

Cahuc, Pierre; Carcillo, Stéphane; Le Barbanchon, Thomas

Working Paper

The Effectiveness of Hiring Credits

IZA Discussion Papers, No. 11248

Provided in Cooperation with:

IZA – Institute of Labor Economics

Suggested Citation: Cahuc, Pierre; Carcillo, Stéphane; Le Barbanchon, Thomas (2017) : The Effectiveness of Hiring Credits, IZA Discussion Papers, No. 11248, Institute of Labor Economics (IZA), Bonn

This Version is available at:

<https://hdl.handle.net/10419/177052>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

DISCUSSION PAPER SERIES

IZA DP No. 11248

The Effectiveness of Hiring Credits

Pierre Cahuc
Stéphane Carcillo
Thomas Le Barbanchon

DECEMBER 2017

DISCUSSION PAPER SERIES

IZA DP No. 11248

The Effectiveness of Hiring Credits

Pierre Cahuc

Crest-Ensaie, Ecole Polytechnique, IZA and CEPR

Stéphane Carcillo

OECD, Sciences Po and IZA

Thomas Le Barbanchon

Bocconi University, CEPR, IGER and IZA

DECEMBER 2017

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ABSTRACT

The Effectiveness of Hiring Credits*

This paper analyzes the effectiveness of hiring credits. Using comprehensive administrative data, we show that the French hiring credit, implemented during the Great Recession, had significant positive employment effects and no effects on wages. Relying on the quasi-experimental variation in labor cost triggered by the hiring credit, we estimate a structural search and matching model. Simulations of counterfactual policies show that the effectiveness of the hiring credit relied to a large extent on three features: it was non-anticipated, temporary and targeted at jobs with rigid wages. We estimate that the cost per job created by permanent hiring credits, either countercyclical or time-invariant, in an environment with flexible wages would have been much higher.

JEL Classification: C31, C93, J6

Keywords: hiring credit, labor demand, search and matching model

Corresponding author:

Pierre Cahuc
Laboratoire de Macroéconomie
CREST
15 Boulevard Gabriel Péri
92245 Malakoff Cedex
France
E-mail: cahuc@ensae.fr

* This paper builds on a previous working paper, which circulated under the title: “Do Hiring Credits work in Recessions? Evidence from France”. We thank Jérôme Adda, Peter Ganong, Bertrand Koebel, Francis Kramarz, Camille Landais, Claire Lelarge, Benoit Ourliac, Barbara Petrongolo, Steve Pischke, Fabien Postel-Vinay, Roland Rathelot, Jean-Marc Robin, Alexandra Roulet, Sebastien Roux, Alain Trannoy, seminar participants at CREST-ENSAE, the OECD, the University of Maine, the University of Helsinki, the London School of Economics, the University of Lausanne, the University of Strasbourg, and the French Ministry of the Economy and Finance, for their valuable remarks. We also thank the French Public Employment Agency, Pôle emploi, the French Directorate for Employment and Training, and the French National Statistical Institute, Insee, for data access. The authors are solely responsible for any errors or omissions. The opinions expressed and arguments employed herein are solely those of the authors and do not necessarily reflect the official views of the OECD or of its member countries. Stephane Carcillo was employed by the Ministry of the Economy, Finance and Employment when the hiring credit analyzed in this paper was implemented. Financial supports from the Labex ECODEC No. ANR-11-LABEX-0047 and from the European Research Council through the grant Finimp-macro (n. 283483) are gratefully acknowledged. The data were accessed using the Secure Data Access Center - CASD, supported by the “Investissements d’Avenir” program (reference: ANR-10-EQPX-17).

1 Introduction

Hiring credits have been used in the United States and in a number of European countries to counteract the employment effects of the 2008-2009 recession.¹ Despite this wide use, many economists think that hiring credits are probably useless during recessions, when aggregate demand is insufficient relative to labor and other resources available in the economy.² In fact, there is very little empirical evidence about the effects of hiring credits. Evidence on federal programs in the US dates back to the 80s (Perloff and Wachter, 1979, Bishop 1981), and the only recent evidence concerns hiring credits implemented at the U.S. states level (Neumark and Grijalva, 2015). We seize the opportunity of the natural experiment induced by the 2009 French hiring credit to highlight the effectiveness of such policies. Reduced-form estimates of the effects of the French program relying on comprehensive administrative data show that the hiring credit has had a significant impact on employment. Then, we use quasi-experimental variations induced by the program to estimate key structural parameters of a search and matching model. Simulations of this model show that the cost per job created is very sensitive to the type of hiring credit – temporary vs. permanent, countercyclical vs time-invariant,³ generalized to all hires vs. targeted at a small subset of hires –, and to the economic environment – rigid vs. flexible wages.

The French hiring credit, announced on 4 December 2008, relieved firms from social contributions on new hires until 31 December 2009. The program was arbitrarily restricted, for budgetary reasons, to firms with fewer than 10 employees, and to low-wage workers. We show that these restrictions and other features of the program ensure that its implementation can be considered as a natural experiment. Moreover, only a small fraction of hires were actually eligible for the hiring credits so that the program did not trigger spillover effects.

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

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

³By definition hiring credits provide subsidies to new jobs for a limited period of time at the beginning of the job spell. Temporary hiring credits are one-off schemes that provide these subsidies during specific periods, whereas permanent hiring credits provide them permanently. Permanent hiring credits can be either time-invariant or countercyclical, i.e. provided in slowdowns only. Neumark and Grijalva (2015) report that 99 of the 147 hiring credits recorded in the United States over the period 1970-2012 are permanent.

Our evaluation of the French hiring credit relies on two identification strategies. The difference-in-differences strategy compares the evolution of small firms (between 6 and 10 employees) and medium-size firms (between 10 and 14 employees) from November 2008, just before the program’s inception, until November 2009.⁴ The IV strategy compares employment pool \times sector cells with high and low shares of subsidized hires. We use the share of low-wage workers from 2006 to 2008 as an instrument for the share of subsidized hires. Both strategies yield converging results. The French hiring credit significantly increased by 0.8 percentage point the growth rate of targeted firms. Moreover, the employment effects are concentrated as expected on eligible jobs, i.e. low-wage jobs. The impact of the hiring credit emerged quickly: hires and employment began to rise three months after the introduction of the credit. The evolution of hours worked is similar to that of employment, meaning that firms did not substitute hours of new workers benefiting from the hiring credit for those of incumbent employees. We find no increase in wages associated with the hiring credit, and firms did not increase layoffs in order to hire workers at lower cost. Year placebo tests, and robustness analysis varying the firms’ size bandwidth selecting the estimation sample, confirm our results. Comparing ineligible medium-sized firms in labor markets with a high or low fraction of subsidized hires, we show that the hiring credit did not trigger equilibrium effects, either through wage adjustments or effects on labor market tightness.

Building on these reduced-form analyses, we use quasi-experimental variations in labor cost induced by the program to estimate a structural search and matching model. We show that the variations in the coverage of the hiring credit and in the tightness across local labor markets allow us to identify two key parameters: the elasticity of the marginal revenue (with respect to labor) and the vacancy posting cost. The time-variation in tightness and job finding rates within local labor markets before the 2009 French hiring credit allow us to identify a third key parameter, i.e. the elasticity of the matching function with respect to the number of job-seekers, as in Borowczyk-Martins et al. (2013).

Introducing directed search with wage posting into the model, in line with Moen (1997) we show, in the spirit of the sufficient statistic approach (Chetty, 2009), that the three structural parameters estimated above are sufficient to define the cost per job created by

⁴Consistent with the program, we split firms according to their size computed from November 2007 to November 2008, before the program was announced.

hiring credits in different cases: exogenous vs. endogenous wage, temporary vs. permanent hiring credit (either countercyclical or time-invariant),⁵ hiring credit generalized to all firms vs. hiring credit targeted at a small subset of firms (sufficiently small to have no impact on the labor market tightness). Using our previous estimates of the structural parameters, we compute the cost per job created by these counterfactual policies. In the baseline scenario, which corresponds to the 2009 French hiring credit, the *gross* cost per job created is around one fourth of the average annual wage. To compute the cost per job created *net* of savings on social benefits, we exploit a survey that provides information about the characteristics of the beneficiaries of the hiring credit. It turns out that the 2009 hiring credit has been very effective, since the net cost per job created is about zero.

Nevertheless, our simulations suggest that the effectiveness of hiring credits is contingent on particular circumstances. In line with Kitao et al. (2010) and Kaas and Kircher (2015), we find that the (one-off, non-expected) temporary nature of hiring credits plays a key role: permanent hiring credits, even targeted at a small subset of firms, create jobs at a cost multiplied by a factor of four compared with one-off non-anticipated temporary credits available for new hires during one year.⁶ Hiring credits generalized to all firms would have featured only a slightly higher gross cost per job created than a similar hiring credit targeted at a small subset of firms, as long as wages are exogenous. This result, obtained in a context of high unemployment rates, means that congestion effects induced by the hiring credit are too small to induce significant increases in recruitment costs. When wages are flexible, the cost per job created by temporary hiring credits generalized to all firms is only slightly higher than the cost per job created by temporary hiring credits targeted at a small subset of firms, because one-off temporary increases in labor market tightness induced by temporary hiring credits have little impact on the expected gains of unemployed workers, and therefore on wages. However, permanent economy-wide hiring credits induce permanent increases in labor market tightness, and thus have a stronger impact on the expected gains of unemployed workers and then on wages. We find that

⁵In this paper, the cost per job created by a temporary hiring credit, set out for one year, corresponds to the monthly cost necessary to create one supplementary job at the one-year time horizon, whereas the cost per job created by a permanent hiring credit, set out without any foreseen time limit, is the monthly cost necessary to create one supplementary job permanently, i.e. on an infinite time horizon.

⁶Note that permanent hiring credits are different from wage subsidies, as, for a given worker, the credit vanishes after a certain tenure in the firm - one year in our simulations.

the reaction of wages multiplies by about 2 the cost per job created by generalized and permanent hiring credits. This casts doubt about the effectiveness of permanent hiring credits, which are frequent (Neumark and Grijalva 2015, OECD, 2010).

Our paper contributes to the empirical debate on the effectiveness of hiring credits. It is related to Neumark and Grijalva (2015) who analyze state hiring credits in the US.⁷ Using a difference-in-differences strategy across US states, they point to moderate positive employment effects of credits targeting the unemployed during recessions. To our knowledge, our paper is the first empirical evaluation of a temporary hiring credit relying on comprehensive firm-level administrative data. The richness of the data and the quasi-experimental situation induced by the French hiring credit allow us to evaluate the impact of the hiring credit, with proper identification strategies, on a wide range of outcomes not available in previous studies. Moreover, our paper is also the first empirical evaluation of a temporary hiring credit in Europe. European empirical evidence mostly concerns the effects of permanent payroll tax reductions. As both hires and incumbents are eligible for these payroll tax reductions, they may imply large deadweight losses, and their estimated effects only partially inform us about the effects of hiring credits.⁸ In Europe, hiring subsidies may also be part of broader strategies to activate the unemployed. They are frequently coupled with job search assistance programs, which makes it difficult to distinguish their impact, as in Blundell et al. (2004).⁹

We also contribute to the literature which builds bridges between quasi-experimental or experimental data and structural estimation, i.e. Attanasio et al. (2012), Ferrall (2012), Gautier et al. (2012), Galiani et al. (2015), Lise et al. (2015), Todd and Wolpin (2006) and Wise (1985). Our approach features both internal validity and external validity. The source of the identification of the key structural parameters is quasi-experimental and

⁷Our work is also related to the evaluations of the New Job Tax Credit (NJTC) implemented in the US during the 70s by Perlof and Wachter (1979) and Bishop (1981). Both studies find positive effects of the program, but their analyses suffer from the economy-wide implementation of the NJTC, which makes it difficult to define a proper counterfactual control group.

