Do Hiring Subsidies Work in Recessions ? Evidence from France [*]

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Abstract

This paper evaluates a French program of hiring subsidies targeted to firms with less than 10 employees and to workers paid below 1.6 times the minimum wage. Using an almost perfect natural experiment and rich administrative data covering all French firms, we find that the program had a strong and fast impact on employment, corresponding to a wage elasticity of labor demand around -2. Although the subsidy was not conditional on net job creation, it did not induce firms to substitute new workers for incumbent workers in order to benefit from the subsidy. Nevertheless, we find that subsidies conditional on net job creation above the employment growth rate threshold of *minus* 4 percent would have maximized job creation at constant budget, creating 40 percent more jobs than the non conditional subsidy, if the take-up rate had remained identical for both types of subsidy.

Keywords: **JEL** : C31, C93, J6

1 Introduction

Hiring subsidies have been used in the US and in a number of European countries to counteract the employment effects of the 2008-2009 recession. New hires sometimes benefited at this occasion from very significant tax reliefs at a high cost for the public purse. These reductions in non-wage costs came in a variety of formats and conditionalities. In some countries (e.g. Portugal, Ireland) tax reliefs were targeted on the long-term unemployed or other disadvantaged groups and required firms to have net employment growth over the relief period.[1] In other countries (e.g. France and Spain), the scope of tax reliefs was larger and no such condition on net hires was required.[2]

Although widely used to foster employment during recessions, hiring subsidies are highly controversial. Becker (2010), reacting to the Hiring Incentives to Restore Employment (HIRE) Act enacted in 2010 in the US, argued that it will increase churning and wages with very little employment effects. Posner (2010) states that disregarding the abundant possibilities of gaming the program, stressed by Becker, the proposal is unlikely to be effective because it violates the economic principles that ought to guide stimulus programs. The main argument of Posner is that a stimulus is designed to stimulate demand, not supply, because the economic problem for which a stimulus program is a solution in a recession is insufficient demand. This reasoning is in line with new-keynesian macroeconomists who claim that employment subsidies are ineffective during recessions because low employment is the consequence of an insufficient aggregate demand (Gali, 2013). However, macroeconomists relying on search and matching models achieve different conclusions. They claim that hiring subsidies are effective to foster job creation during recessions. The reason is that during recessions the cost per hire falls because the labor market tightness is lower, and the opportunity cost of not having an unemployed worker hired rises because the duration of unemployment rises (Jung and Kuester, 2013). All in all, there is not even a start of consensus among economists about the effectiveness of hiring subsidies in recessions.

The lack of empirical evaluations of hiring subsidies contributes to these diverging views. Neumark (2013), in a survey of the literature on the effect of hiring subsidies concludes that they may have significant effects on employment when they are not targeted on specific demographic groups. However, as stressed by Neumark, the evidence is rather scarce.

Our paper contributes to filling this gap. We evaluate the impact of a hiring subsidy, called *zéro charges,* implemented in France from December 2008 to December 2009. The hiring subsidy is targeted to firms that had less than 10 full time equivalent employees before the introduction of the subsidy. This policy has two important advantages for our evaluation purpose. First, it was a real surprise: it has been announced and implemented the same day, on 4 December 2008, and kept secret before the announcement. Second, since for fiscal reasons only firms with less than 10 employees before the announcement of the measure were eligible, the hiring subsidy was arbitrarily restricted to a subset of firms comparable to other firms that were not eligible. These two features make the implementation of *zéro charges* an almost perfect natural experiment

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

[2] In Spain, a 1500 euro per year social contribution rebate applies for two years to new hires of workers with family responsibilities on permanent contracts. See OECD (2010) for a detailed presentation of subsidy measures in 2009.

which allows us to evaluate (for the first time, to the best of our knowledge) the consequences of a non categorical temporary hiring subsidy at the firm level with a proper identification strategy. We use a comprehensive database on all firms followed since 2005. We match this database with the administrative records of the policy which identify each single firm which benefited from the hiring subsidy. This extremely detailed database provides information about employment, hours, hires, separations and wages day by day.

Using a difference-in-differences strategy, we compare the evolution of small and medium firms between 6 and 10 employees and between 10 and 14 employees from November 2008, just before the introduction of the hiring subsidy, to November 2009. The employment growth of small firms significantly increased relative to that of medium firms. Consequently, in 2009, while the economy plunged into the recession, the employment growth of small firms in 2009 decreased less than that of medium firms. The estimated elasticity of employment with respect to the drop in labor cost induced by the subsidy is about -2, a high value which can be explained by the fact that the measure is targeted to low wage workers in a context where there is a high minimum wage. The impact of the hiring subsidy emerged quickly: hires and employment began to rise 3 months after the introduction of the subsidy. The evolution of hours worked is similar to that of employment, meaning that firms did not substitute hours of new workers benefiting from the subsidy for that of incumbent employees. We find an increase in separation rates in small firms associated with the hiring subsidy. However, we show that this phenomenon does not arise because firms fire some employees and replace them with workers for whom they can collect the subsidy. The increase in separation rates is merely a by-product of the positive effect of the subsidy on employment: as shown by Abowd et al. (1999), in France each job created in a given year is associated with 3 hires and 2 separations.

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

Our contribution also sheds light on the design of hiring subsidies. The widespread concern that the principal effect of hiring subsidies may be to raise the excess worker turnover without significant impact on employment induces many debates on the appropriate design of hiring subsidy programs. However, the lack of reliable information leads to convoluted policy decisions with very uncertain consequences. For instance, in the US the original proposal by the Carter administration in 1977 regarding what would become the New Job Tax Credit (NJTC) was a hiring subsidy with no conditionality on net employment growth. Following many debates, the Congress eventually passed the NJTC which targeted growing firms. This came at the cost of a considerably more complex measure.[3] Despite this complexity, among the 147 hiring credits enacted by U.S. states from 1969 to 2012, 143 required that the number of jobs associated with hires is above specified thresholds.[4] In 2009, the Obama administration originally set out a hiring credit targeted on growing firms, while the Congress passed a measure, in the HIRE Act, which did not target specifically such firms. Employers who hired unemployed workers into new or existing positions between February and December 2010, could be exempted from their share of Social Security taxes on wages (reduction of around 6.2% in labor cost for a full-time low-wage worker). In addition, for each worker retained for at least a year, businesses may

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

[4]Neumark and Grijavla (2013)

claim an additional general business tax credit, up to $1,000 per worker. Like *zéro charges*, the HIRE measure was not restricted to growing firms. The only condition is that the new hire was unemployed during the 60 days before beginning work or, alternatively, worked no more than 40 hours for anyone during the 60-day period. This leaves access to a rather large pool of potential candidates. Our paper shows that *zéro charges*, which is a hiring subsidy without conditionality on net employment growth, did not induce firms to increase layoffs in order to hire workers at lower cost. We could argue that this is because there is little churning in France. As shown by Abowd et al. (1999), this is not the case: the level of churning is high since about 90 percent of entries into employment are on temporary jobs. As a consequence of this high churning, the gross cost of the hiring subsidy per job created born by the government is relatively high. It amounts to about 75% of the labor cost of one of these jobs. We estimate that about 95 percent of this cost goes to hires that would have been created absent the subsidy, meaning that there are large windfalls for firms. This leads us to explore the potential impact of hiring subsidies conditional on net job creation. We find that the cost per job created would have been about 40 percent lower if *zéro charges* had been conditional on positive net job creation, assuming that the take-up rate remained unchanged. But job creation would have been 60 percent lower. We also find that subsidies conditional on net job creation above the employment growth rate threshold of *minus* 4 percent would have maximized job creation at constant budget, creating 40 percent more jobs than *zéro charges,* if the take-up rate had remained identical for both types of subsidy.

The paper is organized as follows. Section 2 reviews the literature evaluating the employment effects of hiring subsidies. Section 3 describes the hiring subsidy scheme implemented in France in 2009 (*zéro charges*). Section 4 presents the data, descriptive statistics and the empirical strategy. The results are presented in section 5 and robustness checks in section 6. The last section concludes.

2   Literature review

The literature on the effect of hiring subsidies has been recently surveyed in Neumark (2013) who concludes that hiring subsidies do not have significant effects on employment when they are targeted at specific disadvantaged groups (such as long-term unemployed or disabled workers). Such targeted policies stigmatize their beneficiaries and entail substitution effects. However Neumark also concludes that non-targeted hiring subsidies may have significant effects on employment. The evidence, which is rather scarce, relies on two empirical evaluations of the New Job Tax Credit (NJTC), which subsidized growing firms during the late 70s. Using survey data, Perloff and Wachter (1979) compare firms declaring that they know about the NJTC and firms which do not. They conclude that employment grew 3 percent faster thanks to the NJTC. They concede that their result is an upper bound of the true effect as serious endogeneity bias affect their comparison. Using aggregate time series, Bishop (1981) concludes that the NJTC had significant positive employment effects (between 0.66 and 2.95%) and negative effects on prices.

The NJTC was the only US hiring subsidy implemented at the federal level before the HIRE Act. At the state-level, there were many more Job Creation Tax Credit. Chirinko and Wilson (2010) construct a large data set documenting all those policies. They conduct an event-study (difference-in-differences across US states) and estimate that the tax credit increased employment the month when firms both know and can qualify for the tax credit. They pay

particular attention to dynamic effects and document an Ashenfelter dip between the signing and qualifying dates. More recently, Neumark and Grijalva (2013) analyze state-level hiring subsidies. Using difference-in-differences across states, they do not find positive effects on job growth for many of the types of hiring credits. However, some specific types of hiring credits, targeting the unemployed and those that allow states to recapture credits when job creation goals are not met, appear to have succeeded in boosting job growth. Moreover Neumark and Grijalva (2013) point out that inefficiencies for certain types of hiring credit seem related to churning behaviors.

Most of the European literature on the empirical effect of labor cost reduction concerns *permanent* labor tax relief. Our study is to our knowledge the first empirical evaluation of a *temporary* hiring subsidy in France. Crepon and Desplatz (2001), Cheron et al (2010) and Barlet et al (2010) find positive employment effects of payroll tax exemptions for low wage workers implemented in the early 90s in France. Givord et al. (2013) find that the zone Franche Urbaine program, comparable to US enterprise zones, which exempts businesses from taxes for a period of at least five years, had significant effects on both business creation and employment but also has significant negative spillovers on neighboring areas.

The evidence is mixed in other European countries. Huttunen et al. (2013) do not find any positive employment effect of wage subsidies targeted at older, full-time, low-wage workers in Finland. Neither do Bennmarker et al. (2009) find any positive employment effects in permanent firms of a 10% payroll tax reduction introduced in the Northern regions of Sweden in 2002. On the contrary, Goos and Konings (2007) find positive employment effect of payroll tax subsidies in Belgium. They show that increases in wage that tend to mitigate employment effects are lower in low-wage exporting industries.

