2: Difference-in-differences estimates

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.010*** (.002)	.008*** (.002)	.009*** (.002)
Hours growth	.010*** (.002)	.009*** (.002)	.008*** (.002)
Hiring rate	.014*** (.005)	.012*** (.004)	.019*** (.005)
Separation rate	.005 (.005)	.004 (.004)	.010* (.005)
Survival rate	.000 (.001)	.000 (.001)	.000 (.001)
Nb. Observations	405,376	405,376	206,854

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) and different specifications (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in the 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 the 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 ineligible jobs

	Eligible jobs	Ineligible jobs	All jobs
Employment growth	.010 ^{***} (.003)	.002 (.004)	.008 ^{***} (.002)
Hours growth	.012 ^{***} (.003)	.005 (.004)	.008 ^{***} (.002)
Hiring rate	.011 ^{***} (.004)	.005 (.008)	.008 ^{**} (.004)
Separation rate	.001 (.004)	.003 (.008)	.000 (.004)
Nb. Observations	349,996	349,996	349,996

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) and different types of jobs (columns): eligible jobs below 1.6 times the minimum wage that last at least one month; ineligible 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 the 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 the 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

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
All wages	.000 (.001)	-.001 (.001)	.000 (.002)
Low wage incumbents	.000 (.001)	-.001 (.001)	.000 (.001)
Eligible entrants	.000 (.002)	.000 (.002)	-.001 (.002)
Nb. Observations	210, 553	210, 553	105, 277

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) and different specifications (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in the 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 the 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 from 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

Hiring difficulties	Quartile 1		Quartile 2		Quartile 3		Quartile 4		All	
	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Emp growth	.015*** (.004)	.015*** (.004)	.014*** (.004)	.014*** (.004)	.009** (.004)	.008** (.004)	.004 (.003)	.002 (.002)	.010*** (.002)	.009*** (.002)
Hours growth	.015*** (.004)	.015*** (.004)	.013*** (.004)	.013*** (.004)	.008** (.004)	.007** (.004)	.006** (.003)	.004 (.003)	.010*** (.002)	.009*** (.002)
Nb. Observations	73, 199		82, 752		92, 073		115, 179		363, 203	

Source : DADS (Insee) and BMO (Pôle Emploi). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) 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 the 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 the 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 varying the bandwidth.

Size bandwidth	7-13	6-14	5-15	[5,8]-[13,16]
Employment growth	.005*** (.002)	.008*** (.002)	.011*** (.002)	.015*** (.002)
Hours growth	.006*** (.002)	.009*** (.002)	.012*** (.002)	.016*** (.002)
Hiring rate	.012** (.005)	.012*** (.004)	.015*** (.004)	.015*** (.005)
Separation rate	.007 (.005)	.004 (.004)	.003 (.004)	.000 (.005)
Survival rate	.000 (.001)	.000 (.001)	.000 (.001)	.000 (.001)
Nb. Observations	283, 737	405, 376	549, 022	363, 101

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 7: Difference-in-differences estimates for all firms, with placebo years

Placebo	December 2006	December 2007
Cohorts	2006-2007	2006-2008
Covariates	Yes	Yes
Employment growth	-.001 (.002)	.001 (.002)
Hours growth	-.001 (.003)	.001 (.002)
Hiring rate	.001 (.003)	-.004 (.003)
Separation rate	.002 (.003)	-.005* (.003)
Survival rate	.000 (.001)	.001 (.001)
Nb. Observations	178, 603	270, 593

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) 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 the 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 the 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-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 8: Difference-in-differences estimates based on employment pool x sector units

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.008*** (.002)	.007*** (.002)	.008*** (.003)
Hours growth	.008*** (.002)	.008*** (.002)	.008*** (.002)
Nb. Observations	12, 262	12, 262	6, 177

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) and different specifications (columns) based on averaged labor market outcomes for 5 different sectors in 348 employments pools. Only employment pool x sector units with at least 50 firms are retained in the sample. Within each employment pool x sector unit the treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in the 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 the 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. As covariates, we include year dummies, sector dummies, region dummies and their interactions. We also include dummies for distribution of firms' age, the share of 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), lagged employment growth, lagged hiring rate and lagged separation rate. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