⁸Indeed, Goos and Konings (2007), Huttunen et al. (2013), Bennismarker et al. (2009), Egebark and Kaunitz (2013) and Skedinger (2014) find rather small employment effects of permanent payroll tax reduction.

⁹A notable attempt to distinguish the relative effectiveness of the different components of activation strategies is Sianesi (2008). For Sweden in the 1990s, she finds that entering a temporary job subsidy program rather than searching further in open unemployment increased employment rates soon after the program ended.

makes use of a well-defined policy shock. Thus we gain internal validity. Then simulations of the underlying economic model enable us to discuss the external validity of our reduced-form results. This framework is useful to quantify congestion externalities in search and matching models (Beaudry et al., 2012, 2014; Crépon et al., 2013; Gautier et al., 2012; Lalive et al., 2013). It is closely related to Beaudry et al (2014) who show that the wage elasticity of employment is larger in absolute value at the industry-city level than at the city-level. They argue that the effects of wage shocks at the city-level are damped by congestion externalities induced by the reaction of the city-level labor market tightness. We also find that congestion externalities play a very important role through wages. Congestion externalities exert an upward pressure on wages that significantly reduces the employment effects of economy-wide hiring credits compared with hiring credits targeted at a small subset of firms.

The paper is organized as follows. Section 2 describes the hiring credit scheme (*zéro charges*) implemented in France in 2009. Section 3 presents the data, descriptive statistics and the empirical strategy of the reduced-form approaches. The difference-in-differences estimates are presented in Section 4. The results of the IV estimation are presented in Section 5. Section 6 shows that the French program did not trigger equilibrium effects. Section 7 proceeds to the structural estimation of the search and matching model and evaluates the cost per job created by hiring credits in different environments. The last section concludes.

2 Institutional background

The *zéro charges* (zero contributions) measure was announced by the French President on 4 December 2008. According to the original announcement, any hire (or temporary contract renewal) of a low-wage worker in a firm with fewer than 10 employees occurring from the date of the announcement until 31 December 2009 could benefit during the same year from an employer social contribution relief.¹⁰ The relief is maximal for workers with an hourly remuneration at the minimum wage level (8.82 euros in 2009). With *zéro charges*, employers do not pay any social contribution at the minimum wage level. The

¹⁰The new relief is in addition to the existing general social contribution reduction on low wages called the *Fillon reduction*, which has prevailed since the 1990s and concerns all firms in the private sector.

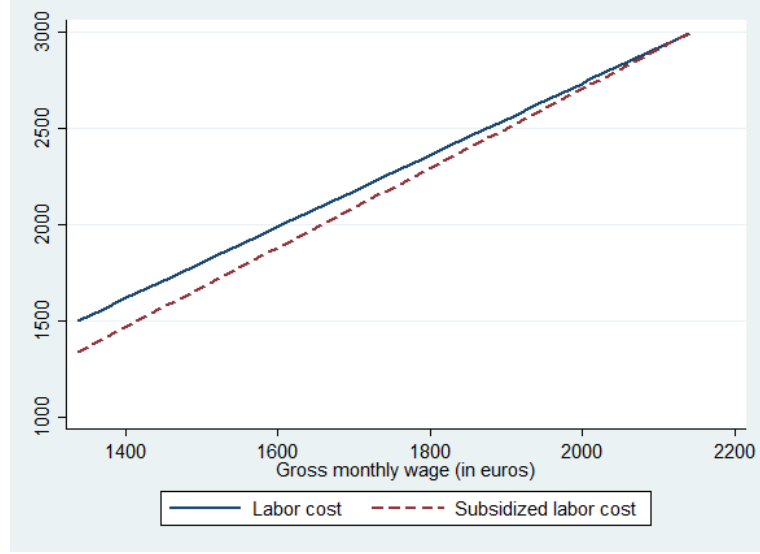


Figure 1: The hiring credit schedule.

Note: The horizontal axis reports the monthly wage (in euros) net of employer social contributions of a full time worker (in 2009, the monthly minimum wage was 1,338 euros in gross terms, i.e. including employees' social contributions). The vertical axis reports the monthly labor cost. The continuous line displays the labor cost without the hiring credit. The dotted line shows the labor cost with the hiring credit.

relief then decreases as the hourly wage level rises up to 1.6 times the minimum wage. Figure 1 shows that the hiring credit reduces the labor cost by 12% for a full-time worker paid at the minimum wage. The maximum amount of the hiring credit over 12 months represents 2,400 euros. When the wage is 30 percent above the minimum wage, the subsidy rate represents only 4 percent of the labor cost.

Before the first announcement, the policy was not anticipated because it was kept secret as part of a stimulus package to be disclosed all at once.¹¹ This is illustrated by Figure 2 which shows that Google searches for the item “hiring subsidy” (*aide embauche*) started to increase in December 2008, once the announcement for the program was made. There is no Google search for the item *zéro charges* before early 2009.

¹¹For instance the newspaper *Les Echos*, describes in an article entitled “Le gouvernement envisage d’accélérer ses paiements et remboursements aux entreprises”, published on 27 November 2008, all potential measures that the President Sarkozy was supposed to announce the following week at the Press conference of 4 December 2008. The hiring credit is not mentioned in this article. On 4 December 2008, the article entitled “Sarkozy dévoile un plan de 26 milliards d’euros pour relancer l’économie”, summarizing the contents of the press conference, does mention the hiring credit.

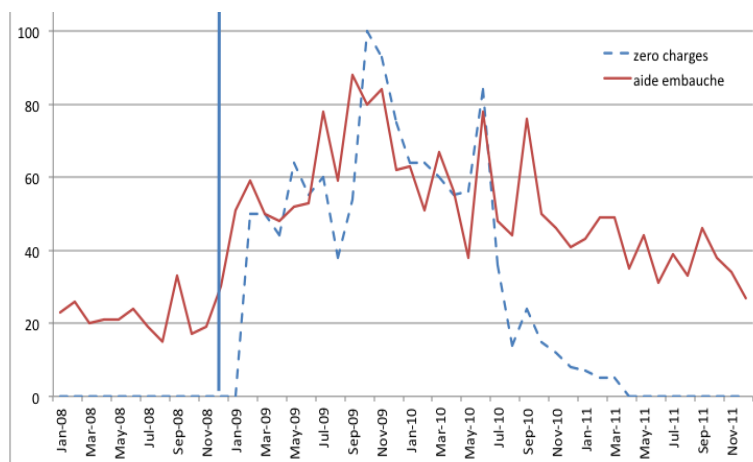


Figure 2: Results of Google search for the policy name.

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

The practical details of the hiring credit were rapidly set out in a decree published on 20 December 2008. To start with, only firms and associations belonging to the private sector could get the hiring credit. Firms and associations had to request the *zéro charges* relief for each hire separately, filling out a one-page form and attaching the labor contract. The claim had to be sent to the French Public Employment Service (Pôle emploi) which reimbursed for the social contributions payments on eligible hires at the end of each quarter.

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

Third, only entities with fewer than 10 full-time equivalent employees¹² on average

¹²The size criteria are very precise and follow the usual rules set in the labor code (see cerfa n° 13838-01). Only ordinary employees are kept in the computation of the size (thus excluding apprentices and

between January and November 2008 could apply. Hence, the period used to define the size criteria ends just before the announcement of the policy, on 4 December 2008. A growing firm reaching 10 or more employees over the year 2009 could still continue to receive subsidies and apply for new hires until the end of 2009. This meant that the size criteria could not be manipulated by firms wishing to benefit from the hiring credit.

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

On 16 November 2009, the policy was extended to hires occurring up to 30 June 2010.¹³ On this occasion, the duration of the hiring credit was extended for up to 12 months from the hiring date, instead of the cutoff date of 31 December 2009 for the initial scheme. This new rule was also applicable to hires made in 2009 before the announcement of the extension, and which already benefited from *zéro charges*. Firms below the average of 10 full-time equivalent employees from January 2009 to December 2009 were also eligible for the extended program for their new hires in 2010. Hence it is more challenging to study the effects of the policy in 2010, as some firms treated in 2009 may not have been able to apply in 2010, because eligibility for the extended period was then based on the average size over 2009. As a consequence, we focus on outcomes in 2009.

The hiring credit was initially part of a wider array of policies - the stimulus package - designed to cope with the 2008-2009 crisis. Within that array, this is the only item specifically targeted at small firms, and the only item directly altering the labor cost. The exact size threshold of 10 employees was mostly determined by the government budget constraints. Broadly speaking, there were no other explicit legal changes in this period

temporary agency employees and those hired as part of a labor market program). The size is computed as the average of the end-of-month number of employees from January to November 2008. Fixed-term workers contribute *pro rata temporis* their number of days present in the firm over the month. This means that fixed-term workers hired on the 15th of the month working full-time represent 0.5 employees. However, workers hired on permanent contracts are counted as 1 employee during the month no matter what day of the month they were hired on. All wage-earners working part-time, either on fixed-term or permanent contracts, are accounted *pro rata temporis* their regular number of hours during the month, excluding overtime hours. For instance, wage-earners working mornings only are counted as 0.5 employee.

¹³The first announcement about a possible extension was made in the end of September 2009. This fact and the corresponding google search data shown on Figure 2, suggest that firms did not expect that the credit would be extended before the last quarter of 2009.

that exerted a varying impact on firms with less or more than 10 employees.

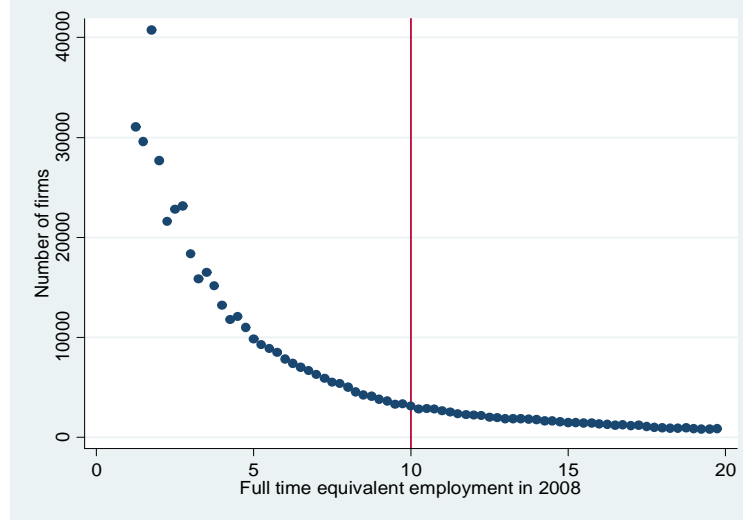


Figure 3: Firm size density. Source DADS.

While there are some minor discontinuities at the 10-employee threshold in the French legislation,¹⁴ we do not see any accumulation of firms just below the threshold (see Figure 3). This suggests that the changes in the labor cost or in the labor regulations at the threshold are not significant, nor salient enough to lead firms to sort. Such a sorting might have meant that firms below and above the threshold were reacting differently to the business cycles. This is in line with Ceci-Renaud and Chevalier (2010) who do not find any bunching at the 10-employee threshold. This contrasts with the accumulation of firms just below the 50-employee threshold, as reported by Gourio and Roys (2014) and Garicano et al. (2013). The difference in patterns around the 10 and 50-employee threshold is probably due to the greater change in costs at the 50-employee threshold and its greater saliency.¹⁵ As Garicano et al. (2013, p. 14) put it, “Although there are some

¹⁴An increase in the contribution rate for continuing vocational training of 0.55% to 1.05%, an obligation of monthly payments of social security contributions (instead of quarterly payments), an obligation for payment of transport subsidies and the loss of the possibility of a simplified balance sheet.