From a theoretical point of view, Kitao et. al. (2010) build a search and matching model to compare the outcomes of hiring subsidies, payroll tax reduction and employment subsidies in bad times. They find that a hiring subsidy and a payroll tax deduction, as in the HIRE Act, can stimulate job creation in the short term, but can cause a higher equilibrium unemployment rate in the long term. The increase in the unemployment rate in the long term follows an increase in net wages and in job destruction. Such equilibrium effects are less likely to occur in the context of a temporary policy such as the one we study in our paper. Jung and Kuester (2013) also build a search and matching model and characterize the optimal policy mix (production tax, hiring subsidy, lay off tax and unemployment benefits) along the business cycle. They show that all three policy instruments should be risen during recessions.

3   Institutional background

The measure *zéro charges* (zero contributions) was announced by the French President on the 4th of December 2008. According to the original announcement, any hiring (or fixed-term contract renewal) in a firm with less than 10 employees occurring from the date of the announcement until the 31st of December 2009 could benefit during the same year from a complete employer social contribution relief at the level of the minimum wage, and a relief decreasing in the gross hourly wage level up to 1.6 times the minimum wage (see figure 1). The amount of this new subsidy was substantial. It amounts to 200 euros per month for a full-time worker paid at the minimum wage (a reduction of 12% of the labor cost). When the gross wage is 30 percent above the minimum wage, the subsidy rate represents only 4 percent of the labor cost.
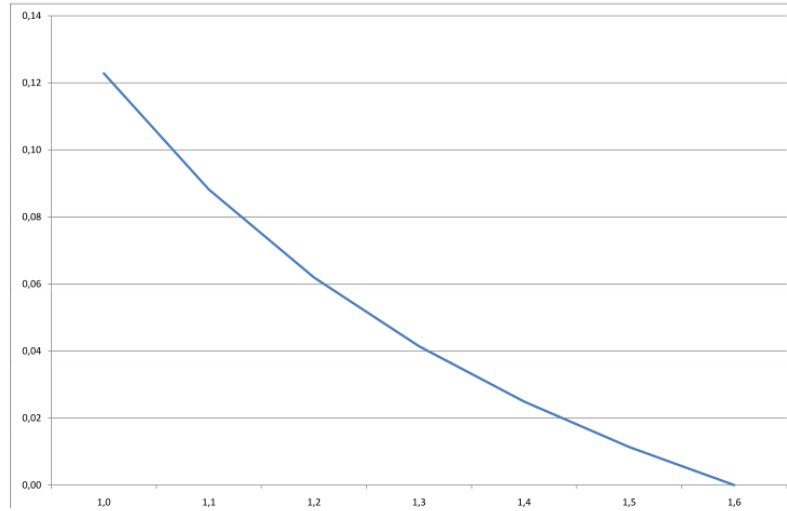
Figure 1: The subsidy rate as a percentage of the labor cost. Note: The horizontal axis reports the ratio of gross wage over the gross minimum wage.

The maximum amount over 12 months represents 2,400 euros. The new relief would come on top of the existing general social contribution reduction on low wages which concerns all firms in the private sector, called the "*Fillon reduction*" . Before the first announcement, the policy was not anticipated. This is confirmed by figure 2 which shows that Google search on the item "hiring subsidy" (aide embauche) started to increase in December 2008, once the announcement for the subsidy was made. There is very little Google search for this policy before early 2009.

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

- First, only firms and associations belonging to the private sector benefiting could get the subsidy. Firms and associations should ask for the additional relief *zéro charges* for each hire separately, filling out a one-page form and attaching the labor contract.
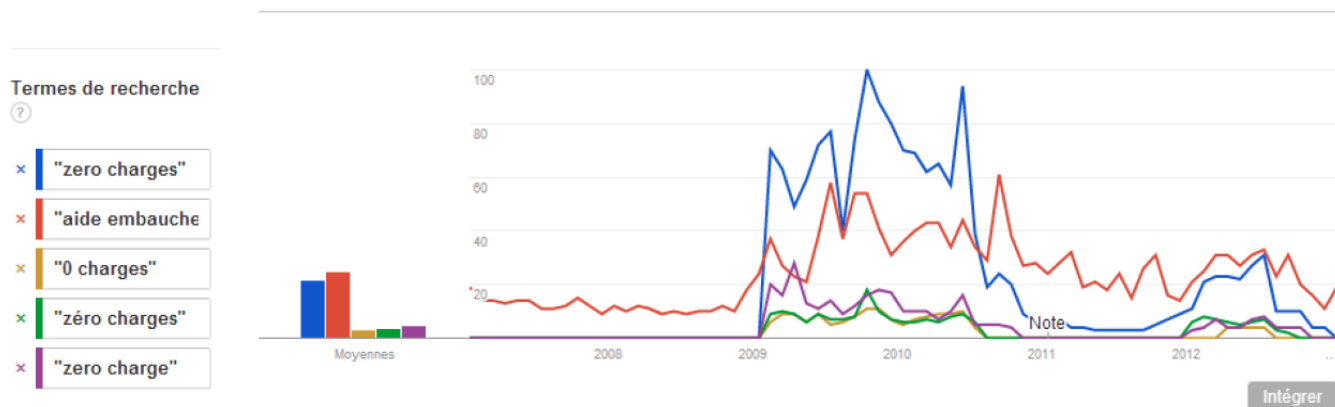

Figure 2: Results of google search for the policy name. Source: google trends website.

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

- Third, only entities with less than 10 full-time equivalent employees [5] on average between January and November 2008 could apply. Hence, the period used to define the size criteria ends before the announcement of the policy, on 4 December 2008. This means that the size criteria cannot be manipulated by firms wishing to benefit from the subsidy. Moreover a growing firm reaching 10 or more employees over the year 2009 can still continue to receive subsidies or apply for new hires until the end of 2009. Studying effects of the policy in 2010 is more challenging as some firms treated in 2009 may not be able to apply in 2010, because eligibility for the extended period was then based on the average size over 2009, which again should not exceed 10 employees.

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

On 16 November 2009, the policy was extended to hires occurring up to 30 June 2010. At this occasion, the duration of the subsidy was extended for up to 12 months from the hiring date, instead of 31 December 2009 for the initial scheme. This new rule was also applicable to hires made in 2009 before the announcement and which already benefited from *zéro charges*. Firm below the average of 10 full-time equivalent employees from January 2009 to December 2009 were eligible to the extented program for their new hires in 2010.

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

Moreover there are no significant discontinuities at the 10 employees threshold in the French legislation, inducing a change in the labor cost or in the labor regulations (see Ceci-Renaud and Chevalier, 2010). As a consequence we do not see any accumulation of firms just below the threshold (see figure 3). We can thus be confident on the absence of sorting around the size threshold (such a sorting could have meant that firms below and over the threshold react differently to the business cycles).[6]

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[5] The size criteria is very precise (see cerfa n 13838-01). Only ordinary employees are kept in the size's computation (excluding apprentices and sponsored employees). The size criteria is the average over several months of end-of-month sizes. Employees contributes *pro rata temporis*. The contribution of temporary employees also depends on their hiring date if it is within the considered month.

[6] This 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, 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).
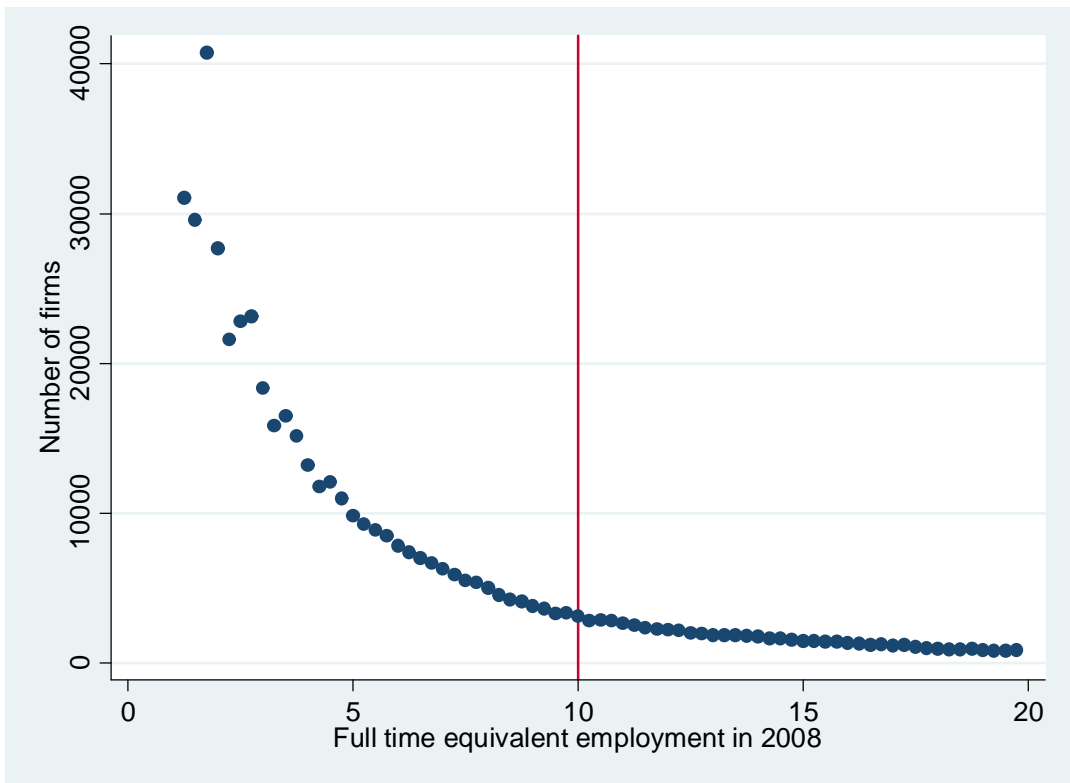
Figure 3: Firms'size density. Source DADS.

4 Data and empirical strategy

4.1 Data

We use an extremely rich and accurate set of information from two distinct sources:

- the *Déclarations Administratives de Données Sociales* ("DADS") built by the French Statisticdal Institute (Insee) from firms' social contributions declarations. Firms declare employment spells, the number of hours worked, and associated wages for each worker at the plant level.

- and the administrative file produced by the French Public Employment Service (*Pôle emploi*) which administered the payment of the subsidy, designated as the "hiring subsidies" file. It contains information on the firms which took up *zéro charges*, the level of the hiring wage, and the exact amount and duration of the subsidy received.