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

Independent variable	OLS			
	Employment growth		Hours growth	
Covariates	No	Yes	No	Yes
S_{jk}	.049* (.029)	.040 (.029)	.044 (.029)	.042 (.030)
$S_{j(-k)}$	-.021 (.030)	.013 (.031)	-.056* (.029)	-.001 (.030)
$S_{(-j)k}$.053 (.046)	-.014 (.089)	.006 (.048)	.067 (.089)
Test $S_{jk} + S_{j(-k)} + S_{(-j)k} = 0$ (p-value)	.0769	.6934	.9072	.2574
R^2	.365	.388	.427	.444
Nb. Observations	1, 512	1, 512	1, 512	1, 512

Source : DADS (Insee). Note: The dependent variable is either the growth rate of employment or that of hours from 1 December 2008 to 30 November 2009 in each employment pool x sector unit, among firms with 10 to 14 full-time equivalent employees in the previous year. The independent variables are the ratios of subsidized hires, which correspond to three variables: (1) the number of subsidized hires in 2009 divided by the number of hires in the employment pool x sector unit in 2008, among firms with 0 to 14 full-time equivalent employees in the previous year. (2) the same ratio but measured among firms belonging to the same employment pool and to other sectors than the one considered for the dependent variable. (3) the same ratio but measured among firms belonging to the same sector and to other employment pools than the one considered for the dependent variable. Only employment pool x sector units with at least 50 firms are retained in the sample. As covariates, we include sector dummies, region dummies and their interactions, dummies for distribution of firms' age, the share of female or male workers with different occupations (managers, white-collar or blue-collar workers), lagged employment growth and lagged hiring rate rates, the change in the survival rate of firms within the unit between 2008 and 2009, as well as the employment growth rate in 2009 in the same sector as the unit but in employment zones located nearby. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 10: First stage of the instrumental variable estimates.

IV 2SLS - First stage		
Covariates	No	Yes
	S_{jk}	
Z_{jk}	.412*** (.027)	.375*** (.026)
$Z_{j(-k)}$.466*** (.039)	.380*** (.032)
$Z_{(-j)k}$.287*** (.029)	.132*** (.049)
Test $Z_{jk} = Z_{j(-k)} = Z_{(-j)k} = 0$ (p-value)	.0000	.0000
	$S_{j(-k)}$	
Z_{jk}	.227*** (.052)	.153*** (.026)
$Z_{j(-k)}$.684*** (.030)	.632*** (.028)
$Z_{(-j)k}$	-.089** (.038)	-.221*** (.060)
Test $Z_{jk} = Z_{j(-k)} = Z_{(-j)k} = 0$ (p-value)	.0000	.0000
	$S_{(-j)k}$	
Z_{jk}	.006* (.004)	.014*** (.004)
$Z_{j(-k)}$	-.003 (.009)	-.027*** (.009)
$Z_{(-j)k}$.618*** (.005)	.567*** (.007)
Test $Z_{jk} = Z_{j(-k)} = Z_{(-j)k} = 0$ (p-value)	.0000	.0000
Nb. Observations	1,512	1,512

Source : DADS (Insee). Note: The dependent variables are the ratios of subsidized hires, which correspond to three variables: First panel: the number of subsidized hires in 2009 divided by the number of hires in the employment pool x sector unit in 2008, among firms with 0 to 14 full-time equivalent employees in the previous year. Second panel: the same ratio but measured among firms belonging to the same employment pool and to other sectors than the one considered for the dependent variable. Third Panel: the same ratio but measured among firms belonging to the same sector and to other employment pools than the one considered for the dependent variable. The independent variables are the instruments used in the second stage, i.e. the corresponding shares of eligible hires in 2008 (i.e. the ratios of the eligible hires in 2008 to total hires in 2008, among firms with 0 to 14 full-time employees in the previous year) Only employment pool x sector units with at least 50 firms are retained in the sample. As covariates, we include sector dummies, region dummies and their interactions, dummies for distribution of firms' age, the share of female or male workers with different occupations (managers, white-collar or blue-collar workers), lagged employment growth and lagged hiring rate rates, the change in the survival rate of firms within the unit between 2008 and 2009, as well as the employment growth rate in 2009 in the same sector as the unit but in employment zones located nearby. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 11: Instrumental variables estimates of equilibrium effects on the growth rates of employment and hours among firms with 10-14 employees.