¹⁵There are more substantial discontinuities at the 50-employee threshold in the French legislation, notably the possibility for employee unions of designating delegates, various obligations in the field of vocational training, the obligation to set up a works council, the obligation to set up a separate committee on health, safety and working conditions, the obligation to set up a “profits participation fund” in favor of employees, the obligation to set up “social plans”, i.e. time-consuming and costly procedure in the event of redundancy involving 9 or more employees, and the loss of the possibility of a simplified presentation of the financial statements.

regulations that bind when a firm (or less often, a plant) reaches a lower threshold such as 10 or 20 employees, 50 is generally agreed by labour lawyers and business people to be the critical threshold when costs rise significantly”.

3 Data and empirical strategy

3.1 Data

We use administrative data from two main distinct sources:

- the *Déclarations Administratives de Données Sociales* (DADS) built by the French Statistical Institute (INSEE) from the social contributions declarations of firms. Each year firms declare the employment spells, the number of hours worked, and the associated wages for each worker.
- the administrative file produced by the French Public Employment Service (*Pôle emploi*) which administered the payment of the subsidy, designated as the “hiring credit file”. It contains information on the firms which enrolled in the *zéro charges* program, the level of the hiring wage, and the exact amount and duration of the subsidy received.

The DADS cover about 85% of French wage earners. Civil servants from the French central, regional and local administrations (general government) and workers from the public health care sector or employed by householders (e.g. for house-keeping or child care) do not appear in this employment register (until 2009). We append the employment registers from 2005 to 2009,¹⁶ creating a panel of firms.¹⁷ We restrict the sample to firms in the for-profit private sector and we drop the agricultural sector as well as associations. We also drop workers in temporary help agencies, as we do not know in which firms they actually work, as well as the 1% of firms with the highest employment growth rates in the sample. All relevant information pertaining to firm size, the number of hires, separations,

¹⁶The specification concerning the type of labor contract, either fixed-term or open-ended, is not available before 2005. Since the type of contract is used to compute the number of full time equivalent workers, as explained in footnote 12, we cannot use the DADS before 2005.

¹⁷There is no permanent identifier for individual workers. Our data are not a panel of individual workers.

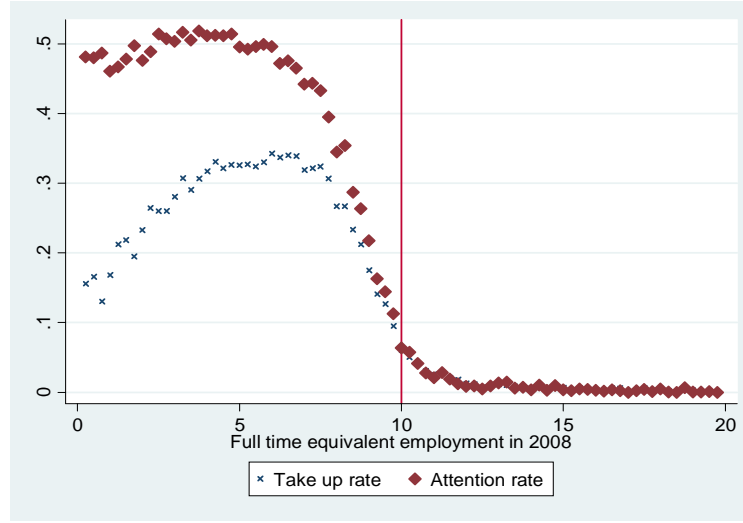


Figure 4: Fraction of firms and of hires that benefited from the hiring credit by firm size in 2009.

Note : The take-up rate is the share of firms below 10 employees benefiting from the hiring credit in 2009; the attention rate is the share of hires with wages below 1.6 times the minimum wage lasting one month or more and which were subsidized in 2009. The number of full-time equivalent employees is measured over the first 11 months of 2008.

the wage levels and the duration of contracts is taken from the DADS data set which describes the universe of firms relevant to our evaluation. We also compute the eligibility condition based on the size threshold (full-time equivalent) from the employment register, as the Public Employment Service computed it for the subsidized firms only.

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

To the extent that, as discussed above, firms were not able to manipulate their size to meet the eligibility criterion, the drop in the attention rate before the threshold of 10 employees and the positive fraction of firms from 10 to 12 employees benefiting from the hiring credit are likely the consequences of measurement error. The eligibility criterion is difficult to measure precisely in the employment register at our disposal. In particular, according to the legal rules, workers hired on permanent contracts are considered to be present in the firm from the beginning of the month, even if they have been hired during the month. Since we only observe the type of contract at the end of the year for every worker, we are unable to know whether workers have been hired on permanent or temporary contracts because temporary contracts may have been converted into permanent contracts. Another reason could be that computing the eligibility criterion is a complex task, especially for small firms. Only ordinary employees are kept in the size computation, excluding apprentices and diverse categories of employees benefiting from other subsidies; employees contribute *pro rata temporis* but overtime hours are not taken into account. These features of the eligibility criteria may induce firms to overestimate their size and to refrain from claiming *zéro charges*. The resulting absence of discontinuity in the take-up rate prevents us from using a regression discontinuity design.

3.2 Empirical strategy of the reduced-form approach

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

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

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

Hiring credits aim at increasing employment through their effect on hires. However, it is possible that firms benefit from important amounts of hiring credits while the effects on net employment are negligible. Becker (2010) and Posner (2010), reacting to the Hiring Incentives to Restore Employment (HIRE) Act passed in the US in 2010, argued that it will increase churning and wages with very little effect on employment. In our context,

churning is potentially an important concern to the extent that worker flows in excess of those strictly necessary to achieve a given change in employment are large in France (Abowd et al., 1999, Cahuc et al., 2014).

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

In what follows, we estimate the impact of the hiring credit on employment, wages, hours of work, hires and separations, using two different identification strategies: a difference-in-differences strategy and an IV strategy. The difference-in-differences strategy contrasts the evolution of employment in firms with fewer than 10 employees and firms with more than 10 employees before and after the reform was implemented. The IV strategy contrasts the evolution of employment in employment pool \times sector cells with high or low treatment intensity after the reform was implemented. We instrument the treatment intensity by the share of low-wage workers in the cell from 2006 to 2008 (before the reform).

4 Difference-in-differences

This section presents the difference-in-differences econometric model and our main results on the effects of the hiring credit on employment, hours worked, wages, hires and separations.

4.1 Econometric model

We analyze yearly cohorts of firms. We select, for each cohort t , firms whose size criterion in year $t - 1$ is around the cut-off (that is 10 full-time equivalent employees, calculated as the average of end-of-month *pro-rata temporis* headcounts between January and November of year $t - 1$) and estimate the following difference-in-differences model:

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

where Y_{it} is the outcome of firm i in period t , Z_{it} an eligibility dummy equal to 1 if the firm size in period $t - 1$ is below 10, D_t a dummy for year 2009 when subsidies can be

claimed, X_{it} a vector of covariates listed in the next section and in the note of table 2. δ is our parameter of interest. It captures the differential evolution of the group targeted by the hiring credit. It can be interpreted as an Intention-To-Treat parameter. Accordingly, we refer, in this section, to firms with fewer than 10 employees in year $t - 1$ as our “treatment” group, even if they do not claim the hiring credit. Note that because we define our eligibility dummy for every year, the treatment effect estimate is robust to potential mean-reversion bias that could occur if the definitions of the control and treatment groups had been based on the size of firms in 2008 only.¹⁸

In the benchmark estimations, the bandwidth goes from 6 (included) to 14 (excluded) full-time employees in the previous year.¹⁹ In Table 1, we report characteristics of our 2009 cohort. These characteristics are measured in 2008. In the first three columns, we compare small and medium size firms. Small (i.e. eligible) firms operate less frequently in manufacturing industry and slightly more often in retail, transport and merchant services than non-eligible medium size firms. They are slightly more frequently located in the Parisian area, and less frequently in the North West and the North East parts of France. Almost half of small firms have sales of less than 2 million euros, while four medium-size firms out of five exceed that mark. Small firms are also younger: 13 percent have existed for less than 5 years vs. 10 percent for medium-size firms. The composition of the workforce (in 2008) differs between small and medium-sized firms. Small firms have more white collar employees, while medium-sized firms have more blue collar workers. Finally, the share of low-paid workers and that of part-time workers are both higher in small firms. These variables (lagged) are included in the regressions to control for these differences.

¹⁸Let’s consider the simple difference-in-differences model, without controls, using the 2008 and 2009 cohorts of firms and with employment growth as an outcome. Then the difference between small and medium firms of the 2008 cohort writes: $\Delta = \mathbb{E} \left[\frac{L_{2008} - L_{2007}}{L_{2007}} | L_{2007} < 10 \right] - \mathbb{E} \left[\frac{L_{2008} - L_{2007}}{L_{2007}} | L_{2007} > 10 \right]$. If there is some mean-reversion bias around 10 employees, Δ would be strictly positive. Mean-reversion bias entails that the same difference using the 2009 cohort would be strictly positive even in the absence of treatment effect. However taking the difference-in-differences nets out the mean-reversion bias. Had we defined the treatment group according to the firm size in 2008, whatever the firm cohort, any mean-reversion bias would have contaminated our difference-in-differences estimate. Year placebo estimates in Appendix C.1 confirm that our definition of the treatment group is robust to mean-reversion bias.

¹⁹In Appendix C.3, we present a robustness analysis where we vary the bandwidth around the 10-employee cutoff.

Table 1: The characteristics in 2008 of eligible/ineligible and subsidized/unsubsidized firms

	Eligible	Ineligible	Diff test	Subsidized	Not subsidized	Diff test
Nb employees in 2008	6-10	10-14	p-value	6-10	6-10	p-value
Manufacturing	.153	.193	.000	.137	.160	.000
Construction	.186	.187	.474	.191	.182	.000
Retail and transport	.307	.288	.000	.319	.302	.000
Hotels and restaurants	.100	.091	.000	.143	.083	.000
Merchant services	.254	.244	.000	.208	.272	.000
Parisian area	.241	.229	.000	.154	.275	.000
North-West	.245	.254	.000	.265	.237	.000
North-East	.120	.124	.032	.128	.116	.000
South-East	.268	.264	.185	.307	.253	.000
South-West	.127	.129	.382	.146	.119	.000
Sales below 2 millions euros	.449	.204	.000	.519	.422	.000
Young firm (age below 5 years)	.134	.102	.000	.145	.129	.000
Mean share of...						
... male managers	.207	.216	.000	.161	.225	.000
... female managers	.121	.115	.000	.105	.127	.000
... male white-collar	.079	.076	.004	.093	.074	.000
... female white-collar	.210	.187	.000	.253	.194	.000
... male blue-collar	.347	.364	.000	.352	.344	.006
... female blue-collar	.036	.041	.000	.035	.036	.655
Mean share of ...						
...low-wage workers	.622	.605	.000	.712	.586	.000
...part-time workers	.256	.208	.000	.258	.255	.039
Nb. of obs.	73,042	31,893	-	20,451	52,486	-

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

4.2 Main results

The validity of difference-in-differences estimations is heavily dependent on the common trend assumption. We describe the common trend for treated firms with previous size between 6 and 10 (excluded) and control firms with previous size from 10 to 14 in Figure 5. The outcome is average employment growth in each group.²⁰ Employment is computed at the firm level. Employment in year t is defined as employment on 30 November of year t . This ensures that employment in 2008 is not influenced by the hiring credit that was announced on 4 December 2008. Let $L_{i,t}$ denote employment in firm i on 30 November of year t ; average employment growth for each group is $\frac{1}{N_t} \sum_i \frac{L_{i,t} - L_{i,t-1}}{L_{i,t-1}}$ where N_t is the number of firms in the group. Figure 5 shows that the difference in employment growth rates between the treatment group and the control group is negative and constant from 2006 to 2008. In 2009, this difference becomes positive: the growth rate of the treatment group drops by 0.9 percentage points while that of the control group drops by 1.6 percentage points.²¹ 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.