The DADS cover around 85% of French wage earners. Civil servants from the French central administration (ministries) and workers from the care sector or employed by household employers (e.g. for house-keeping or child care) do not appear in the employment registers (until 2009). We append the employment registers from 2005 to 2009, creating a panel of firms.[7] We only consider employment contracts with a duration at or above one month. We restrict the sample to firms in the private sector and drop the agriculture sector and 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 and lowest employment growth rates in the sample [8] All relevant information for firms' size, the number of hires, separations, the level of wages and the duration of contracts offered are aslo taken from the DADS which describes the universe of firms relevant to our evaluation.

The eligibility condition based on the size threshold is also computed from those registers. To compute full time equivalent, it is considered that each worker in firm i contributes to the firms' size up to the share of the time worked over the year. For example, an individual working all day long during six months represents 0.5. An individual working during the entire year, but only on mornings, also represents 0.5.

Figure 4 displays the fraction of firms actually benefiting from the subsidy in 2009 depending on the number of full time equivalent employees as measured over the first 11 months of 2008 (consistently with the eligibility criteria). This fraction decreases sharply for firms with 8 employees or more and goes to zero for firms with size between 12 employees or more (see figure 4). The drop before the threshold of 10 employees and the positive fraction, of about 3 percent, of firms from 10 to 12 employees benefiting from the subsidy seems to be consequence of measurement error. This is not surprising to the extent that computing the number of full time equivalent employees according to the eligibility criteria 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.

The fraction of eligible hires to which the subsidy is actually paid is higher than the fraction of firms that benefited from *zéro charges* because only hires of workers paid below 1.6 times

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[7]There is no permanent identifier for individual worker. Our data is not a panel of individual worker.

[8]Further information on the data sets and on data is available in table (9) in the appendix.

Figure 4: Fraction of firms that benefited from the subsidiy by firm's size in 2009

the minimum wage are eligible. According to the "hiring subsidy" file, 0.7 million hires are subsidized in 2009. According to the DADS, 1.8 million hires are eligible in 2009. This implies a take-up rate of 39%.

4.2 Empirical strategy

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

The hiring subsidy can influence employment throught 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

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

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 subsidies aim at increasing employment through their effect on hires. However, it is possible that firms benefit from important amounts of hiring subsidies while the effects on net employment are negligible. If low employment is the consequence of an insufficient demand for the products of the firms, hiring subsidies can induce firms to fire some employees, and then to replace them with workers for whom they can collect the subsidy. In this context, the main impact of the hiring subsidy may be to increase churning and wages with very little employment effects. 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 subsidy increases employment, it is nevertheless possible that its effects on hours workerd is limited, because firms have incentives to substitute hours of subsidized employees, for those of non subsidied employees. Therefore, it is important to also analyze the reply of hours of work.

In what follows, we estimate the impact of the hiring subsidy on employment, 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 (10 full time equivalent end-of-month size averaged between January and November of year $t-1$) and estimate the following difference-in-differences model:

$$Y_{it} = \alpha + \beta Z_{it} + \gamma D_{it} + \delta Z_{it} D_{it} + b X_{it} + u_{it} \tag{1}$$

where Y_{it} is the outcome of firm i in period t, Z_{it} an eligibility dummy equal to 1 if the firms' size in period $t-1$ is below 10, D_{it} a dummy for year 2009 when subsidies can be claimed, X_{it} a set of covariates. δ is our parameter of interest. It captures the differential evolution of the group targeted by the subsidies program.

In the benchmark estimations, we chose a bandwidth between 6 (included) and 14 (excluded) employees. In Table 1, we report characteristics of our 2009 cohort. These characteristics are measured in 2008. In the first three columns, we compare small and medium firms. Small (eligible) firms are more frequently operating in the manufacturing industry and to a less extent also in the retail, transport and merchant services than the bigger firms. They are more frequently located in the Parisian area and less in the North West part of France. Almost half of small

firms have sales lower than 2 millions euros, while one medium size firms out of four does. Small firms are also younger: 13 percent have less than 5 year old vs.10 percent for medium size firms. The composition of the workforce (in 2008) differs between small and medium firms. Small firms have more white collar employees, while medium firms have more blue collar workers. Finally, the share of low-paid workers and that of part-time workers are higher in small firms.

5   Results

We now turn to our main results on the effect of the subsidy on employment, hours worked, hires, separations and churning. These results allow us to evaluate the cost per job created and the windfalls for firms. Since we find that the windwalls represent a very large share of the total cost of the subsidy, about 94 percent, we explore the consequence of alternative hiring subsidy schemes conditional on net job creation. Various robusteness checks are presented in the next section.

5.1   Employment and hours

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

The validity of difference-in-differences estimations heavily relies on the common trend assumption. We describe the common trend for treated firms with previous size between 6 and 10 (excluded) and control firms with previous size from 10 to 14. The outcome is average employment growth in each group. Employment is computed at the firm level. To ensure that employment in 2008 is not influenced by the hiring subsidy that was announced on 4 December 2008, employment in year t is equal to employment on 30 November of year t. Let $L_{i,t}$ denotes employment in firm i on 30 November of year t, average employment growth for each group is $(\sum_i \frac{L_{i,t}-L_{i,t-1}}{L_{i,t-1}})$. Figure 5 shows that the average employment growth is negative for the treatment group and the control all along the period. This is because new entrants, which typically account for a significant share of employment growth, are excluded from the sample.[9] The difference in employment growth rates between the treatment group and the control group is negative and constant from 2006 to 2008. In 2009, this difference becomes positive: the growth rate of the treatment group drops by 0.9 percentage points while that of the control group drops by 1.6 percentage points. Figure 6 shows that the same phenomenon arises for hours of work: the average growth rate of total hours of work per firm of the treatment group is below that of the control group from 2006 to 2008 and becomes larger than that of the control group in 2009. This points to positive treatment effects, that we estimate below.

In Table 2, we present our difference-in-differences estimates for different outcomes (in lines) and specifications (in columns). In column 1, our baseline sample comprises all cohorts from 2006 to 2009 without covariates. In column 2, we add covariates control. In column 3, we restrict the sample to cohorts 2008 and 2009 (to avoid potential specification errors about underlying trends). The results are very stable. They indicate that the subsidy increased the rate of growth of employment of the treatment group by 0.7 percent (column 2, line 4 of Table 2). Table 2 shows that the impact of the subsidy on the growth of hours of work is similar to that on employment,

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[9]Remind 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.
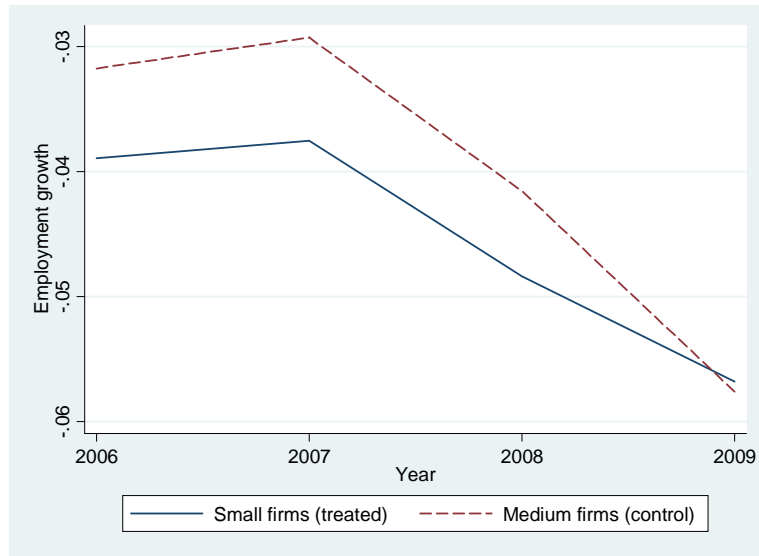
Figure 5: Average growth rate of employment in firms in the treated and control groups. Growth rate of employment between 30 November of year $t-1$ and year t. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November)

Figure 6: Average growth rate of hours in firms in the treated and control groups. Growth rate of the number of hours worked within each firm between November of year t and November of year $t-1$. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November).
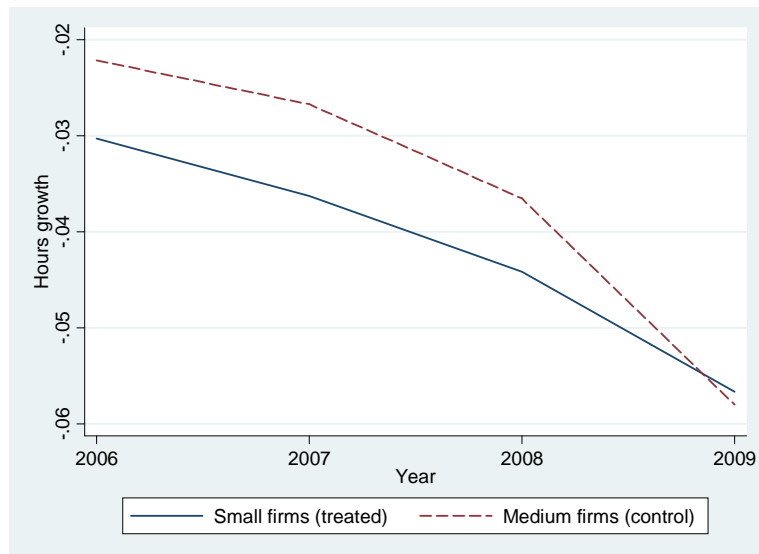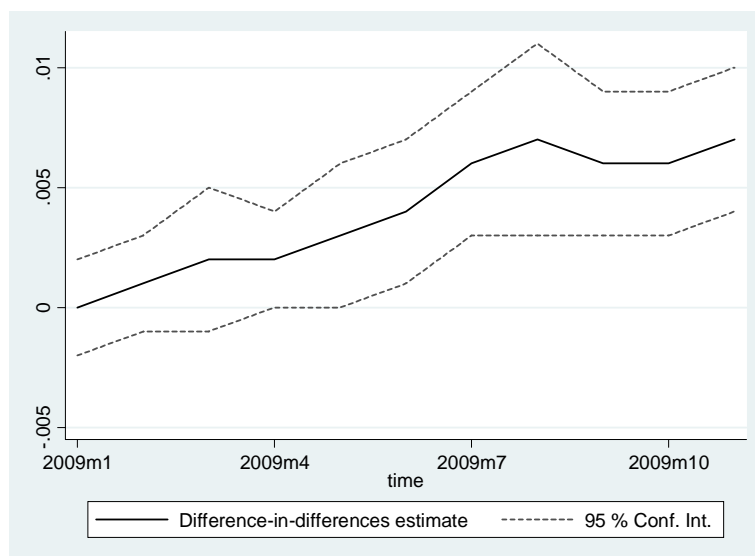
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Figure 7: Difference-in-differences estimates of the impact of the hiring subsidy on the employment growth rate. The outcome is $(L_{m,t} - L_{t-1})/L_{t-1}$ where $L_{m,t}$ denotes employment at the end of month m of year t and L_{t-1} employment on 30 November of year $t-1$. Estimations include years and covariates presented in table 2, column 2.