IV 2SLS - Second stage				
Independent variable	Employment growth		Hours growth	
Covariates	No	Yes	No	Yes
S_{jk}	.055 (.097)	.023 (.096)	-.033 (.098)	.003 (.100)
$S_{j(-k)}$.006 (.100)	.061 (.092)	.042 (.099)	.068 (.097)
$S_{(-j)k}$.043 (.095)	-.006 (.132)	.068 (.098)	.084 (.1352)
Endogeneity test (Wu-Hausman) (p-value)	.4080	.0556	.1435	.0072
Test $S_{jk} + S_{j(-k)} + S_{(-j)k} = 0$ (p-value)	.2903	.5950	.4343	.2956
Nb. Observations	1, 512	1, 512	1, 512	1, 512

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 x sector unit, among firms with 10 to 14 full-time equivalent employees in the previous year. The independent variables are the ratios of subsidized hires, which correspond to three variables: (1) the number of subsidized hires in 2009 divided by the number of hires in the employment pool x sector unit in 2008, among firms with 0 to 14 full-time equivalent employees in the previous year. (2) the same ratio but measured among firms belonging to the same employment pool and to other sectors than the one considered for the dependent variable. (3) the same ratio but measured among firms belonging to the same sector and to other employment pools than the one considered for the dependent variable. These ratios are instrumented by the corresponding shares of eligible hires in 2008, i.e. the ratios of the eligible hires in 2008 to total hires in 2008, among firms with 0 to 14 full-time employees in the previous year. Only employment pool x sector units with at least 50 firms are retained in the sample. As covariates, we include sector dummies, region dummies and their interactions, dummies for distribution of firms' age, the share of female or male workers with different occupations (managers, white-collar or blue-collar workers), lagged employment growth and lagged hiring rate rates, the change in the survival rate of firms within the unit between 2008 and 2009, as well as the employment growth rate in 2009 in the same sector as the unit but in employment zones located nearby. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

A Appendix

A.1 The model with hiring and firing costs

We consider a discrete time partial equilibrium model of a firm that produces 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 revenue 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 $c_H(H)$, which satisfies $c_H(0) = c'_H(0) = 0$, $c'_H(H) \geq 0$, $c''_H(H) > 0$, where $H \geq 0$ stands for the number of hires. Similarly, firing costs are equal to $c_F(F)$, which satisfies $c_F(0) = c'_F(0) = 0$, $c'_F(F) \geq 0$, $c''_F(F) > 0$ where F denotes the number of firings. An exogenous proportion q of workers quit the firm at the end of 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 β denotes the discount factor and \mathbb{E} the expectation operator. The value function of the firm satisfies

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

subject to the law of motion of employment:

$$L = (1 - q)L_{-1} + H - F \tag{A1}$$

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 σ . We analyze the impact of the hiring credit in the neighborhood of $\sigma = 0$. In the period in which the hiring credit is implemented, the expression for the profit is

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

Profit maximization with respect to H and F yields the first order conditions

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

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

where λ_H , λ_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. From the first order conditions, with $\lambda_H = \lambda_F = \bar{\lambda}_F = 0$, we get:

$$c'_F(F) + c'_H(H) = \sigma. \tag{A4}$$

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

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

$$L = (1 - q)L_{-1} + H - F \tag{A6}$$

- **Case 2:** $H > 0$, $F = 0$. In this case, there are no firings and $\lambda_F > 0$. Thus, the first order conditions, together with $\lambda_H = 0$, imply

$$R_L(A, L) = w - \sigma + c'_H(H) - \beta \mathbb{E}V_L(A', w', L) \quad (\text{A7})$$

$$L = (1 - q)L_{-1} + H \quad (\text{A8})$$

- **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 the number of hires (and employment) is defined by

$$R_L(A, H) = w - \sigma + c'_H(H) - \beta \mathbb{E}V_L(A', w', H) \quad (\text{A9})$$

The amount of subsidy paid to the firm is maximum and equal to σL .

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

$$R_L(A, L) = w - c'_F(F) - \beta \mathbb{E}V_L(A', w', L)$$

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

- **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 (such as *zero charges*).