²⁰We focus on the effect of the hiring credit on the growth rate of employment rather than on the employment level for the following reason. The common trend assumption on the employment level requires identical differences in employment levels between year t and year $t - 1$ for the control and the treatment group before 2009, i.e. $\bar{L}_t^C - \bar{L}_{t-1}^C = \bar{L}_t^T - \bar{L}_{t-1}^T$ where \bar{L}_t^j stands for average employment of group j ($j = C$ for the control group and $j = T$ for the treatment group) in year $t < 2009$. We checked that this assumption is not fulfilled. This is not surprising inasmuch as the impact of productivity shocks or labor costs shocks on the employment level are expected to increase with the size of the firm. This is the case, for instance, when the wage elasticity of labor demand is constant. To see this, consider a simple static model, where the production function is $F(L)$ and the labor cost is equal to the net wage w times the labor wedge ϕ . The optimal level of employment satisfies $F'(L) = w\phi$. This equation implies that a one percent change in labor cost induces a change in employment level that is proportional to the initial employment level of the firm, i.e. $dL = L\varepsilon d\phi/\phi$, where $\varepsilon = F'(L)/LF''(L)$ denotes the elasticity of labor demand with respect to the labor cost $w\phi$. Note that taking log employment as outcome instead of the employment growth rate is valid only if the distribution of past employment is stable over cohorts of the control and the treatment groups. Running diff-in-diff on growth rates allows us to get estimates that are robust to potential changes in the distribution of past employment since the change in employment of each firm is proportional to its past size.

²¹The average employment growth is negative for the treatment group and the control group all along the period. This is because new entrants, which typically account for a significant share of employment growth, are excluded from the sample. Bear in mind that, by construction, we cannot include new entrants since we study the behavior of firms that had between 6 and 14 full time equivalent employees the previous year.

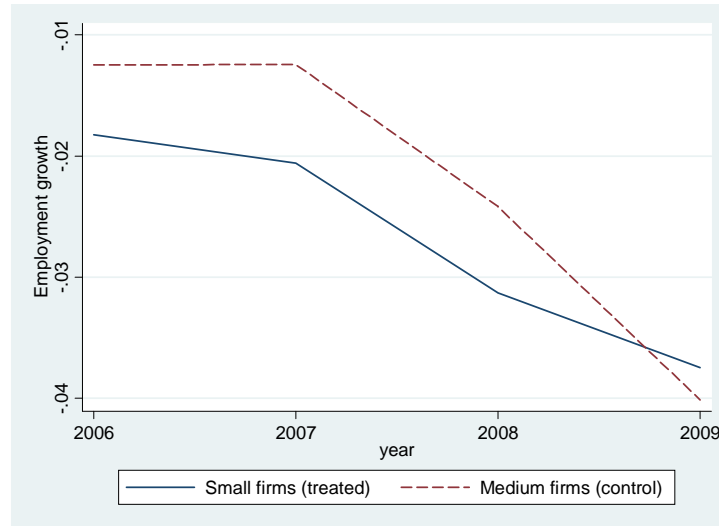


Figure 5: Average employment growth rate in firms in the treated and control groups.
 Note: Growth rate of employment between 30 November of year $t - 1$ and year t . The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in the previous year (average from 1 December to 30 November).

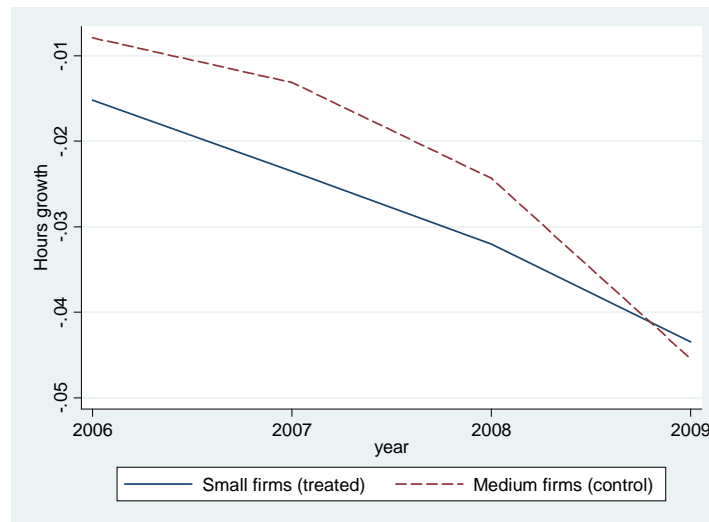


Figure 6: Average hours growth rate in firms in the treated and control groups.

Note: Growth rate of the number of hours worked within each firm between November of year t and November of year $t - 1$. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in the previous year (average from 1 December to 30 November).

In Table 2, we present our difference-in-differences estimates for different outcomes (in rows) and specifications (in columns). In column 1, our baseline sample comprises all cohorts from 2006 to 2009 without covariates. In column 2, we add covariates control which include year, sector and regions dummies, as well as their interactions, dummies for firm age, for 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). In column 3, we restrict the sample to cohorts 2008 and 2009 (to avoid potential specification errors related to underlying trends). To deal with serial correlation problems which could lead to misleadingly small standard errors (Bertrand et al, 2004), we follow the approach suggested by Cameron and Miller (2015): in addition to the non-clustered robust standard errors, we compute the cluster-robust standard errors at progressively broader levels, starting at the firm level, then at the employment pool \times sector unit level, and eventually at the employment pool level.

The results are very stable. They indicate that the hiring credit increased the employment growth rate of the treatment group by about 0.8 percentage point (column 2 of Table 2). Table 2 shows that the impact of the hiring credit on the growth of hours of work is similar to that on employment, indicating that firms did not reduce working hours on existing jobs to compensate for new hires. The last row of Table 2 shows that the hiring credit had no impact on the survival of firms, meaning that the hiring credit raised employment in surviving firms. Indeed, estimates on the subsample of surviving firms are identical to that of all firms, as shown in Table A11 in appendix.²²

Table 3 displays separately the impact of the hiring credit on eligible jobs – jobs paying below 1.6 times the minimum wage that last at least one month – and on ineligible jobs.²³ The hiring credit has a strong positive and significant impact on employment and

²²Our estimates are not weighted by firm size. This could bias our results if, for instance, the elasticity of labor demand depends on the size of firms. We checked that estimates provided in the course of the paper yield results similar to weighted estimates. This is illustrated by Table A12 in appendix which shows the weighted estimates corresponding to those displayed in Table 2.

²³The number of observations in Table 3 is smaller than in Table 2 because it excludes firms with eligible jobs only and firms with ineligible jobs only. The last column of Table 3 displays the difference-in-differences estimates for all jobs with this smaller sample. Results are identical to those displayed in Table 2, corresponding to the full sample also comprising firms without jobs either below or above 1.6 times the minimum wage.

Table 2: Difference-in-differences estimates of the impact of the hiring credit on various labor market outcomes in 2009

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Emp. growth	.009*** (.002; .002; .002; .002)	.008*** (.002; .002; .002; .002)	.009*** (.002; .002; .002; .002)
Hours growth	.010*** (.002; .002; .002; .002)	.009*** (.002; .002; .002; .002)	.008*** (.002; .002; .002; .002)
Hiring rate	.014*** (.005; .004; .004; .004)	.012*** (.004; .004; .004; .004)	.019*** (.005; .004; .004; .004)
Sep. rate	.005 (.005; .004; .004; .004)	.004 (.004; .004; .004; .004)	.010** (.005; .004; .004; .004)
Survival rate	.000 (.001; .001; .001; .001)	.000 (.001; .001; .001; .001)	-.001 (.001; .001; .001; .001)
Nb. Obs	406,468	406,468	207,379

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) and different specifications (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t; the growth rate of the number of hours worked between November of year t-1 and November of year t; the number of hires from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1; the survival rate from 30 November year t-1 to 30 November year t. We define firms as survivors if they have at least one employee paid at the end of November in 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). Standard deviations in parentheses, respectively, not clustered, clustered at the firm level, at the employment pool X sector level, at the employment pool level. There are 348 employment pools ("zones d'emploi", Insee) and 5 sectors. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 3: Difference-in-differences estimates of the impact of the hiring credit for eligible and ineligible jobs on various labor market outcomes in 2009

	Eligible jobs	Ineligible jobs	All jobs
Employment growth	.011*** (.003)	.002 (.004)	.008*** (.002)
Hours growth	.012*** (.003)	.005 (.004)	.008*** (.002)
Hiring rate	.011*** (.004)	.005 (.008)	.008** (.004)
Separation rate	.000 (.004)	.003 (.008)	.000 (.004)
Nb. Observations	350, 633	350, 633	350, 633

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

hours for eligible jobs only. The impact for non eligible jobs is rather positive, but not significantly different from zero. This means that the hiring credit has had a positive impact on total employment and total hours mainly through its impact on eligible jobs, and very marginally on ineligible jobs.

4.3 Robustness checks

Our data set allows us to observe the evolution of employment month-by-month from 2006 to 2011. We use this information to exhibit the common trends of employment growth between small and medium-sized firms at the monthly level, before the introduction of the policy.

We compute the average employment growth from November of year $t - 1$ to the end

of month m in year t for both small and medium firms according to their size in year $t - 1$. We denote the average employment growths for small and medium-sized firms by $g_{m,t}^1$ and $g_{m,t}^0$ respectively.

We then estimate, for every month from 2007 to 2009, the corresponding difference-in-differences coefficients: $g_{m,t}^1 - g_{m,t}^0 - (g_{m,t-1}^1 - g_{m,t-1}^0)$. Since there is no policy change from January 2007 to November 2008 (last month before the announcement of the policy), the difference-in-differences estimates provide placebo tests, as they should be 0 over this period. Figure 7 shows that there is indeed no differential employment evolution between small and medium-sized firms. This figure is a further check of the common trend at annual frequency shown in Figure 5. Appendix C provides year placebo tests on other outcomes, as well as other robustness checks (placebo size cutoffs, and changes in the selection of the estimation sample).

Figure 7 also sheds light on the adjustment of employment in 2009. The estimated impact of the hiring credit increases steadily over the year.²⁴ It is clear that *zéro charges* has had a significant and quick impact on employment, since the difference-in-differences estimates are positive, at 95% level of confidence, from the end of February 2009 – three months after the start of the policy. This result is in line with the literature on dynamic labor demand, according to which employment reacts quickly to shocks on labor costs, with a delay that is clearly infra-annual (Hamermesh, 2013).