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 subsidy had no impact on the survival of firms, meaning that the main effect of the subsidy has been to raise employment in surviving firms. Indeed, estimates on the subsample of surviving firms are identical to that of all firms, as shown in table 10 in appendix.[10]

Our rich data set allows us to show the evolution of employment over the year 2009. Figure 7 displays the difference-in-differences estimates for the effect on the subsidy month by month over the year 2009. The estimated impact of the subsidy 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 demand, in a delay that is clearly infra annual (Hamermesh, 2013).

Since the subsidy decreased the total wage bill of firms below 10 employees by 0.4 percent,[11] these estimates point to an employment elasticity with respect to the hiring subsidy around 1.75. This figure is a lower bound of the wage elasticity of labor demant for several reasons.

First, the subsidy might have increased wages. Estimating the precise impact of *zéro charges* on wages is tricky because the subsidy may raise wages of each worker on one hand; but, on the other hand, it may also induce firms to hire less skilled workers, with lower wages, since the

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[10]Our estimates are not weighted by firm size. This could bias our results if, for instance, the elasticity of labor demand depends on the size of firms. We checked that estimates provided along the paper yield almost identical results as weighted estimates. This is illustrated by table 11 in appendix which shows the weighted estimates corresponding to those displayed table 2.

[11]In 2009 the total cost of the hiring subsidy amounts to 361 millions euros and the total wage bill in firms below 10 employees to 87 billions euros.
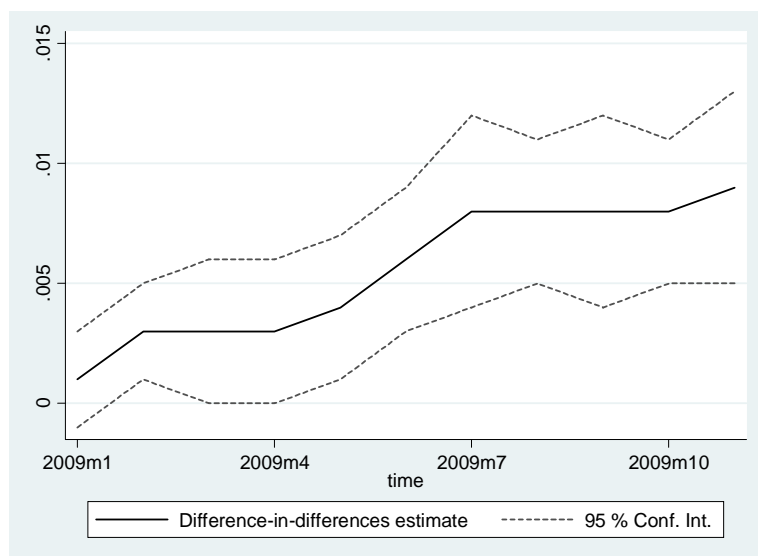
Figure 8: Difference-in-differences estimates of the impact of the hiring subsidy on the growth rate of hours of work. The outcome is $(h_{m,t} - h_{t-1})/h_{t-1}$ where $h_{m,t}$ denotes hours of work in month m of year t and h_{t-1} hours of work inNovember of year $t-1$. Estimations include years and covariates presented in table 2, column 2.

subsidy decreases with the wage as shown on figure 1. The wage profile of the subsidy makes it impossible to provide reliable estimates of their true effect on wages that would require to disentangle these two effects. However, the pressure of the subsidy on wages is likely small in our context, because the subsidy is targeted to low wages, which may be over their competitive level due to the minimum wage and to the collective agreements that are most often binding for low wage workers, especially in small firms.

Second, the adjustment of employment requiring time, it is likely that the employment level in November 2009 did not fully reach its target. Since a new hiring subsidy scheme targeted to firms with less than 10 full time employees in previous year has been introduced at the end of November 2009, it impossible to evaluate the impact of *zéro charges* beyond this date.

Third, since *zéro charges* is a temporary subsidy ending at the end of 2009, its impact on employment is smaller than a permanent subsidy for firms in which there are labor adjustment costs.

Fourth, as shown by figure 4, the take-up rate of firms between 8 and 10 full time equivalent employees in previous year is small relative to firms between 4 and 8 full time equivalent employees in previous year. It is reasonable to interpret this phenomenon as a consequence of measurement error on the number of full time employees. This measurement error also explains that about 3 percent of firms with 10 to 12 full time equivalent, that belong to the control group, benefited from the hiring subsidy. Table 3 presents the estimates for different bandwidths. We find results consistent with those obtained with our benchmark bandwidth covering firms from 6 to 14 employees in previous year. In particular, the difference-in-differences estimates are higher when the treatment group includes firms with higher take-up rates. This is the case in columns 3 and 4, as shown by figure 4. Column 5 shows that the estimates are also higher when the control group excludes firms with residual take-up. This suggests that our benchmark estimates, which

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rely on firms from 6 to 14 full time equivalent employees in previous year, are lower bounds of the impact of the subsidy. The corresponding estimates of wage elasticity of labor demand equal 2.25 and 3 when the bandwidth goes from 5 to 15 employees and [5,8]-[12,15] employees respectively.

Accordingly, our results suggest that the wage elasticity of labor demand is quite high, around 2, for small firms and low wage workers. This figure might seem relatively large with respect to the conventional order of magnitude which points to a value of the elasticity for low wage workers around 1 (Hamermesh, 1993, Cahuc et al. 2014). The relative large elasticity we find may be related to the specific behavior of small firms, for which the elasticity of labor demand might be larger than that of larger firms. It might also be the consequence of our empirical strategy. We rely on a very good natural experiment, which induced a non expected exogenous shock on labor cost. We also exploit comprehensive administrative data which provide precise information on employment and hours of work. Most empirical studies on labor demand do not rely of these two features. In these studies, changes in labor costs are generally expected by firms, provided that they are truly exogenous, which is generally not the case. High quality data at the firm level are also very scarce. This may lead such studies to underestimate the wage elasticity of labor demand. From this perspective, it is worth stressing that the rare empirical contributions that provide estimates of labor demand relying on proper identification strategy also find high wage elasticity of labor demand (Angrist, 1993, Acemoglu et al. 2004).

5.2 Churning and separations

Table 2 shows that the hiring subsidy raises the separation rate. However, this does not necessarily mean that firms replace incumbent workers with new workers to benefit from the subsidy. Using French data over the period 1987-1990, Abowd et al. (1999) estimate that each job created in a given year is associated with 3 hires and 2 separations. Davis et al. (2012) also find that hires rise more than one-for-one with job creation in the US. This relation indicates that a higher incidence of recently formed matches at more rapidly growing firms generates higher separation rates. There are two reasons for this. One is purely mechanical: at given quit rate, the separation rate, equal to the number of separations during the period divided by employment at the beginning of the period (or by the average of employment at the beginning and at the end of the period), increases when employment grows faster. Another reason is due to the fact that filling a job requires to find the good match with the good worker, which is not always the case with the first hire. Accordingly, if the hiring subsidy fosters job creation, it may also increase churning, even if firms to do not strategically raise their separations to hire new workers at lower cost.[12]

The upper chart of Figure 9 shows the relation between the hiring rate and the employment growth rate in small size and medium size firms over the period 2006-2008. The vertical axis displays the average annual hiring rates[13] 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 subsidy had induced employers to replace incumbent workers with new workers

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[12]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).

[13]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 $t-1$.
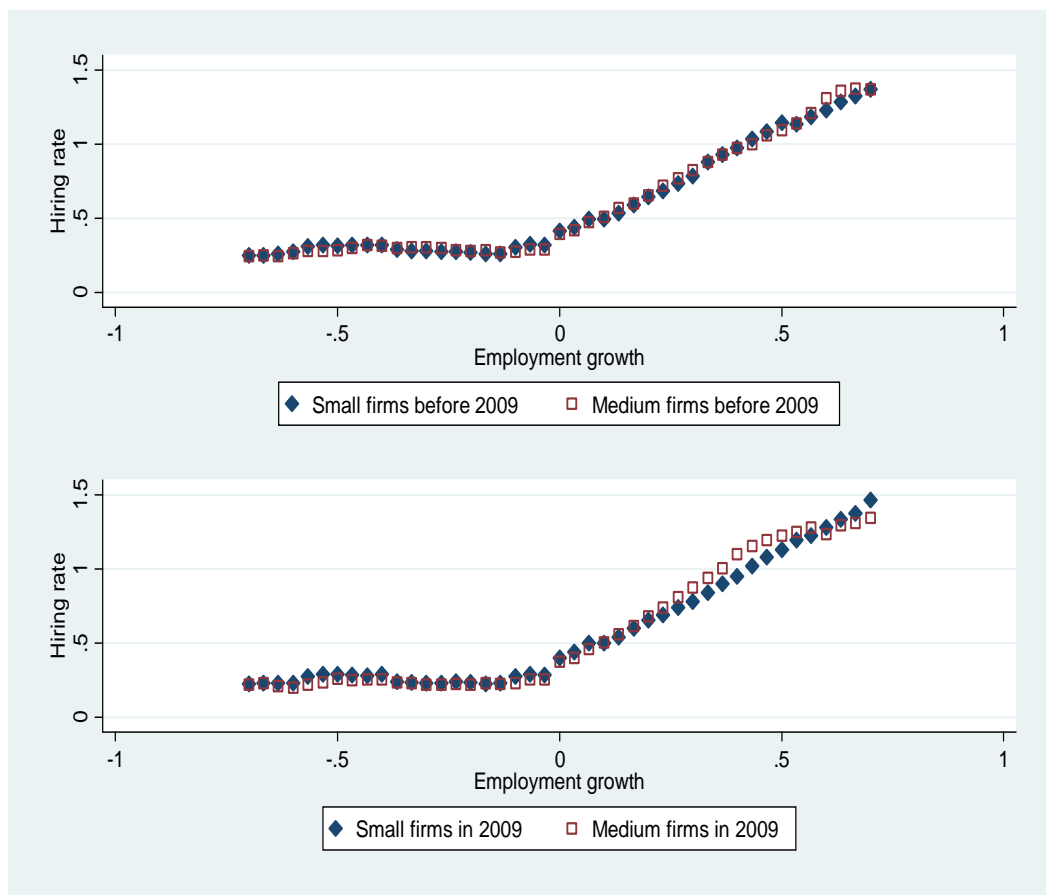
Figure 9: Hiring rate and employment growth rate in small size and medium size firms. The upper chart displays the average of the mean hiring rate by employment growth rate bins over 2006-2008. The bottom chart displays the average hiring rate by employment growth rate bins in 2009. Dots represent 6-bin moving averages. Small size firms have 6-10 (excluded) full time equivalent employees in previous year. Medium size firms have 10-14 full time equivalent employees in previous year. Source: DADS.

only to benefit from the subsidy, in 2009, the hiring rate, at a given employment growth rate, should be higher in small firms, eligible to the subsidy, than in medium size firms, non eligible to the subsidy. The bottom panel of figure 9 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 subsidy did not induce firms to increase labor turnover only in order to benefit from the subsidy.