A.2.1 Changes in wage

Let us study the impact of a temporary 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 obtain, 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 $l_F(A, w)$. Note 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

$$\frac{d\mathcal{L}}{dw} = \int_0^{\bar{A}} \frac{\partial l_F(A, w)}{\partial w} dG(A) + \int_{\bar{A}}^{\infty} \frac{\partial l_H(A, w)}{\partial w} dG(A)$$

or

$$\frac{wd\mathcal{L}}{dw} = \int_0^{\bar{A}} l_F(A, w)\varepsilon_F(A, w)dG(A) + \int_{\bar{A}}^{\infty} l_H(A, w)\varepsilon_H(A, w)dG(A)$$

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

$$\begin{aligned}\bar{\varepsilon}_F &= \frac{1}{\int_0^{\bar{A}} l_F(A, w)dG(A)} \int_0^{\bar{A}} l_F(A, w)\varepsilon_F(A, w)dG(A) \\ \bar{\varepsilon}_H &= \frac{1}{\int_{\bar{A}}^{\infty} l_H(A, w)dG(A)} \int_{\bar{A}}^{\infty} l_H(A, w)\varepsilon_H(A, w)dG(A)\end{aligned}$$

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 :

$$\varepsilon_w \equiv \frac{wd\mathcal{L}}{\mathcal{L}dw} = (1 - \pi)\bar{\varepsilon}_F + \pi\bar{\varepsilon}_H \quad (\text{A10})$$

where π 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 temporary 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 *zéro charges* hiring credit did not induce firms to fire workers so as to replace them by less costly entrants, as shown in section 4.3, 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 ρ the take-up rate of the hiring credit among eligible firms with a positive number of hires.³⁷ Equations (A7) and (A8) imply that total employment can be written

$$\mathcal{L} = \int_0^{\bar{A}} l_F(A, w)dG(A) + (1 - \rho) \int_{\bar{A}}^{\infty} l_H(A, w)dG(A) + \rho \int_{\bar{A}}^{\infty} l_H(A, w - \sigma)dG(A)$$

where \bar{A} satisfies, as previously, $l_F(\bar{A}, w) = l_H(\bar{A}, w)$.

We look for the impact of a change in the average cost of labor among all firms, which we denote by $\bar{\psi}$. By definition, we have

$$\bar{\psi} = w - \frac{\sigma\rho}{\mathcal{L}} \int_{\bar{A}}^{\infty} h(A, w - \sigma)dG(A)$$

³⁷If all hires in firms that benefit from the credit are not subsidized, we interpret σ as the average subsidy per hire.

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

$$\frac{d\bar{\psi}}{d\sigma} = -\frac{\rho}{\mathcal{L}} \int_{\bar{A}}^{\infty} h(A, w) dG(A)$$

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

$$\frac{d\mathcal{L}}{d\sigma} \frac{d\sigma}{d\bar{\psi}} = \frac{\mathcal{L}}{\int_{\bar{A}}^{\infty} h(A, w) dG(A)} \int_{\bar{A}}^{\infty} \frac{\partial l_H(A, w)}{\partial w} dG(A)$$

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

$$\varepsilon_{\sigma} \equiv \frac{\bar{\psi}}{\mathcal{L}} \frac{d\mathcal{L}}{d\sigma} \frac{d\sigma}{d\bar{\psi}} = \frac{\bar{\varepsilon}_H}{\eta} \quad (\text{A11})$$

where $\eta = \int_{\bar{A}}^{\infty} h(A, w) dG(A) / \int_{\bar{A}}^{\infty} l_H(A, w) dG(A)$ stands for the hiring rate of firms with a positive number of hires eligible to the hiring credit. From equations (A10) and (A11) we get the relation between the elasticities ε_{σ} and ε_w :

$$\varepsilon_w = \varepsilon_{\sigma} + (1 - \pi)\bar{\varepsilon}_F + \bar{\varepsilon}_H \left(\pi - \frac{1}{\eta} \right)$$

This relation takes a simple form if we assume that ε 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. Figure 7, which displays the adjustment of employment over the year 2009, suggests that this is a relevant approximation for *zéro charges*. If ε is identical in all firms we get

$$\varepsilon = \eta \varepsilon_{\sigma}$$

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 lay off incumbent workers so as 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 fall into case 1b above, and which benefit from the conditional hiring credit (i.e. for which $(L - L_{-1}) / L_{-1} > \gamma$, where γ denotes the employment growth threshold above which firms become eligible for 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 lay some workers off in order to replace them by entrants whose cost is lower because they benefit from the hiring credit,

which corresponds to case 1. We describe the frontier between cases 1 and 2 in the firing costs and hiring costs plane. More precisely, we assume that the parameter values pin down the model in case 1 of appendix A.1. We consider an increase in the hiring and firing costs, and derive the conditions when firms cease to fire any workers (and switch to case 2). We follow a perturbation method and assume that there exist Δ_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 (Δ_H, Δ_F) . They verify:

$$\begin{aligned} R_L(A, L(\Delta_H, \Delta_F)) &= w - \sigma + (1 + \Delta_H)c'_H(H(\Delta_H, \Delta_F)) - \beta\mathbb{E}V_L(A', w', L(\Delta_H, \Delta_F)) \\ \sigma &= (1 + \Delta_H)c'_H(H(\Delta_H, \Delta_F)) + (1 + \Delta_F)c'_F(F(\Delta_H, \Delta_F)) \\ L(\Delta_H, \Delta_F) &= (1 - q)L_{-1} + H(\Delta_H, \Delta_F) - F(\Delta_H, \Delta_F) \end{aligned}$$