Figure 7 also reports difference-in-differences estimates for 2010 and 2011. In order to use only the pre-treatment period as reference to compute the differential evolution of the employment growth rate between small and medium sized firms over these two years, we then define the difference-in-differences estimate for month m of year t as $g_{m,t}^1 - g_{m,t}^0 - (g_{m,2008}^1 - g_{m,2008}^0)$. It is important to remark that these difference-in-differences estimates do not allow us to evaluate the impact of zero charges properly. As explained above, the government announced on 16 November 2009 that the policy, which was initially designed to stop on 31 December 2009, was extended to hires occurring up to 30 June 2010. Firms below the average of 10 full-time equivalent employees from January 2009 to December 2009 were eligible for the extended program for their new hires in 2010. In this context, the difference-in-differences method is not appropriate to properly identify the effects

²⁴It reaches 0.009 in November 2009, which is by construction consistent with Column 3 in Table 2.

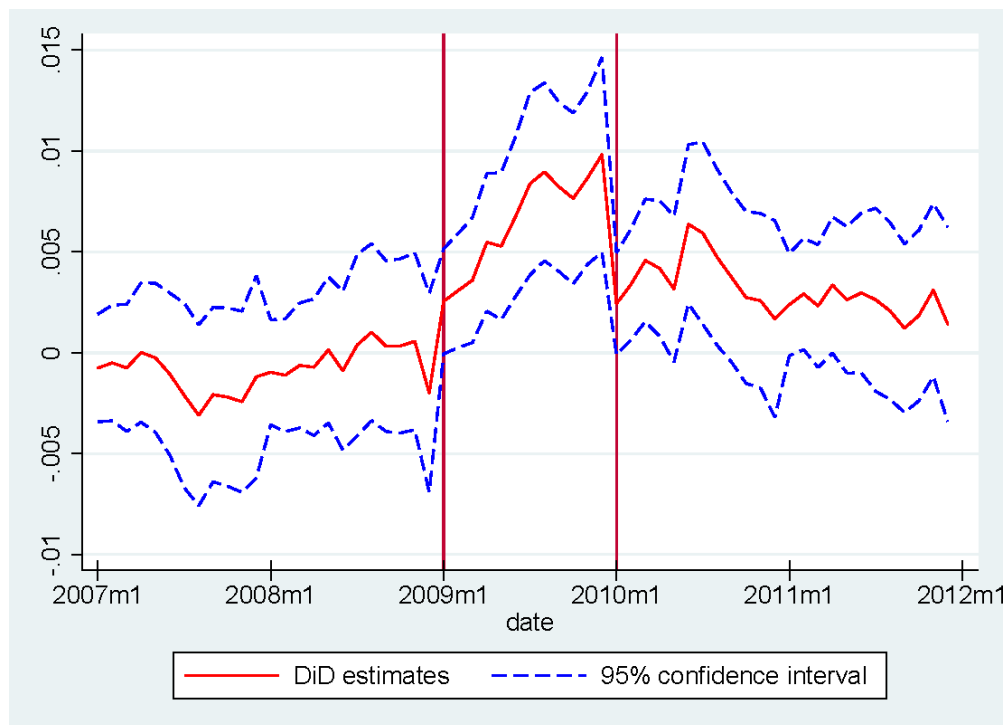


Figure 7: Difference-in-differences estimates on the employment growth rate at the monthly frequency.

Note: Each point plots the monthly difference-in-differences estimate of employment growth rates between small and medium firms. The employment growth rate of month m of year t is the average employment growth from November of year $t - 1$ to the end of month m in year t , for both small and medium-sized firms according to their size in year $t - 1$. The DiD estimates of each year t is computed using relevant reference years. The reference year is 2006 for the 2007 DiD estimates, 2007 for the 2008 DiD estimates, and 2008 for DiD estimates of years 2009, 2010 and 2011. The estimates are obtained in a regression controlling for the same covariates as in column 3 of Table 2 and where standard errors are robust. The 2009 DiD estimates capture the impact of the 2009 hiring credits. The DiD estimates of years 2007 and 2008 are placebo tests of the common trend assumption. The DiD estimates of years 2010 and 2011 are potentially biased by endogenous selection into treatment groups for the 2010 extension of the hiring credit as discussed in the main text.

of the policy in 2010, as some firms treated in 2009 may not have been able to apply in 2010, because eligibility for the extended period was then based on the average size over 2009.²⁵ Moreover, firms could have manipulated the eligibility criteria for the 2010 extension in December 2009: firms above the 10 employees threshold in November 2009 had an incentive to downsize. For this reason, the paper is focused on the impact of *zéro charges* until November 2009.

That being said, Figure 7 shows that the difference-in-differences estimates are lower in 2010 than in 2009, and only marginally significant for the first semester in 2010. Small firms which already benefited from the hiring credit in 2009 did not experience any policy change in December 2009, and thus did not grow beyond their already high employment levels of 2009. This explains the sharp drop in the difference-in-differences estimates in January 2010, which capture the effect of the hiring credit on the employment growth from November 2009 to January 2010. Then, during the second semester of 2010, difference-in-differences estimates are not statistically significant. They are marginally lower than during the first semester, capturing the reaction of firms to the disappearance of the hiring credit. More interestingly, we do not find any significant intertemporal substitution effects, which would have implied significantly slower growth of small firms after the credit ends. However, insofar as the difference-in-differences estimates are probably biased after 2009, they are not relevant to properly identify intertemporal substitution effects either. Appendix Table A13 comprises another test which points to the absence of substitution effects: it shows that there is no evidence of different employment effects of the hiring subsidy between service sectors and other sectors in which production can be more easily substituted from one period to another, thanks to storage.

4.4 Evaluation of the employment elasticity

Since the hiring credit decreased the total labor cost of firms in the treatment group by 0.2 percent²⁶ and increased total employment by 0.8 percent, our estimates point to an

²⁵Similarly, the policy extension announced in 2009 may have affected the firm size distribution in 2010 and consequently the split between medium and small size groups in the computation of the difference-in-difference estimates for 2011.

²⁶In November 2009, firms in the treatment group got 3.6 million euros from *zéro charges* while their labor cost during that month was 1.75 billion euros, which corresponds to a decrease of 0.21% in labor cost. Over the course of year 2009, *zéro charges* decreased the labor cost of firms in the treatment group

employment elasticity with respect to the change in labor cost induced by the hiring credit of around -4 , belonging to the 95% confidence interval $[-6, -2]$.

The strong employment impact of zero charges relies on the absence of wage increases and on the absence of increased churning of workers, as shown in appendices A and B. Even if wages and labor turnover did not increase, this figure may at first sight seem incredibly high, compared to usual estimates of labor demand elasticities. For instance, Kramarz and Philippon (2001) and Abowd et al. (2006) found that the elasticity of employment with respect to the minimum wage is about -2 for men and -1.5 for women in France. Crepon and Desplatz (2001), using a different empirical strategy, found an elasticity equal to -0.8 for workers close to the minimum wage. The strong employment impact is not due to an intertemporal substitution effect, where firms would have frontloaded hires before the end of the first hiring credit period. We verify that the effect is still as strong in the service industries where production cannot be easily re-scheduled and buffered using inventories (see appendix Table A13).

The strong employment impact of *zéro charges* can be explained by the fact that a temporary decrease in average labor cost can have stronger employment effects when it is induced by a hiring credit than by wage changes that apply to all employees. To show this, let ε stand for the elasticity of contemporaneous employment, L , when the change in average labor cost per worker is due to a temporary change in the wage cost w of all incumbent and entrant workers. This is the standard definition of labor demand elasticity when the payroll equals wL . Let ε_σ stand for the elasticity of employment with respect to the average labor cost per worker when the change in average labor cost per worker is due to *zéro charges*. Bear in mind that the hiring credit alters the cost of entrants (i.e. new hires) only. The relation between employment and hires is given by the law of motion of employment (1). Let us assume that ε is identical in all firms and that the hiring credit does not increase churning of workers and wages, which is the case for *zéro charges* as shown in appendices A and B. We get (see appendix F.1.1 for computation details):

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

where η is the share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up of *zéro charges*. As long as $\eta < 1$, the employment

by 0.14%. The amount of subsidies paid by *zéro charges* increased progressively during 2009.

elasticity induced by the hiring credit is larger, in absolute value, than that induced by a proportional change in the wage cost of all workers. The reason is that subsidizing the jobs of incumbent workers yields no employment effects in firms that do not layoff workers absent the subsidy: all it does is to create windfalls for these firms. Using hiring credits is a means to target subsidies at marginal jobs, which yields positive employment effects, without providing subsidies to incumbent workers, which yields no employment effects in these firms.²⁷ In the limit case where $\eta = 1$ (which would happen if the take-up rate were 100% and the whole workforce were renewed over the course of a year), the two elasticities are identical because the entire workforce of firms that benefit from the hiring credit is subsidized.

All in all, we find that ε , the elasticity of employment with respect to labor cost induced by a change in wage, is smaller, in absolute value, than when the labor cost is modified by the hiring credit. The 95% interval confidence of the elasticity ε is $[-1.5, -0.5]$,²⁸ which is in line with previous estimates obtained for France. We show in Appendix C.3 that this estimate is likely a lower bound for the absolute value of the elasticity of employment, which becomes larger when the difference-in-differences model is estimated with bandwidths that exclude firms with positive take-up rates from the control group.

5 IV strategy: variations in treatment intensity across labor markets

The validity of the difference-in-differences identification strategy relies on the assumption that medium-sized firms - our control group - are not affected by the policy. This assumption is questionable. First, the supplementary hires induced by the hiring credit might increase labor market tightness (vacancies/unemployment ratio) and thus the recruiting costs for all firms. Second, the hiring credit may also increase wages in all firms. In our context, it is unlikely that the control group has been affected by the hiring credit

²⁷It should be noticed that wage subsidies can lower job destruction (Schoefer, 2015). Since most job separations affect workers of low tenure, who potentially benefit from the hiring credit, the differential impact of hiring credits and wage subsidies on job separations is an open question left for future research.

²⁸ The share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up of *zéro charges* amounts to 0.26 which implies that $\varepsilon = -4 \times 0.26 \simeq -1$ with a 95% confidence interval equal to $[-1.5, -0.5]$.

to the extent that the hiring credit subsidized about 1% of all jobs only in the economy.²⁹ Nevertheless, to deal with this issue, we implement an alternative identification strategy which does not rely on the assumption that medium-sized firms have not been impacted by the policy. We exploit variations in the treatment intensity across local sector-specific labor markets.

5.1 Econometric model

We group firms into cells wherein the intensity of the treatment differed. Cells are defined as sectors in employment pools (or commuting zones). We distinguish 348 employment pools,³⁰ and 5 sectors (manufacturing, construction, retail and transport, hotels and restaurants, and other merchant services). We propose to use variation in treatment intensity across cells to estimate the impact of the hiring credit on employment among eligible firms. Treatment intensity can be measured by the share of subsidized hires among small firms in each cell. In cells where there is a larger share of subsidized hires among small firms, *zéro charges* should entail a larger effect on the growth rate of employment among these firms.³¹ Formally, our equation of interest is:

$$Y_{jk}^{2009} = \alpha + \delta I_{jk} + \gamma Y_{jk}^{2008} + \beta X_{jk} + v_{jk} \quad (4)$$

where Y_{jk}^{2009} is the employment growth rate³² between November 2008 and November 2009 averaged across eligible firms (with 6 to 10 employees in 2008) in commuting zone j and producing in sector k , Y_{jk}^{2008} is the lagged outcome, I_{jk} is the share of subsidized hires among all hires in 2009 in small firms and captures the treatment intensity, X_{jk} is a set of controls at the cell level.