5.3 Cost per job created and windfalls for firms

Based on our estimates, it is possible to compute the estimated gross cost per job created. The gross fiscal cost of the measure for 2009 was 361 million euros. The same year, thanks to the subsidy the total wage bill in firms with less than 10 employees in the previous year (87 billion) decreased by 0.4 percentage points. Hence, the implied elasticity of employment to the subsidy is $0.008/0.004 = 2$. Now the total number of jobs in small firms was 3.1 million in 2009, which based on the estimated elasticity of employment gives $3,100,000 \times 0.004 \times 2 = 24,800$ jobs created. This means that the gross cost per job created is $15,000$ euros per year (about 0.75 times the average cost of a full-time job at the minimum wage). This is gross cost, because it ignores the savings these created jobs generate 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 on these additional jobs. In France, it is usually estimated that the total average savings on social expenditure per job created for low wage workers amounts to $12,000$ euros per year. This makes the net cost of the hiring subsidy per job created relatively small, about $3,000$ euros per year.

We can compute the share or hires that have benefited from the subsidy and that would have been created absent the subsidy. According to our estimates, the impact of the subsidy on the hiring rate, H/L_{-1}, is equal to $\delta = 0.017$. The ratio of eligible hires over past employment in firms below ten full time employees in previous year is equal to 0.29. Accordingly, the share of hires among eligible hires that have been created by the subsidy amounts to $0.017/0.29 = 0.06$, meaning that 94 percent of the cost of hiring subsidy funds hires that would have occured absent the subsidy.

This large share implies that firms benefit from important windfalls associated with hiring subsidies.

5.4 Evaluation of the impact of hiring subsidies conditional on net job creation

Given the large windfalls for firms associated with the *zero charges* scheme, it is worth looking at alternative schemes that aim at reducing the cost of hiring subsidies. Many schemes rely on hiring subsidies conditional on net job creation. In order to shed light on the differences in the effects of subsidies conditional on net employment and non conditional on net job creation, we rely on a simple model of a firm with hiring and firing costs. This model, presented in appendix, compares the impact of the two schemes when the hiring subsidy σ per eligible hire is identical for both schemes. It shows that:

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

(a) In firms where firing costs and hiring costs are sufficiently low, employers layoff a positive share of incumbent workers who are replaced with subsidized workers.

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

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

Since our empirical evaluation concludes that *zéro charges*, which is a hiring subsidy non conditional on net job creation, did not induce firms to layoff workers in order to hire subsidized workers, we evaluate the potential impact of hiring subsidies conditional on net job creation assuming that we are in case 1b. We still compare the two shemes when the subsidy σ per eligible hire is the same for both schemes. If one assumes that the impact of *zéro charges* on the growth rate of employment is homogeneous across firms and that the take-up rate of eligible hires is identical for both types of subsidy, the cost ratio per job created between the subsidy conditional on net job creation above the growth threshold $\bar{g}$ and the non conditional subsidy is (see appendix):

$$\frac{\delta + \frac{1}{I(\bar{g})} \sum_i (g_i - \bar{g})}{\delta + \eta + g}, \tag{2}$$

where δ is the impact of *zéro charges* on the average employment growth rate ($\delta = 0.07$ according to table 2), g_i stands for the employment growth rate of firm i absent the subsidy, $I(\bar{g})$ is the number of firms eligible to the conditional subsidy, η and g are the average hiring rate and the average employment growth rate of the treatment group of *zéro charges* absent the subsidy. The term $\frac{1}{I(\bar{g})} \sum_i (g_i - \bar{g})$ can be either increasing or decreasing with the threshold $\bar{g}$ depending on the shape of the distribution of growth rates. Accordingly, the cost ratio does not necessarily decrease with $\bar{g}$.

The ratio of employment created by the subsidy conditional on net job creation over employment created by the non conditional subsidy is $L_{-1}(\bar{g})/L_{-1}$, where $L_{-1}(\bar{g})$ denotes total employment in 2008 of firms eligible to the subsidy conditional on net job creation above the growth rate threshold $\bar{g}$ and L_{-1} is total employment in 2008 of the treatment group of *zéro charges* (see appendix).

Figure 10 shows that the cost ratio amounts to 68 percent when only positive net job creations are subsidized (i.e.when $\bar{g} = 0$). In that case, the subsidy creates only 40 percent of jobs that are created with the non conditional subsidy. However, it cannot be optimal to set $\bar{g} = 0$ if the aim of the policy is to maximize job creation for a given budget, because the cost ratio increases with $\bar{g}$ when $\bar{g}$ is above -4 percent. This means that, in our context, a subsidy conditional on positive employment growth threshold, as it is the case in most US states (Neumark and Grijalva, 2013), cannot maximize employment creation.

In order to determine the value of the threshold $\bar{g}$ which maximizes the number of job created for a given budget, one needs to make some assumptions. Let us assume that the employment elasticity with respect to the subsidy (expressed in labor cost percentage) is constant and that the amount of the subsidy per hire is adjusted to balance the budget constraint when $\bar{g}$ is
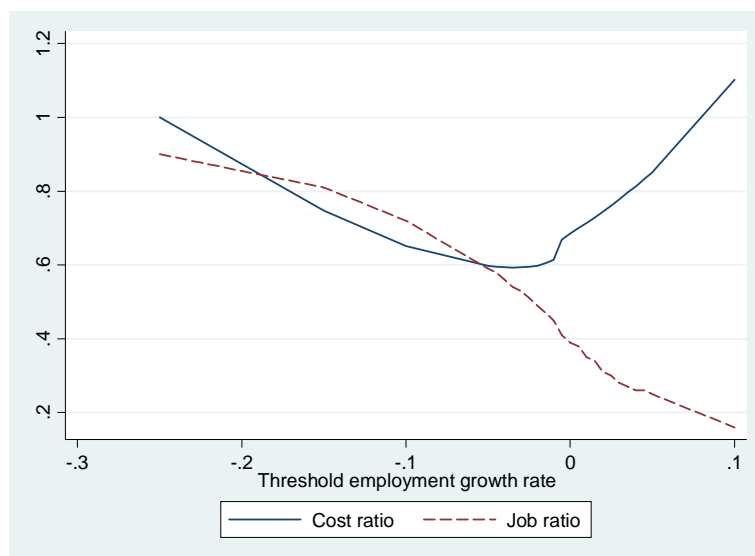
Figure 10: Cost ratio per job created and job ratio between the subsidy conditional on net job growth and the non conditional subsidy. The subsidy per hire is identical for both schemes. The cost ratio is the ratio of the cost per job created for the subsidy conditional on net job creation and over the cost per job created for the non conditional subsidy. The job ratio is the ratio of jobs created. The horizontal axis displays the growth rate threshold above which net job creations obtain the conditional subsidy.

changed. Then, job creation is maximized when the cost per job created is minimized. In our case, figure 10 shows that this means that it is optimal to set the employment growth rate threshold at-4 percent. At this level, figure 10 shows that the cost per job created is 40 percent lower than with the non conditional subsidy. This implies that the number of jobs created is also 40 percent higher.[14] This result suggests that it could have been worth targeting the hiring subsidy to net job creation and providing more generous subsidies per job created. However, this conclusion needs further investigations because subsidies conditional on job creation are much more complex to implement than non conditional subsidies. This implies that the take-up rate might be significantly lower with conditional subsidies. Moreover, since firms do not know with certainty when they hire workers if they will reach the threshold above which they become eligible when subsidies are conditional on net job creation, their impact might be smaller than that of non conditional subsidies.

6 Robustness checks

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

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[14]Let c stands for the cost per job created, by n the number of job created and by b the total expenditure. The budget constraint is $cn = b$. When b is constant, one has $\frac{dc}{c} = -\frac{dn}{n}$.
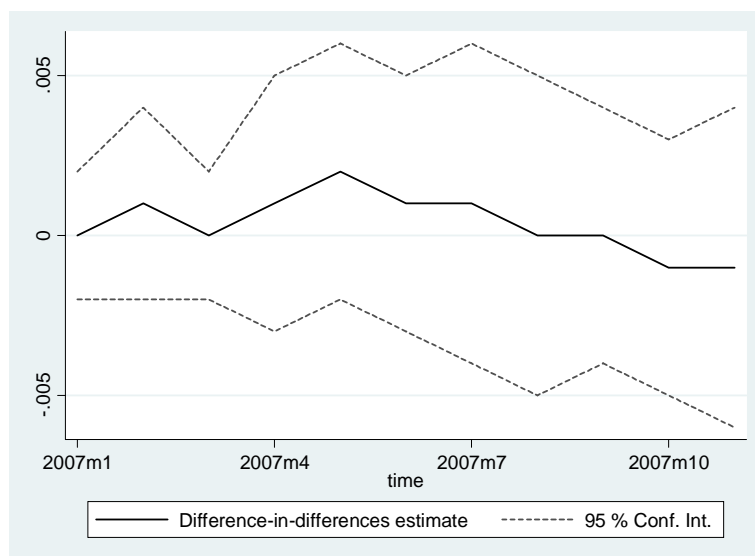
Figure 11: Placebo test as if the policy had been implemented in December 2006. Difference-in-differences estimates of the impact of the hiring subsidy if the subsidy had been introduced in December 2006. The outcome is the growth rate of employment $(L_{m,t} - L_{t-1})/L_{t-1}$ where $L_{m,t}$ denotes 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.

6.1 Year placebo tests

We perform a series of placebo tests using the data from 2005 to 2008. We use the specification of column 3 in table 2 as if the policy had been implemented in December 2006 (using cohorts 2006 and 2007) or December 2007 (using cohorts 2006, 2007 and 2008). Table 4 shows that employment, hours, hires and separations of the treatment and the control group evolved differently neither in 2007 nor in 2008, contrary to 2009 when *zéro charges* was introduced. Figures 11 and 12, which display the month-by-month difference-in-differences estimates for employment in year 2007 (if the shock had been in December 2006) and in year 2008 (if the shock had been in December 2007) respectively, show that the month-by-month evolution of employment was similar in the control and the treatment group over these 2 years. These results comfort the relevance of the common trend assumption.