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:

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

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

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

which yields in steady state:

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

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

$$dF = \frac{c'_H B}{c''_H c''_F - B(c''_H + c''_F)} \Delta_H - \frac{c'_F (c''_H - B)}{c''_H c''_F - B(c''_H + c''_F)} \Delta_F \quad (\text{A12})$$

The iso-curve $F = cste$ in the (Δ_F, Δ_H) plane is such that $dF = 0$. From the two previous equations we get

$$\frac{\Delta_F}{\Delta_H} = \frac{c'_H B}{c'_F (c''_H - B)} < 0$$

This implies that the slope of the frontier between cases 1 and 2 in the (Δ_F, Δ_H) plane (i.e. $F = 0$ and $H > 0$) is negative. Equation (A12) shows that F decreases with Δ_H in case 1. Therefore, case 1, where $F > 0$, lies below the frontier $F = 0$; and case 2, where $F = 0$, lies above the frontier as shown on figure 15. This proves 1a and 1b in section 5.2.

Let us now show claim 2. Assume that the temporary hiring credit is conditional on net job creation for all jobs created above the threshold employment growth rate γ . We analyze the impact of a small temporary change in σ in the neighborhood of $\sigma = 0$ so that the value function remains

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

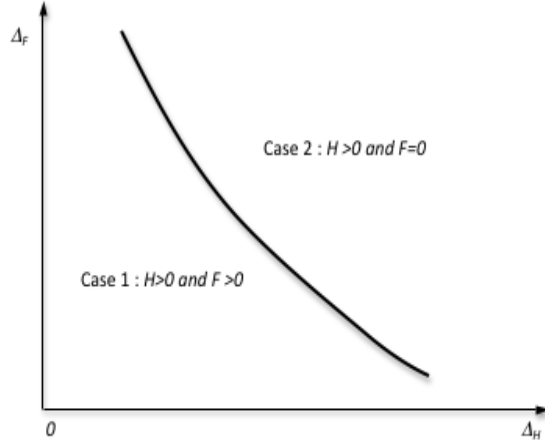


Figure 15: Cases 1 and 2 of appendix A.1 in the firing costs and hiring costs plane

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

The current profit in the period in which the conditional hiring credit is implemented is

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

Assume that $L > (1 + \gamma)L_{-1}$, the first order conditions with respect to H and F are

$$\begin{aligned} R_L(A, L) - w + \sigma - c'_H(H) + \beta \mathbb{E}V_L(A', w', L) + \lambda_H &= 0 \\ -R_L(A, L) + w - c'_F(F) - \beta \mathbb{E}V_L(A', w', L) + \lambda_F - \bar{\lambda}_F &= 0 \end{aligned}$$

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 above, 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 γ . 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 for both types of credit, we can use equations (A7) and (A8) to define total employment as

$$\mathcal{L} = \int_0^{\bar{A}} l_F(A, w) dG(A) + \int_{\bar{A}}^{\bar{A}} l_H(A, w) dG(A) + (1 - \rho) \int_{\bar{A}}^{\infty} l_H(A, w) dG(A) + \rho \int_{\bar{A}}^{\infty} l_H(A, w - \sigma) dG(A)$$

where \bar{A} satisfies $l_F(\bar{A}, w) = l_H(\bar{A}, w)$; \tilde{A} satisfies $l_H(\tilde{A}, w) = (1 + \gamma)L_{-1}$ and ρ is the share of eligible firms that benefit from the conditional hiring credit. For the sake of simplicity, we assume that the threshold γ is sufficiently high to yield a benefit only to firms with a positive number of hires during the period. It can easily be checked that formula (A13), which we wish to prove, holds true when the threshold γ is so small that it can also yield a benefit to firms that dismiss workers during the period (i.e. for which labor demand is determined by $l_F(A, w)$).