²⁹About 20 percent of workers are employed in firms with fewer than 10 employees. The take-up rate of *zéro charges* amounts to 24 percent, and 26 percent of workers employed on 30 November 2009 in firms with positive take-up benefit from the hiring credit. Thus, the share of jobs that benefit from *zéro charges* in November 2009 was equal to $0.20 \times 0.24 \times 0.26 = 1.2$ percent.

³⁰We 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.

³¹This design was used to evaluate the effect of the federal minimum wage in the United States using cross-state variation in the fraction of low wage workers (Card, 1992).

³²Henceforth, we focus on employment for the sake of clarity. Results on hours of work, which are not statistically different from those obtained on employment, are consistent with those obtained with the difference-in-differences strategy.

The share of subsidized hires may be correlated with the error term v_{jk} to the extent that the take-up rate is endogenous. As an instrument for the treatment intensity, we use the share of eligible hires (below 1.6 the minimum wage) in small firms averaged from 2006 to 2008. Note that this share is measured before the introduction of the program. The share of eligible hires in 2009 would probably be more correlated with treatment intensity but it is also endogenous, since firms can always shift their wage offers to be eligible for the hiring credit. Therefore it cannot be used as an instrument.

One concern with our instrument could be that it also affects employment growth directly. For example, this would be the case if the business cycle has differential effects for low-wage or high-wage workers. To limit this concern, we include, as an independent variable, a shift-share prediction of employment growth from 2008 to 2009 in the same cell. In every cell jk , we measure the 2008 share of employment in the sub-industries l of sector k , which we denote S_{jk}^l . Then, for every 2-digit industry l , we compute the national employment growth from 2008 to 2009 in firms below 50 employees, excluding the commuting zone j : g_{-j}^l . The shift-share prediction writes: $\sum_{l \in k} S_{jk}^l g_{-j}^l$. To further control for local shocks, we introduce as independent variables the lagged employment growth from 2007 to 2008 of small firms in the commuting zones around the cell of interest, both producing in the same sector and in the other sectors. The set of controls X_{jk} also includes sector dummies and some key cell characteristics measured in the previous year as in the difference-in-differences strategy: the distribution of firms' age, the (log) cell size, the share of firms with sales below 2 million euros, the share of part-time workers and the share of female/male executive/white-collar/blue-collar workers. It also includes cell-level measures of sales/value-added/dividends per employee and of financial leverage (i.e. debt over equity), averaged over the pre-policy period from 2005 to 2008.³³ These balance sheet data come from the French tax returns FICUS/FARE.

The IV strategy can only succeed if there is variation in the share of low-pay hires across employment pool \times sector cells. This is the case as shown in Figure 8, which displays the distribution of this share among the 348 employment pool \times 5 sector cells in our sample (i.e. around 1720 observations, in total, given that some pools do not feature all sectors). The 2006-2008 average share of eligible hires among small firms (with 6 to 10

³³The debt-over-equity ratio is a commonly-used measure of financial leverage in the literature (see the recent example in Giroud and Mueller, 2017).

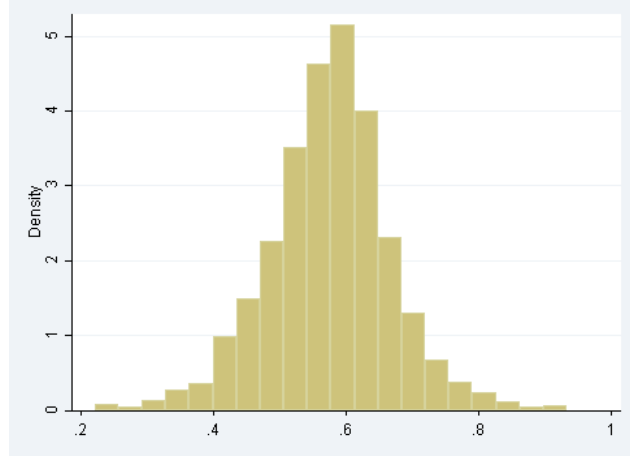


Figure 8: Density of the share of eligible hires from 2006 to 2008 among small firms.

employees to be consistent with our baseline estimates) averages 0.575 with a standard deviation of 0.093.

5.2 Results

We first estimate equation (4) using weighted OLS, where weights are the employment size of each cell as measured among 6-10 employees firms in 2008. Columns 1 and 2 of Table 4 show that a higher share of subsidized hires is associated with a higher growth rate of employment. However, the presence of confounding variables, influencing the employment growth rate and the share of subsidized hires, can imply that OLS estimates are biased. For instance, commuting zones where entrepreneurs are more dynamic may create more jobs and may have lower take-up rates of the hiring credit because their opportunity cost of applying for the hiring credit is higher. Columns 3 and 4 of Table 4 show the results of the IV estimation. The upper panel reports the estimates of the first stage. It shows that the share of eligible hires from 2006 to 2008 is a strong predictor of the treatment intensity in 2009. The coefficients of the second-stage estimation, reported in the bottom panel, are larger than those obtained with OLS, and they are consistent with the estimates obtained with the difference-in-differences strategy.³⁴ We checked that

³⁴To compare these estimates with the benchmark intention to treat estimates obtained in table 2, one needs to account for the share of subsidized hires among small firms. In our sample, the weighted average value of this share is 0.17. Using the value of the coefficient of the fourth column of table 4 this leads to effects at the mean value of $0.17 \times 0.138 = 0.023$ on the employment growth rate with 95 percent

restricting the sample to cells with at least 10, or 30 firms between 6 and 10 employees, does not alter these results. Appendix C provides year placebo tests.

Table 4: Cross-unit estimates of the impact of the hiring credit on employment growth rates in 2009, based on OLS and IV

Covariates	OLS		IV-2SLS	
	No	Yes	No	Yes
			First stage	
			Share of subsidized hires (I_{jk})	
Share of eligible hires in 2006-2008			.519*** (.025)	.267*** (.036)
R^2			.294	.392
			Second stage	
			Employment growth	
Share of subsidized hires (I_{jk})	.029** (.012)	.028** (.012)	.067*** (.020)	.138** (.069)
R^2	.006	.057		
Nb. Observations	1, 721	1, 721	1, 721	1, 721

Source : DADS and FICUS/FARE (Insee), Pole emploi. Note : This table presents estimates based on cross-cell differences in the intensity of treatment in 2009. Each cell corresponds to one employment pool and one sector. Within each cell the treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). Each cell is weighted by its relative employment size as measured among 6-10 employees firms. We consider as outcome the growth rate of employment between 30 November of 2008 and 2009. We control for the lagged employment growth rate from 2007 to 2008. The share of subsidized hires in 2009 is instrumented average share of eligible hires (i.e. hires in small firms with wages between the minimum wage and 1.6 times the minimum wage) in the corresponding cell from 2006 to 2008. Covariates include a shift-share prediction of employment growth from 2008 to 2009 in the same cell, and the lagged employment growth from 2007 to 2008 of small firms in the commuting zones around the cell of interest. The set of controls also includes sector dummies and some key cell characteristics measured in the previous year: the distribution of firms' age, the (log) cell size, the share of firms with sales below 2 million euros, the share of part-time workers and the share of female/male executive/white-collar/blue-collar workers. It also includes cell-level measures of sales/value-added/dividends per employee and of financial leverage (debt over equity), averaged over the pre-policy period from 2005 to 2008. Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

To check further that our two identification strategies yield consistent results, we regress the difference-in-differences equation (2) on the sample made of the employment pool \times sector units. To do so, for each unit and each year we compute the average growth rates of employment and hours worked separately for the two groups of firms (treatment or control). We weight each employment pool \times sector unit by its employment interval confidence [0.001, 0.046].

size among firms from 6 to 14 full-time equivalent employees in the previous year. The results, in appendix Table A14, are similar to those of Table 2. If we restrict our sample to employment pool \times sector units with at least 10, or 20 firms with 6 to 10 employees present in a given year, results remain unchanged.

6 Equilibrium effects

Our previous results suggest that medium-sized firms have not been affected by the hiring credit targeted at small-sized firms to the extent that we find consistent results with the difference-in-differences and the IV identification strategies. Nevertheless, to shed more light on this issue, we check whether employment and wages of firms above 10 employees, as well as the duration of vacancies have been impacted by the share of subsidized hires in their employment pool \times sector cell. If there are equilibrium effects that reduce the net impact of the hiring credit, we should observe lower growth rates of employment among non-eligible firms in cells with a higher share of subsidized hires. Similarly, we should observe either higher wage growth or longer vacancy duration in cells with a higher share of subsidized hires. We adopt the same definition of cells as in the previous section (348 commuting zones \times 5 sectors).

Within each of the 1,720 employment pool \times sector units for which we have observations in both the treatment and the control groups for 2008 and 2009 (i.e. 1675 observations, in total), we compute the ratio of subsidized hires in 2009 to all hires observed in the same year among firms with 0 to 20 full-time equivalent employees in 2008, denoted S_{jk} , where j stands for the employment pool and k for the sector. The average value of S_{jk} , is 0.182, and its standard deviation is 0.085. We also compute for each unit the average growth rate of employment and of wages from November 2008 to November 2009 among firms having from 10 to 20 full-time equivalent employees in the previous year.³⁵ Using data on vacancies posted at the French Public Employment Service, we compute the average time needed to fill vacancies posted in 2009 for each unit. We then compare the labor market outcomes across units with different shares of subsidized hires.

³⁵To control for differential selection into employment across unit, we restrict the wage computation to workers who were both employed in 2008 and in 2009 (see appendix A for more details on wages data).

To achieve this, we estimate the following model :

$$Y_{jk}^{2009} = \alpha + \lambda S_{jk} + \gamma Y_{jk}^{2008} + \beta X_{jk} + u_{jk} \quad (5)$$

where Y_{jk}^{2009} stands for our outcome of interest in 2009 in employment pool j and sector k , Y_{jk}^{2008} its value in the previous year, S_{jk} is the share of hires in the cell subsidized by the 2009 hiring credit. The term u_{jk} is a residual. We include the same set of control variables X_{jk} as in the IV strategy of the previous section.

As in the previous section, the number of subsidized hires in 2009 might be affected by unobserved shocks that also affect employment of medium-sized firms, meaning that the ratio S_{jk} of subsidized hires in 2009 is potentially endogenous in equation (5). For this reason, the ratio S_{jk} of subsidized hires in 2009 is instrumented by the corresponding average share of eligible hires from 2006 to 2008 (when the subsidy was not yet implemented), denoted E_{jk} .

Columns 1 and 2 of Table 5 present the results of the OLS estimation of equation (5), one row for each outcome. Columns 3 and 4 present the results of the IV estimation. Focusing on the results for employment growth and wage growth (first two rows), there is a statistically significant positive correlation between the share of subsidized hires in 2009 and the growth rates of employment and wages in 2009, when we do not add any controls (column 1). Without any controls, the IV estimates are also statistically significant (column 3). However, as we add controls, the OLS and IV estimates are smaller and no longer statistically significant - except at the 10% level for the OLS estimate on wages. Concerning the vacancy duration, we do not find any statistically significant effect of the local share of subsidized hires, once we add controls and instrument the share of subsidized hires. Table 6, which presents the first stage of the IV estimation shows that the shares of subsidized hires in 2009 are strongly correlated with the instruments. These results remain unchanged if we restrict our sample to all employment pool \times sector cells with at least 10 or 30 firms. In appendix C.4, we show that we do not find any equilibrium effects when we allow the spillovers to go beyond the boundaries of the commuting zone \times sector cell. All in all, these results suggest that the hiring credit had no impact on the medium-sized firms.