6.2 Size threshold placebo tests and triple difference

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

If firms of previous size between 6 and 10 employees behaved differently from firms of previous
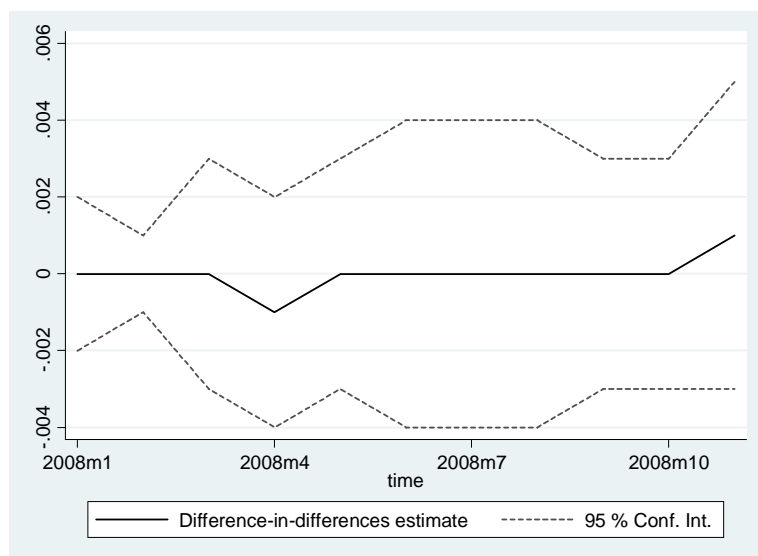
Figure 12: Placebo test as if the policy had been implemented in December 2007. Difference-in-differences estimates of the impact of the hiring subsidy if the subsidy had been introduced in December 2007. The outcome is the growth rate of employment $(L_{m,t} - L_{t-1})/L_{t-1}$ where $L_{m,t}$ denotes employment at the end of month m of year t and L_{t-1} employment on 30 November of year $t-1$. Estimations include years 2006-2008 and covariates presented in table 2.

size between 10 to 14 employees in 2009 because of differences in sizes and not because of the hiring subsidy, we would expect firms with 13 to 16 employees to behave differently from firms with 16 to 19 employees.[15] Figure 13 compares the average employment growth rate for surviving 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 2 groups does not change in 2009.

We proceed to the same comparison among small firms, belonging to our treatment group. Since figure 4 shows that the take-up rate drops dramatically for firms of size above 7, we compare the evolution of employment in firms with 1 to 4 (excluded) with that of firms of size from 4 to 7. Figure 14 shows that employment has evolved in parallel in the two groups of firms.

This suggests that the difference in employment growth across our treatment and control group does not stem from differences in behavior due to differences in size.

To dig further this issue, and net out the effect of any possible difference in growth rates across firms of different sizes unrelated to the subsidy, we proceed to a triple difference estimation where we compare the evolution of firms of previous size from 6 to 10 employees and 13 to 16 employees to that of firms from 16 to 19 employees. We estimate the following equation

$$Y_{it} = \alpha + \beta_6 Z_{6,it} + \beta_6 Z_{13,it} + \gamma D_{it} + \delta_6 Z_{6,it} D_{it} + \delta_{13} Z_{13,it} D_{it} + b X_{it} + u_{it} \qquad (3)$$

where $Z_{6,it}$ and $Z_{13,it}$ are dummy variables equal to 1 if the firms' size in period $t-1$ belongs to 6-10 and to 13-16 respectively. D_{it} a dummy for year 2009 when subsidies can be claimed,

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

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Figure 13: Average growth rate of employment in placebo groups. 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 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 previous year.

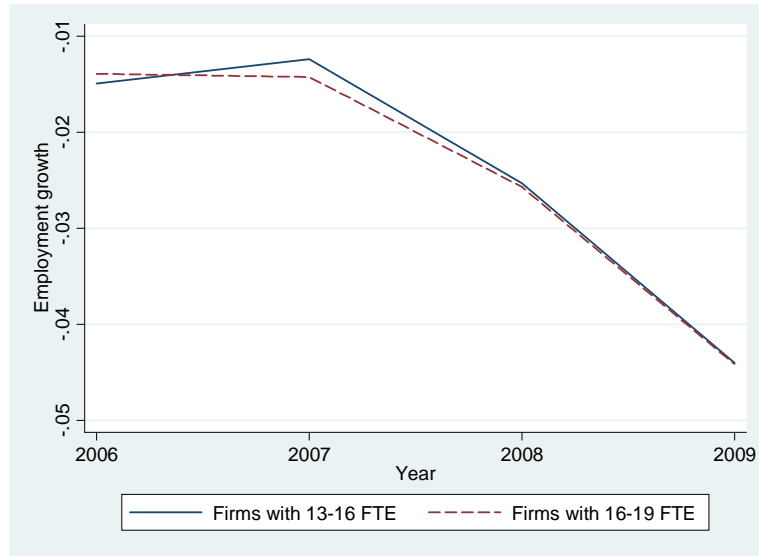Figure 14: Average growth rate of employment in placebo groups.Growth rate of employment between 30 November of year $t-1$ and year t. One group comprises firms of size between 1 (included) and 4 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The other group comprises firms of size from 4 (included) to 7 full time equivalent employees in previous year.
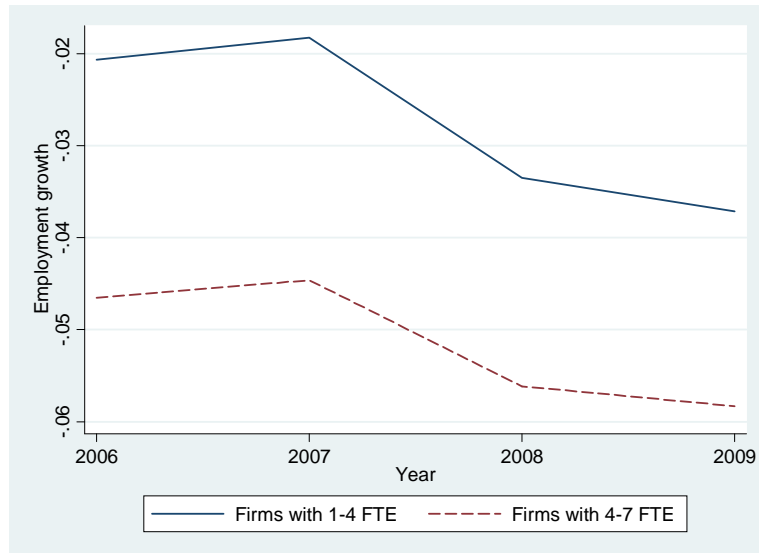
X_{it} a set of covariates. The population comprises firms with 6 to 10 and 13 to 19 employees in period $t-1$. Firms with 10 to 12 employees in previous year, of which a tiny fraction benefited from the subsidy, are excluded. In this setup, the impact of the subsidy is measured by the difference between coefficients δ_6 and δ_{13}. The estimate of coefficient δ_6 when Y_{it} is the growth rate of employment is presented in table 5, line 1. It is consistent with previous estimates that excluded firm with 10 to 12 employees as in the last column of table 3. The estimate of coefficient δ_{13} is presented in table 5 line 2. It shows that in 2009 firms with 16 to 19 employees in previous year had a similar evolution of employment and hours as firms with 13 to 16 employees in previous year. Line 3 of 5 shows that coefficients δ_6 and δ_{13} are indeed different, with p-values very close to zero. The following lines reproduce these estimates for hiring, separation and survival rates and yield similar conclusions.

6.3 Equilibrium effects

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

To deal with this issue we check whether employment and hours worked of the control group have been impacted by the share of subsidized hires in their employment pool. If there are equilibrium effects, in areas with a high share of subsidized hires we should observe lower growth rates of employment or hours among non eligible firms than in areas with a low share of subsidized hires. We distinguish 348 employment pools.[16] Within each employment pool, we compute the share of subsidized hires in 2009, and the average growth of employment and of hours worked of firms from 10 to 14 full time equivalent employees in previous year. We then compare the labor market outcomes in employment pools with different shares of subsidized hires. To do so, we estimate the following simple model :

$$Y_j = \alpha + \beta Share_j + bX_j + u_j \tag{4}$$

where Y_j is the average outcome (growth rate of employment or hours) of employment pool j between 1 December 2009 and 30 November 2010 among firms from 10 to 14 full time equivalent employees in previous year, and $Share_j$ is the share of subsidized hires in employment pool j over the same period dummy and u_j is a residual. We also include a number of area-specific controls X_j, such as the share of firms by sector, the distribution of firms' age, as well as the growth rate of employment, the hiring and separation rates in the employment pool in 2008. This aims at better controlling for the labor market dynamics specific to each pool. If the coefficient β is negative and significant, this indicates the presence of equilibrium effects.

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[16] We 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.

Now, the share of subsidized hires in 2009 is potentially endogenous in equation (4). This share is not only influenced by potentially exogenous factors, such as the number of small firms or the prevalence of low-wage employment in one given area before the implementation of the subsidy, but also by endogenous factors such as the share of eligible hires actually benefiting from the subsidy in 2009. For instance, in areas featuring higher employment growth among medium size firms it is likely that small firms will also experience a similar growth pattern. This would boost the share of eligible hires among all hires taking place the area in 2009, as well as the share of hires actually subsidized in 2009. This mechanism would yield a positive correlation between Y and $Share$, and bias estimates of equilibrium effects based on OLS. For this reason, the share of subsidized hires in 2009 is instrumented by the share of eligible hires among all hires in an employment zone in 2008 (when the subsidy was not yet implemented), and by the share of small firms below 10 full-time-equivalent employees at the end of 2007 and still present in 2008 among all firms present the same year in an employment zone.

Table 6 presents the estimates based on the OLS. It shows that there is no statitiscally significant correlation between the share of subsidized hires and the average growth rates of employment and hours in 2009. Tables 7 shows the estimates when the share of subsidized hires in 2009 is instrumented by the share of eligible hires and the share of small firms in 2008, both for the growth rate of employment and that of hours. The lower panel of this table presents the first step of the estimations. It shows that the share of subsidized hires in 2009 is strongly correlated with the instruments. The first panel shows the.second step of the estimation. The coefficients are those of the instrumented variable. These coefficients are similar to those obtained with the OLS: no equilibrium effects are detected, even when the full set of controls is included in the regression. The two instruments are not rejected by the overtidentification test (Sargan). However, the endogeneity test (Wu-Hausman) suggests that the share of subsidized hires in 2009 is not endogenous both when the dependent variable is the growth rate of employment in 2009 or when it is the growth rate of hours. In that case the estimates based on the OLS are more efficient.