The average labor cost per employee is

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

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

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

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

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

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 credit conditional on net job creation above the employment growth rate threshold γ as

$$\varepsilon_{\gamma} \equiv \frac{\tilde{\psi}}{\mathcal{L}} \frac{d\mathcal{L}}{d\sigma} \frac{d\sigma}{d\tilde{\psi}} = \frac{1 + \Gamma(\gamma)}{\Gamma(\gamma) - \gamma} \tilde{\varepsilon}_H$$

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

$$\varepsilon_{\gamma} = \frac{1 + \Gamma(\gamma)}{\Gamma(\gamma) - \gamma} \varepsilon \tag{A13}$$

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 assessments of recruitment difficulties in 388 local employment pools, and 24 industries providing 8,622 estimates each year (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 up 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 makes it possible 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 1 January and 30 June 2009, out of 270,755 beneficiaries recorded during that period. The survey collected the gender, age, and education of the recruitees, 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 or temporary), 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 recruitees) and those 26 years old or more. We use this information to estimate the savings on social benefits induced by the jobs created by *zéro charges*. To do so we compute the social benefits that would have been received by the beneficiaries if they had remained on the dole.

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 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 person 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 for the minimum income of 450 euros per month, which inactive people are as well, for which studies show a typical take-up rate of 2/3.³⁸ This provides an average cost of 300 euros per month for the unregistered unemployed and the other inactive individuals, but only for those 26 years old or older, since younger unemployed / inactive people are not eligible for this minimum income scheme. Students may be eligible for scholarships, but these are rather rare. The main benefit for students is one of the three main housing benefits schemes, the average amount of which 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 for the insurance benefit, which was 975 euros on average in 2009. Since only about half of the unemployed are eligible for 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, 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 have just ended an employment period, they would probably have registered 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

³⁸See http://www.social-sante.gouv.fr/IMG/pdf/1_Le_non-recours_au_rSa_et_ses_motifs.pdf

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 the observed hiring wages. All in all, each job created by *zéro charges* generates monthly net savings of 695 euros. This estimate excludes the cost of social in-kind services (such as counselling, case-management and health services) typically more important for unemployed and inactive persons than for those in employment. It also takes into account only the basic amount of the minimum income, excluding all supplements for couples and children.

A.7 Supplementary Tables

Table 12: Ratio of the number of difficult recruitments to the total number of planned recruitments by year

	2006	2007	2008	2009
Average ratio	.427 (.121)	.436 (.119)	.532 (.079)	.459 (.093)
Min	.136	.128	.304	.161
Max	.851	.799	.817	.916
Nb. Observations	363	363	363	363

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

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

	Employed	Registered unemployed	Unregistered unemployed	Training	Education	Other Inactive
Less than 26 years old	29%	36%	5%	5%	18%	7%
26 years old of more	42%	39%	5%	4%	4%	8%

Source :Pole Emploi.

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

	Number of firms		Number of employees (in 2008)	
	below 10	above 10	below 10	above 10
Number of employees (firm level) all	832,910	146,811	3,892,725	11,381,920
+ excluding temp. help agencies, associations & agriculture	654,047	123,177	2,882,882	9,364,554
+ trimming extreme values	647,230	120,075	2,793,922	9,285,739
+ keeping 6-10 and 10-14 employees only	71,391	31,163	649,825	433,702
+ excluding missing control variables	70,998	30,912	646,717	430,109

Source : DADS (Insee). Note: The number of employees is the average number of employees per firm in 2008 (average of monthly full time equivalents between 1 January and 30 November 2008).

Table 15: Difference-in-differences estimates for surviving firms

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.009*** (.002)	.008*** (.002)	.009*** (.002)
Hours growth	.010*** (.002)	.009*** (.002)	.009*** (.002)
Hiring rate	.014*** (.005)	.012*** (.004)	.019*** (.005)
Separation rate	.005 (.005)	.004 (.004)	.010* (.005)
Nb. Observations	399, 412	399, 412	203, 889

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) 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 the 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 the 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

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.009*** (.002)	.008*** (.002)	.008*** (.002)
Hours growth	.010*** (.002)	.009*** (.002)	.008*** (.002)
Hiring rate	.014*** (.005)	.0121*** (.004)	.018*** (.005)
Separation rate	.004 (.005)	.003 (.004)	.010* (.005)
Survival rate	.000 (.001)	.000 (.001)	.000 (.001)
Nb. Observations	405, 376	405, 376	206, 845

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) 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 the 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 the 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.