Table 5: Cross-unit estimates of equilibrium effects on the employment and wage growth rates of firms with 10-20 employees and on the vacancy duration, based on OLS and IV

	(1)	(2)	(3)	(4)
	OLS		IV 2SLS - Second stage	
Employment growth	.073*** (.017)	.027 (.022)	.118*** (.023)	.008 (.051)
Wage growth	.110*** (.021)	.023* (.012)	.185*** (.026)	.055 (.035)
Vacancy duration (log)	-.184 (.145)	.033 (.054)	-.529*** (.157)	-.037 (.142)
Covariates	No	Yes	No	Yes
Nb. Observations	1, 675	1, 675	1, 675	1, 675

Source : DADS and FICUS/FARE (Insee), Pole emploi. Note: This table presents estimates based on cross-cell difference in treatment intensity in 2009. Each cell is one sector in one commuting zone. The treatment intensity is measured as the ratio of subsidized hires in 2009 over total hires in the same year among firms smaller than 20 employees. The dependent variable is either the employment growth of firms sized 10-20 in the previous year (line 1), or the wage growth of their employees (line 2), or the average vacancy duration in 2009 in the cell (line 3). In the IV regressions, the treatment intensity is instrumented by the corresponding shares of eligible hires, i.e. the ratios of the eligible hires in 2008 to total hires in the same year among firms with 0 to 20 full-time employees in the previous year, averaged from 2006 to 2008. Covariates include a shift-share prediction of employment growth from 2008 to 2009 in the same cell, and the lagged outcome. The set of controls also includes sector dummies and some key cell characteristics measured in 2008: the distribution of firms' age, the (log) cell size, the share of firms with sales below 2 million euros, the share of part-time workers and the share of female/male executive/white-collar/blue-collar workers. It also includes cell-level measures of sales/value-added/dividends per employee and of financial leverage (debt over equity), averaged over the pre-policy period from 2005 to 2008. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 6: First stage of the instrumental variable estimates of equilibrium effects.

IV 2SLS - First stage		
	Share of subsidized hires (S_{jk})	
Share of eligible hires in 2006-2008 (E_{jk})	.597*** (.028)	.439*** (.042)
R^2	.478	.593
Covariates	No	Yes
Nb. Observations	1, 675	1, 675

Source : DADS and FICUS/FARE (Insee), Pole emploi. Note: This table presents the first stage of the IV estimation of equation (5). We regress, at the cell level, the share of subsidized hires in 2009 on the share of eligible hires in the same year - among firms smaller than 20 employees. We use the same covariates and weights, as in table 5. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

7 Costs per job created

In this section, we first compute the cost per job created by *zéro charges* under the assumption that there are no equilibrium effects. This assumption is consistent with the results of section 6. Only a small share of firms were eligible for this temporary hiring credit, in a context where the minimum wage was binding and unemployment was high, so that we do not observe any equilibrium effects. In the second part of this section, we estimate the parameters of a structural search and matching model in order to simulate how equilibrium effects could affect the cost per job created, if the hiring credit were implemented in different economic environments, at different scales and on different time spans. A potential concern with this approach is that the estimation of the structural parameters of the search and matching model relies on the behavior of small-sized firms following a shock on low wages. In this context, empirical evidence suggests that our estimates would rather yield a lower bound of cost per job created.³⁶

7.1 Cost per job created by *zéro charges*

Based on our estimates, it is possible to compute the gross cost per job created by the hiring credit in the treatment group. The cost per job created by the hiring credit, c_σ , is a simple function of the contemporaneous wage w and of the employment elasticity with respect to the change in average labor cost induced by the hiring credit (see appendix F.1.1):

$$c_\sigma = -\frac{w}{\varepsilon_\sigma} \quad (6)$$

Since we estimated that $\varepsilon_\sigma = -4$, this formula also indicates that the cost per job created is equal to 25% of the cost of a job. Accordingly, at the end of 2009, the monthly cost of creating one job amounts to around 700 euros.³⁷

³⁶Empirical evidence shows that the labor demand for low-skilled workers is more elastic (Hamermesh (2013), Lichter, Peichl, and Siegloch (2015)). Therefore, if the credit had concerned all types of workers, we expect the cost per job created to be higher. Second, there is far less empirical evidence on the heterogeneity of labor demand elasticity by firm size. That being said, we can expect large firms to be more capital intensive. The standard labor demand theory of Cobb-Douglas firms predicts that the short-run labor demand elasticity decreases with the capital intensity of the firm. Then the cost per job created estimated on small firms is probably a lower bound of the cost of a similar hiring credits, independent of firm size.

³⁷Alternatively, we can directly compute the cost per job created from the treatment effect estimate. There are 646,717 jobs in the treatment group at the end of 2008 (cf. appendix). According to table 2,

This is a gross cost, because it ignores the savings generated by job creation in terms of unemployment and other social benefits that would have been paid in the absence of the measure. It also ignores the remaining social contributions paid by employees on these additional jobs. We exploit a survey, presented in appendix D, which allows us to precisely evaluate the savings permitted by *zéro charges* on social benefits. To this end we rely on two key assumptions. First, consistent with our estimation of the impact of *zéro charges* on net job creation, we assume that the number of jobs created by *zéro charges* reduced non-employment by the same amount. Second, we assume that social benefits would have been paid to individuals identical to the beneficiaries of *zéro charges* if they had remained on the dole. We find that the savings amount to about 700 euros per month. This makes the net cost of the hiring credit per created job equal to zero.³⁸

7.2 The search and matching model

The previous estimated costs hold for a small and temporary hiring credit program which subsidized about 1% of all jobs in the economy and had no equilibrium effects. Moreover, this hiring credit was implemented during a recession, in a context where the minimum wage was binding and unemployment was high. A hiring credit covering more hires or occurring in different environments might entail equilibrium effects which could change significantly the impact of the policy, as suggested by Beaudry et al. (2012, 2014), Crépon et al. (2013), Gautier et al. (2012), and Lalive et al. (2013). To shed light on this issue, we estimate a search and matching model which enables us to account for equilibrium effects.

7.2.1 Model description

We consider a discrete time economy with a large number of labor pools or “islands” in which a representative firm produces a differentiated product with labor only as in the Dixit and Stiglitz (1977) model. The revenue function of the firm in period t is equal to

our estimate of coefficient δ when the dependent variable is $\Delta L/L_{-1}$ in equation (2), equals 0.008. Thus, the number of jobs created in the treatment group is $0.008 \times 646,717 = 5,173$. The *zéro charges* hiring credit provided 3.6 million euros to the firms of the treatment group in our sample at the end of the period, i.e. in November 2009. Accordingly, at the end of 2009, the monthly cost of creating one job amounts to 700 euros.

³⁸These costs do not account for the potential costs associated with distortive taxes used to finance the hiring credit.

$A_t R(L_t)$, where R is an increasing and concave function homogeneous of degree $(1 + \alpha)$, such that $R(L_t) = (L_t)^{1+\alpha} / (1 + \alpha)$, and $A_t > 0$ is a productivity parameter. In this set-up, parameter α is a composite of the labor elasticity of the production function and of the price elasticity of the demand function for the product of the firm, as shown in appendix F.1.1.

The firm needs to post vacant jobs to hire workers. A vacant job costs c_V units of output per period. In each period, the sequence of decisions is as follows: 1) an exogenous proportion q_{t-1} of workers quit the firm; 2) vacant jobs are posted; 3) workers are hired; 4) production takes place and wages are paid. The assumption of exogenous job separation, which allows us to simplify the analysis, is consistent with the finding that the hiring credit did not induce firms to increase labor turnover in order to benefit from the subsidy (appendix B).

A vacant job posted in period t is matched with a worker with probability $m_t \in [0, 1]$ in the period and remains vacant with probability $1 - m_t$. The probability to fill a vacant job is determined by a matching function: $m(\theta_t) = \bar{m}\theta_t^{-\mu}$, where $\theta_t = V_t/U_{t-1}$, equal to the ratio of the number of job vacancies V_t over the number of unemployed workers U_{t-1} , denotes the labor market tightness; \bar{m} is a positive parameter, and μ , the elasticity of matching with respect to unemployment, belongs to the interval $(0, 1)$.

Exogenous wages

Let us denote by w_t the wage in period t and by $\Pi(Z_t, L_{t-1})$ the value function of the firm, where $Z_t = (A_t, w_t, m_t, q_{t-1})$. Let β denote the discount factor and \mathbb{E}_t the expectation operator. When wages are exogenous, the value function of the firm satisfies

$$\Pi(Z_t, L_{t-1}) = \max_{V_t} A_t R(L_t) - w_t L_t - c_V V_t + \beta \mathbb{E}_t \Pi(Z_{t+1}, L_t)$$

subject to the law of motion of employment:

$$L_t = (1 - q_{t-1})L_{t-1} + m_t V_t$$

The solution of the firm's problem is detailed in appendix F. When the wage is exogenous (see appendix F.1), the equilibrium values of employment and of the labor market tightness are defined by the labor demand equation derived from the solution of the

maximization problem of the firm, and by the law of motion of the unemployment rate $u_t = 1 - L_t$:

$$A_t R_L(L_t) = w_t + \frac{c_V}{m(\theta_t)} - \beta(1 - q_t) \mathbb{E}_t \frac{c_V}{m(\theta_{t+1})} \quad (7)$$

$$u_t = u_{t-1} + q_{t-1}(1 - u_{t-1}) - \theta_t m(\theta_t) u_{t-1}. \quad (8)$$

Endogenous wages

The case with endogenous wage is described by a directed search and matching model with wage posting in the spirit of Moen (1997) and Kaas and Kircher (2015). In each labor pool, employers post vacant jobs. Each job vacancy is posted with a non renegotiable wage which applies throughout the employer-employee relationship. Unemployed workers are assumed to have perfect information on the situation in each labor pool. Their search activity can be directed toward their preferred employment pool. The mobility of workers between labor pools is perfect. The solutions to the model and the computation of the cost per job created are presented in appendix F.2.

The assumption of wage posting also implies that the market equilibrium allocation is constrained efficient if the product market is perfectly competitive. To give room to welfare improving hiring subsidies, we assume that there is imperfect competition on the product market. This ensures that the market equilibrium unemployment rate is higher than the constrained efficient unemployment rate.³⁹

7.2.2 Structural estimation

Our objective is to estimate the parameters μ, c_V and $\alpha = LR_{LL}(L)/R_L(L)$, i.e. the elasticity of the marginal revenue with respect to labor. It is shown, in appendix F, that these parameters, together with the discount factor β , the job separation rate q_t , the duration of job vacancies, $1/m(\theta_t)$, and the unemployment rate u_t , allow us to compute the elasticity of employment with respect to different types of hiring credit: temporary, permanent, targeted at a small subset of jobs, generalized to all jobs.

³⁹An alternative possibility would have been to assume perfect competition on the product market and wage bargaining. However, wage bargaining in large (i.e. with more than one employee) firms with decreasing marginal productivity of labor induces over-employment, as shown by Stole and Zwiebel (1996) and Cahuc et al. (2008). Accordingly, assuming wage bargaining in a context with large firms does not produce inefficiently high unemployment, leaving less scope for welfare improving hiring credits.

Table 7: Estimates of the parameters of the matching function.