Note that equilibrium effects could also play on workers flows, independently of any impact on employment and hours. This could happen if, for instance, smaller subsidized firms poached employees in medium size firms, forcing them to recruit more often in order to maintain employment. Unfortunately, we did not find any good instrument for the share subsidized hires when the dependent varaibles are hiring and separation rates. The instruments used for the growth rate of employment and hours are rejected by the overidentification tests These instruments turn out to be are strongly correlated with the hiring and separation rates in medium-size firms in 2009.

7   Conclusion

This paper shows that hiring subsidies targeted to small firms and low wage workers have had a significant impact on employment in France during the 2008-2009 recession. The hiring subsidy, 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 relative high hiring and firing costs and exogenous wage. These results are also at odd with the keynesian conception according to which the labor cost has no employment effects during recessions. We do find that employment reacted strongly and quickly to labor

cost in the hollow of the recession.

We think that one of the noticeable contribution of our paper is to show that the wage elasticity of labor demand is relatively high, at least 2 in absolute value, for low wage workers and small firms in France. It is likely that this relatively high figure, with respect to the results of other studies, relies on our identification strategy which exploits a truly exogenous and non anticipated change in labor cost and on our exhaustive and accurate database. These features of our evaluation allow us to avoid biases, present in most studies on labor demand, that lead to underestimate the labor demand elasticity.

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

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Table 1: Eligible/ Ineligible fimrs' characteristics 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	.241	.194	.0000	.134	.158	.0000
Construction	.178	.175	.3060	.188	.177	.0000
Retail and transport	.301	.287	.0000	.310	.295	.0000
Hotels and restaurants	.092	.082	.0000	.134	.080	.0000
Merchant services	.242	.230	.0000	.194	.259	.0000
Non-merchant services	.033	.037	.0050	.036	.031	.0000
Parisian area	.240	.231	.0030	.153	.273	.0000
North-West	.242	.253	.0000	.265	.237	.0000
North-East	.121	.123	.3640	.128	.116	.0000
South-East	.267	.263	.2380	.305	.252	.0000
South-West	.130	.130	.8390	.149	.121	.0000
Sales below 2 millions euros	.474	.225	.0000	.522	.423	.0000
Young firm (age below 5 years)	.133	.100	.0000	.144	.128	.0000
Mean share of...						
... male managers	.207	.216	.0000	.163	.223	.0000
... female managers	.125	.121	.0020	.109	.131	.0000
... male white-collar	.082	.079	.0020	.095	.075	.0000
... female white-collar	.215	.191	.0000	.255	.199	.0000
... male blue-collar	.335	.351	.0000	.343	.336	.0000
... female blue-collar	.036	.042	.0000	.035	.036	.3360
Mean share of ...						
...low-wage workers	.599	.586	.0000	.706	.579	.0000
...part-time workers	.187	.148	.0000	.180	.192	.0000
Nb. of obs.	64, 587	27, 403		18, 344	48, 177	

Source : DADS (Insee). Note : Low-wage workers earn between the minimum wage and 1.6 times this amount
(on hourly basis). Part-time workers work below 80 percent of normal working hours.

Table 2: Difference-in-differences estimates.

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.008***	.007***	.007***
	(.002)	(.002)	(.002)
Hours growth	.010***	.009***	.008***
	(.002)	(.002)	(.002)
Hiring rate	.015***	.014***	.017***
	(.003)	(.003)	(.003)
Separation rate	.007***	.007***	.010***
	(.003)	(.003)	(.003)
Survival rate	−.000	−.000	−.001
	(.001)	(.001)	(.001)
Nb. Observations	365,740	365,740	187,137

Source : DADS (Insee). Note : this table presents our difference-in-differences estimates for different outcomes (lines) and different specifications (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t; the growth rate of the number of hours worked between November of year t-1 and November of year t; the number of hires from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; 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 varying the bandwidth.

Size bandwidth	7-13	6-14	5-15	[5,8]-[12,15]
Employment growth	.005**	.007***	.009***	.012***
	(.002)	(.002)	(.002)	(.002)
Hours growth	.006***	.009***	.012***	.014***
	(.002)	(.002)	(.0002)	(.002)
Hiring rate	.010***	.014***	.016***	.019***
	(.003)	(.003)	(.0032)	(.003)
Separation rate	.006*	.007***	.007***	.008**
	(.003)	(.002)	(.003)	(.003)
Survival rate	−.000	−.000	.000	.000
	(.001)	(.001)	(.001)	(.001)
Nb. Observations	255,255	365,740	496,630	337,164

Source : DADS (Insee). Note :this table displays the DID estimates varying the bandwidth (in colums). 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 4: 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 (lines) and different placebo years (columns, 12 months starting from December 2006 or 2007, instead of 2009). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t and year t-1; the growth rate of the number of hours worked between November of year t and November of year t-1; the number of hires from 1 December of year t-1 to 30 november of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 november of year t divided by employment on 30 November of year t-1; and the number of excess reallocation from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1. As covariates, we include year, sector and regions dummies, as well as their interactions, we also include dummies for firm age, firms with sales below 2 millions euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 5: Placebo size and triple difference.

Covariates		No	Yes
Employment growth	δ_6	.012***	.011***
		(.003)	(.003)
	δ_{13}	.000	.000
		(.003)	(.003)
	p-value	.0000	.0000
Hours growth	δ_6	.014***	.012***
		(.003)	(.003)
	δ_{13}	.001	.001
		(.003)	(.003)
	p-value	.0000	.0000
Hiring rate	δ_6	.024***	.021***
		(.004)	(.004)
	δ_{13}	.004	.003
		(.005)	(.005)
	p-value	.0000	.0000
Separation rate	δ_6	.012***	.010***
		(.004)	(.004)
	δ_{13}	.004	.004
		(.005)	(.005)
	p-value	.0000	.0000
Survival rate	δ_6	−.000	−.000
		(.001)	(.001)
	δ_{13}	−.000	−.000
		(.002)	(.002)
	p-value	.9057	.7911
Nb. Observations		352, 743	352, 743

Source : DADS (Insee). Note : Column (1) presents our difference-in-differences estimates for different outcomes. The treatment group comprises firms size between 13 (included) and 16 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms size between 16 (included) and 19 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). This corresponds to coefficient delta13 in equation (3). Column (2) presents the difference-in-differences estimates when the treatment group comprises firms size between 6 (included) and 10 (excluded) full time equivalent employees in previous year and the control group comprises firms size between 16 (included) and 19 (excluded) full time equivalent employees in previous year This corresponds to coefficient delta6 in equation (3). Column (3) is the p-value for the test of equality of the estimates of columns (1) and (2). The outcomes are 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; As covariates, we include year, sector and regions dummies, as their interactions, we also include dummies for firms 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 6: OLS Estimates of equilibium effects of the growth rate of employment and hours in 2009 among firm with 10-14 employees.

	OLS			
Area Weights	No		Yes	
Covariates	No	Yes	No	Yes
Employment growth	.063 (.041)	.047 (.042)	.028 (.046)	.024 (.048)
Hours growth	.094** (.043)	.063 (.044)	.040 (.049)	.030 (.051
Nb. Observations	348	348	348	348

Source : DADS (Insee). Note: The dependent variable is either the average growth rate of employment or that of hours over 12 months from 1 December 2008 to 30 November 2009 in each employment pool. The independent variables is the share of subsidized hires in 2009, which is the ratio of all subsidized hires and the sum of all hires in the employement pool in 2009.

Table 7: Instrumental Variables Estimates of Equilibium effects on the growth rates of employment and hours among firms with 10-14 employees.

	IV 2SLS			
Area Weights	No		Yes	
Covariates	No	Yes	No	Yes
Employment growth	.053 (.063)	.045 (.063)	.050 (.082)	.050 (.064)
Wu-Hausman test (p-value)	.8405	.9794	.7318	.6327
Sargan test (p-value)	.7782	.6389	.7401	.5162
Hours growth	.076 (.066)	.041 (.066)	.048 (.091)	.021 (.071)
Wu-Hausman test (p-value)	.7259	.6851	.9121	.8854
Sargan test (p-value)	.8401	.7325	.6886	.5487
First stage coefficients				
Share of eligible hires in 2008	.256*** (.037)	.228*** (.040)	.254*** (.037)	.220*** (.0357)
Share of small firms in 2008	.748*** (.059)	.746*** (.063)	.546*** (.056)	.570*** (.058)
p-value	.0000	.0000	.0000	.0000
Nb. Observations	348	348	348	348

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

Table 8: Supplementary table: sample description

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

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

A Appendix

A.1 The model with hiring and firing costs

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

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

Let us start analyzing hiring subsidies non conditional on net job creation.

There is a temporary hiring subsidy, σ, that allows the firm to reduce the labor cost of entrants, equal to the wage w in the absence of subsidy. Let us denote by $V(A, \sigma, L_{-1})$ the value function of the firm during the current period, where 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

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

subject to the law of motion of employment:

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

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

The first order conditions are

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

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

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.

- **Case 1:** $H > 0, F > 0, F < L_{-1}$. We get from the first order conditions, with $\lambda_H = \lambda_L = \bar{\lambda}_F = 0$:

$$c_F'(F) + c_H'(H) = \sigma \tag{A4}$$

The current values of L, H and F are defined by

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

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

and equation (A4) with $F \geq 0$, $H \geq 0$.

- **Case 2:** $H > 0$, $F = (1 - q)L_{-1}$. 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 we find that hires and employment are defined by

$$\frac{R_L(A, H)}{1 - \beta(1 - q)} = w - \sigma + c_H'(H). \tag{A7}$$

The amount of subsidy paid to the firm is σH, where $H = L - (1 - q)L_{-1} + F = (L - L_{-1}) + qL_{-1}$.

- **Case 3:** $H > 0$, $F = 0$. In this case, $\lambda_H = 0$ and $\lambda_F > 0$. Thus, the first order conditions imply

$$R_L(A, L) = w - \sigma + c'_H(H) - \beta\mathbb{E}\frac{\partial V(A', \sigma', L)}{\partial L}$$

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

To see the effect of hiring and firing costs, let us introduce variables that change these costs. Let us set Δ_i, $i = H, F$, assuming that the cost functions c_i are mutliplied by $(1 + \Delta_i)$. Let us denote by $F(\Delta_F, \Delta_H) \geq 0$ and $L(\Delta_F, \Delta_H) \geq 0$ the value of F and L corresponding to (Δ_F, Δ_H) respectively in case 1. In case 1, the derivatives of L with respect to Δ_F and Δ_H are negative. From equations (A4) and (A5) we can write the equation that defines $F(\Delta_F, \Delta_H)$ as a function of $L(\Delta_F, \Delta_H)$:

$$(1 + \Delta_F)c'_F(F(\Delta_F, \Delta_H)) = w - R_L(A, L(\Delta_F, \Delta_H)) - \beta\mathbb{E}V_L(A', \sigma', L(\Delta_F, \Delta_H)) \tag{A8}$$

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

$$\frac{d\Delta_F}{d\Delta_H} = \frac{-\left[R_{LL}(A, \sigma', L) + \beta\mathbb{E}V_{LL}(A', \sigma', L)\right]\frac{dL}{d\Delta_H}}{c'_F(0) + \left[R_{LL}(A, \sigma', L) + \beta\mathbb{E}V_{LL}(A', \sigma', L)\right]\frac{dL}{d\Delta_F}} < 0$$

Thus, the slope in the (Δ_F, Δ_H) plane is negative. Since equation (A8) implies that F decreases with Δ_H, case 1, where $F > 0$ and $H > 0$ lies below the frontier and case 3, where $F = 0$ and $H > 0$ lies above the frontier. This proves 1a and 1b in section 5.4.

Assume now that subsidies are conditional on net job creation for all jobs created above the threshold employment growth rate $\bar{g}$. The value function of the firm reads

$$V(A, \sigma, L_{-1}) = \max_{(H,F)} R(A, L) - wL + \sigma \max\left[L - (1 + \bar{g})L_{-1}, 0\right] - c_H(H) - c_F(F)$$
$$+ \beta\mathbb{E}V(A', \sigma', L)$$

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

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

$$R_L(A, L) - w + \sigma - c'_H(H) + \beta\mathbb{E}V_L(A', \sigma', L) + \lambda_H = 0 \tag{A9}$$
$$-R_L(A, L) + w - c'_F(F) - \beta\mathbb{E}V_L(A', \sigma', L) + \lambda_F - \bar{\lambda}_F = 0 \tag{A10}$$

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

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

and by the law of motion of employment (A1). This is the same system of equations as in case 3 when there are hiring subsidies. This proves 2 in section 5.4.

A.2   Computation of the cost-ratio

Since our empirical evaluation concludes that *zéro charges*, which is a hiring subsidy non conditional on net job creation, did not induce firms to strategically increase churning, let us focus on this case henceforth.

When subsidies are related to net job creation above the employment growth rate threshold $\bar{g}$, the amount of subsidies paid to firm i that gets the subsidy σ per net job created is

$$\sigma\left[L_i(\sigma) - (1 + \bar{g})L_{i,-1}\right], \tag{A11}$$

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where $L_i(\sigma)$ denotes the employment level associated with the subsidy σ.

When the subsidy is non conditional on net employment growth, the amount of subsidies paid to firm i

$$\sigma H_i(\sigma) = \sigma \left[L_i(\sigma) - L_i^* + H_i^* + S_i(\sigma) - S_i^* \right]. \qquad (A12)$$

In this formula, the amount of subsidy paid to firm i is decomposed in three components. First, the creation of new jobs induced by the subsidy, $L_i(\sigma) - L_i^*$; second, the eligible hires that would occured absent the subsidy, $H_i^* \geq 0$; third, the increase in separations induced by the hiring subsidy, $S_i(\sigma) - S_i^*$. It is worth noting that the increase in separations should not comprise the separations of workers hired during the period, because these workers benefit, by definition, from the subsidy. When such separations induce new hires, they do not raise the amount of subsidy paid to the firm. Accordingly, $S_i(\sigma) - S_i^*$ takes into account only the separations made to replace non-subsidized workers with subsidized workers. Since we have found that he hiring subsidy did not induce firms to increase labor turnover in order to benefit from the subsidy, we can set this term to zero.

From the theoretical model we know that $L_i(\sigma)$ takes the same value in equations (A11) and (A12) for all firms where $L_i(\sigma) \geq (1+\bar{g})L_{i,-1}$. Comparison of equation (A11) and (A12) shows that this implies that the amount of subsidy paid to firm i is smaller when the subsidy is conditional on net job creation than when the subsidy is non conditional on net job creation.

The cost per job created in firm i when subsidies are conditional on net job creation is

$$\frac{\sigma \left[L_i(\sigma) - (1+\bar{g})L_{i,-1} \right]}{L_i(\sigma) - L_i^*} = \sigma + \frac{\sigma \left[L_i^* - (1+\bar{g})L_{i,-1} \right]}{L_i(\sigma) - L_i^*} = \sigma + \frac{\sigma L_{i,-1}}{L_i(\sigma) - L_i^*}(g_i - \bar{g})$$

where $g_i = \left(L_i^* - L_{i,-1} \right)/L_{i,-1}$ is the growth rate of employment absent the subsidy. Let us denote by $J(\bar{g})$ the number of firms that benefit from the subsidy conditional on net job creation, i.e. such that $L_i(\sigma) \geq (1+\bar{g})L_{i,-1}$. Summing over all firms i that benefit from the subsidy conditional on net job creation, the average cost per job created is

$$\frac{1}{J(\bar{g})} \sum_i \left[\sigma + \frac{\sigma L_{i,-1}}{L_i(\sigma) - L_i^*}(g_i - \bar{g}) \right]. \qquad (A13)$$

Let us assume that the term $\frac{L_i(\sigma) - L_i^*}{\sigma L_{i,-1}}$ is identical for all firms that benefit from the subsidy. Denote by $\tilde{\delta}$ this value. Since we are in the case where the impact on hires and employment of subsidies conditional on net employment growth is identical for both types of subsidy, the term $\tilde{\delta}$ can be computed from our estimation of the impact of the subsidy non conditional on net job creation. By definition, our difference-in-differences estimate of the impact of the hiring subsidy on employment is

$$\delta = \mathbb{E}\left[\frac{L_i(\sigma) - L_i^*}{L_{i,-1}} \right],$$

where $\mathbb{E}$ denotes the expectation operator applying to all T firms of the treatment group and L_{-1} denotes total lagged employment of firms of the treatment group. Assuming that the employment of firms of the treatment group that did not benefit from the subsidy (the non compliers) has not been impacted by the subsidy, we have

$$\delta = \frac{1}{T} \sum_{i \in \mathcal{C}} \frac{L_i(\sigma) - L_i^*}{L_{i,-1}} = \sigma s \tilde{\delta},$$

where $\mathcal{C}$ stands for the set of compliers and s for the share of compliers in the treatment group. This equation implies, together with the definition of $\tilde{\delta}$, that $\frac{L_i(\sigma) - L_i^*}{L_{i,-1}} = \delta/s$ for all i belonging to the set of

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compliers. Using this relation, equation (A13) allows us to write the average cost per job created of the hiring subsidy conditional on net job creation as follows:

$$\sigma \left(1 + \frac{\frac{1}{J(\bar{g})} \sum_i (g_i - \bar{g})}{\delta} \right). \tag{A14}$$

The average cost per job created depends on the amount of the subsidy and on two other terms. First, δ, which measures the impact of the subsidy on the average employment growth rate of the treatment group of *zero charges*. Second, $\frac{1}{J(\bar{g})} \sum_i (g_i - \bar{g})$, which is the average gap between the employment growth g_i absent the subsidy and the growth rate threshold $\bar{g}$ for all firms of the treatment group of the hiring subsidy conditional on net job creation.

This cost can be compared with that of subsidies non conditional on net job creation. The cost per job created when the subsidy is non conditional on net job creation, which is obtained from equation (A12), is

$$\sigma \left[1 + \frac{1}{\delta} (\eta + g) \right],$$

where g denotes the average employment growth rate of the treatment group absent the subsidy and η the average hiring rate absent the subsidy. Therefore, the cost ratio per job created between the subsidy conditional on net job growth and the non conditional subsidy is defined by equation (2) in the text.

The number of jobs created by *zero charges* is δL_{-1} where L_{-1} denotes total employment of the treatment group on 30 November 2008. Assuming that the take-up rate of hires eligible to the subsidy is identical for both types of subsidy, the number of jobs created by the hiring subsidy conditional on net job creation above the employment growth threshold $\bar{g}$ is $\delta L_{-1}(\bar{g})$ where $L_{-1}(\bar{g})$ stands for total employment on 30 November 2008 of firms with employment growth in 2009 above the threshold $\bar{g}$ absent the subsidy (i.e. their observed employment growth in 2009 minus the impact of the subsidy on their employment growth, equal to δ).

A.3   Supplementary tables

Table 9: Number of eigible/ ineligible fimrs in the sample in 2008

Nb employees	Number of firms		Number of employees (end of 2008)	
	below 10	above 10	below 10	above 10
all	$934,879$	$155,186$	$3,301,440$	$10,900,920$
6-10 and 10-14	$91,763$	$40,828$	$818,602$	$553,835$
+ excl. temporary temp, associations & agriculture	$83,661$	$36,649$	$718,745$	$484,500$
+ trimming 1% extreme growth	$76,705$	$33,504$	$643,307$	$432,931$
+Excluding missing control variables	$64,587$	$27,403$	$555,197$	$362,690$

Source : DADS (Insee). Note: The number of employee (end of 2008) is the total number of employees per firm at the end of December, with a contract duration of at least one month.

Table 10: Difference-in-differences estimates.

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.008***	.008***	.008***
	(.002)	(.002)	(.002)
Hours growth	.010***	.009***	.009***
	(.002)	(.002)	(.002)
Hiring rate	.015***	.014***	.018***
	(.003)	(.003)	(.003)
Separation rate	.007**	.007***	.010***
	(.003)	(.003)	(.003)
Survival rate	-	-	-
Nb. Observations	$359,862$	$359,862$	$184,193$

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

Table 11: Difference-in-differences estimates.

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.008***	.007***	.006**
	(.002)	(.002)	(.003)
Hours growth	.010***	.009***	.009***
	(.002)	(.002)	(.003)
Hiring rate	.013***	.012***	.017***
	(.003)	(.003)	(.004)
Separation rate	.005	.005*	.011***
	(.003)	(.003)	(.004)
Survival rate	.000	.000	−.001
	(.001)	(.001)	(.001)
Nb. Observations	365, 740	365, 740	187, 137

Source : DADS (Insee). Note : this table presents our difference-in-differences estimates for different outcomes (lines) and different specifications (columns) when firms are weighted according to their size as measured by the number of full time equivalent employees in the previous year. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in previous year (average from 1 December to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t 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.