	(1)	(2)
Dep. var.	Job finding rate (log)	
	OLS	IV
Labor market tightness (log)	.654*** (.018)	.545*** (.060)
Year FE	Yes	Yes
Employment pool FE	Yes	Yes
Nb. Observations	1,392	1,044

Source : Pôle emploi and Enquête emploi (Labor Force Survey, Insee) Note: Estimation of the parameter of the job matching function equation (A15) on 348 employment pools from 2006 to 2009. (1) Standard (within) OLS ; (2) IV regression. As an instrument we use the lagged value of the labor market tightness. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Following the search and matching literature (e.g. Kaas and Kircher, 2015; Hall, 2017), the annual discount factor β is set at 0.95. The other statistics – $q_t, 1/m(\theta_t), u_t$ – are directly observable in our data. We conduct the structural estimation of parameters μ, c_V and α in two steps – see appendix E for more details on the data and on the estimation.

Matching technology

First, using data on job finding rates and tightness at the employment pool level from the Public Employment Service and from the Labor Force Survey, we estimate the parameters of the matching technology. The elasticity μ is identified using variations of job finding rates and labor market tightness over time within employment pools. In the spirit of Borowczyk-Martins et al. (2013), we address potential endogeneity issues using an IV strategy based on past values of the labor market tightness. The results of the estimation are presented in Table 7. We obtain an elasticity of the matching function with respect to the unemployment rate, μ , around 0.45.

Revenue function and vacancy posting cost

Second, we estimate the production parameters, namely the elasticity of the marginal revenue with respect to labor, α , and the cost of posting a vacancy c_V . These parameters are obtained by conducting a structural estimation of the labor demand equation (7). The identification strategy, which relies on the natural experiment triggered by *zéro charges*,

is similar to the IV strategy developed in section 5. We focus on small firms, which are eligible for the program. The policy shock entails exogenous variations in labor cost that are used to identify the revenue parameter α . In employment pools where the shock on labor cost is larger because the share of hires eligible for the hiring credit is larger, average employment effects should also be larger. These differences in employment effects yield the elasticity of the marginal revenue α . The estimation strategy is complemented to as well recover the vacancy posting cost c_V . The effects of the hiring credit on employment are larger where baseline hiring costs are lower. Hiring costs depend on the time needed to fill a vacancy. In a tight labor market, where vacancy duration is large, the hiring credit is less effective. Using heterogeneity in the effects of the hiring credits across local labor markets with low or large vacancy duration, we identify the vacancy posting cost c_V . Both parameters – the elasticity and the vacancy cost – are identified using the average values over 2006-2008 in firms with 6 to 10 full-time employees of the share of eligible hires, of the duration of job vacancies and of the job separation rate, as instruments for the hiring credit and the hiring cost in 2009. Note that the structural estimation of the labor demand is simplified by the fact that the identifying shock has no impact on wages and labor market tightness, as shown in section 6.

The results of the regression of employment growth on labor cost and hiring costs are presented in Table 8. We show in appendix E that the coefficient of the average hiring credit, denoted a_1 , in the regression of Table 8 is the inverse of the absolute value of the marginal revenue elasticity α . The IV estimate of a_1 then entails that the elasticity of the marginal revenue with respect to labor is about -0.96 . We also show in the appendix that the coefficient of the hiring cost, denoted b_1 , is the ratio of the vacancy cost in terms of wage c_V/w_{t-1} over the elasticity α . Consequently, Table 8 implies that the annual cost of a vacant job represents 5.8% of the annual wage. Since the average duration of a job vacancy is about 0.2 year, the hiring cost amounts to 1.2% of the annual wage, which is in line with the available empirical evidence.⁴⁰ Overall, these values imply that the micro elasticity of employment to a change in labor cost, which is about -1 , is consistent with

⁴⁰Kramarz and Michau (2010) estimate that hiring costs represent 2.8% of the wage bill in France. Flinn (2006) finds that the flow vacancy cost is 1.5% of the annual labor cost at the minimum wage in the US. For an overview on several countries, see Manning (2011) Table 2, p 983.

the difference-in-differences and the IV estimates presented above.⁴¹

Table 8: Estimates of the parameters of the revenue function and of the search cost.

	(1)	(2)
	OLS	IV
First stage		
Dep. var.	Average hiring credit Hiring cost	
Share eligible hires	.030*** (.003)	-.003 (.012)
Duration of vacant jobs	.020* (.011)	.851*** (.081)
Separation rate	.015*** (.001)	.341*** (.007)
Second stage		
Dep. var.	Employment growth	
Average hiring credit (a_1)	.694*** (.127)	1.04*** (.282)
Hiring cost (b_1)	-.050*** (.016)	-.061*** (.018)
Controls	Yes	Yes
Nb. Observations	1, 588	1, 588

Source : Pôle emploi and DADS (Insee). Note: This table presents the estimation of equation (A19). In the OLS and the second stage of the IV estimation, the dependent variable is the average growth rate of employment from 1 December 2008 to 30 November 2009 in each employment pool x sector unit, among firms with 6 to 10 full-time equivalent employees in the previous year. The independent variables are the average hiring credit per employee and the hiring cost as defined in equation (A19). Following the structural model, we control for the wage growth in small firms. To control for local shocks, we include as covariates, the employment growth in firms with 10 to 14 full-time equivalent employees in the previous year and the shift-share predictions of employment growth, defined as in the previous sections. We also add the average 2006-2008 employment growth at the regional level, as above. We also control for key cell characteristics: the distribution of firms age in the cell, the average share of part-time workers, and the share of firms with sales below 2 million euros in the previous year. The average hiring credit per employee and the hiring cost are instrumented by the corresponding average values of the share of eligible hires, of the duration of job vacancies and of the job separation rate among firms with 6 to 10 full-time employees over 2006-2008. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with 6 to 10 full-time equivalent employees in the previous year. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

7.3 Cost per job created in different contexts

Let us remind that temporary hiring credits are one-off schemes that provide hiring subsidies during specific periods, whereas permanent hiring credits provide them whatever

⁴¹This elasticity is defined by equation (F11) in appendix F.1.1.

the timing of hiring. In both cases, the subsidy is paid for a short period of time (usually one year) following the hiring. Neumark and Grijalva (2015) report that 99 of the 147 hiring credits recorded in the United States over the period 1970-2012 are permanent. There is also a great diversity of hiring credits in OECD countries (OECD, 2010). Accordingly, it is worth analyzing in detail the consequences of different types of hiring credits in different contexts. We first consider the cost per job created when the hiring credit is either (non-expected, one-off) temporary or permanent. We analyze these credits when they are either targeted at a small subset of firms or accessible to all firms, and when wages are either rigid or flexible. A related issue concerns the feature of permanent hiring credits, which can be time invariant, but also countercyclical, i.e. provided when the economy slows down only. To deal with this issue, we use the equilibrium model to explicitly simulate a context where the government is committed to provide the credits when the economy slows down.

7.3.1 Permanent versus non-expected temporary hiring credits

In this section, we compare one-off non-expected temporary hiring credits lasting one year (like *zéro charges*) with time-invariant permanent hiring credits. Let us remind that the cost per job created by a temporary hiring credit, set out for one year, corresponds to the monthly cost necessary to create one supplementary job at the one-year time horizon, whereas the cost per job created by a permanent hiring credit, set out without any foreseen time limit, is the monthly cost necessary to create one supplementary job permanently, i.e. on an infinite time horizon. The costs per job created reported in Table 9, are computed using the elasticities of employment with respect to labor costs derived from the structural model in the neighborhood of the steady state (see appendices F.1 and F.2). The steady state values of the model are computed over the 2005-2008 pre-recession period used for the estimation of the structural parameters (see appendix F.3). We evaluate the cost per job created for different values of the product market power of firms. In line with available estimates (Bouis, 2008), the markup is set equal to 1.3 in the benchmark case, corresponding to the actual situation in 2008. In the lower panels of Table 9, we consider two alternative cases around this benchmark: zero markup, which yields the constrained efficient allocation when the wage is endogenous, and a markup

higher than in the benchmark, equal to 1.6, which yields higher unemployment rate.

The importance of the duration of hiring credits

Table 9 shows that *zéro charges* was implemented in the most favorable situation: it was temporary, targeted at a small subset of firms, and it occurred in a context where wages were rigid. The cost per job created by a permanent hiring credit targeted at a small subset of firms in the same environment would have been four times larger. This result is an immediate consequence of the relation between the elasticity of employment with respect to a change in average labor cost per job induced by a change in the wage of all workers, ε , and the elasticity of employment with respect to a change in average labor cost per job induced by a temporary hiring credit, $\varepsilon_\sigma = \varepsilon/\eta$ where η stands for the share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up of *zéro charges*. In our setup, η is about 26%. When the hiring credit becomes permanent, η goes to one and the cost per job created by the hiring credit is increased by a factor of four.⁴² Accordingly, hiring credits are more effective at creating jobs at a low cost when they apply on short periods of time.

Targeted versus economy-wide hiring credit

Table 9 shows that the cost per job created by hiring credits accessible to all firms is only slightly bigger than the cost per job created by hiring credits targeted at a small subset of firms when the wage is exogenous. This means that hikes in recruitment costs induced by the increase in the labor market tightness associated with economy-wide hiring credits have only a small impact on employment. This result is the consequence of small hiring costs as stressed above in section 7.2.2.

The adjustment of wages

The comparison of cases with endogenous and exogenous wages shows that the costs per job created are very close in these two cases when the hiring credit is temporary. Note that they are even identical when the hiring credit is targeted at a small subset of firms. This result is a consequence of the assumption of decreasing marginal revenue. This implies that each firm optimally increases the number of hires when the hiring credit

⁴²The cost per job created of a permanent hiring credit is equal to that of permanent employment subsidy if the firms and the government face the same interest rate, which is assumed here.

Table 9: Cost per job created (in percentage of the wage) by hiring credits in different contexts

Hiring credit:	To small subset of firms		To all firms	
	Temporary	Permanent	Temporary	Permanent
Baseline mark-up: 1.3				
Exogenous wage	25.2	96.9	25.7	102.9
Wage posting	25.2	96.9	26.1	217.0
High mark-up: 1.6				
Exogenous wage	25.7	99.1	27.5	107.2
Wage posting	25.7	99.1	28.9	161.8
No mark-up				
Exogenous wage	24.8	95.5	25.0	99.8
Wage posting	24.8	95.5	25.1	272.2

Note: The cost per job created is gross, and as such it does not account for the savings induced by job creation in terms of unemployment and other social benefits that would have been paid in the absence of the measure. It also ignores the remaining social contributions paid by employees on these additional jobs. Hiring credits which are targeted at "small subset of firms" entail no equilibrium effects. Hiring credits which are available to all firms entail equilibrium effects. Temporary hiring credits are provided for hirings occurring during a period of one year. Permanent hiring credits are time-invariant and provided for hirings occurring at any moment. The cost per job created is computed using the formula derived in appendices F1, F2 and F3. The annual discount factor, β , is set to 0.95. The estimation of the structural model yields: $\mu = 0.45$, $\alpha = -0.964$ and the annual cost of a vacant job $cv/w=0.056$. The separation rate, q , and the duration of job vacancies $1/m$ are computed from the DADS and Pole emploi data. We get $q=0.5$, $1/m=0.2$ on average over the period 2005-2008. The unemployment rate in the benchmark situation reported in the top panel of the table is computed from the French Labor Force Survey (Enquête emploi), which yields $u=0.083$. The share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up in 2009 is computed from the DADS and from the "hiring credit file". We get $\eta = 0.26$. The costs per job created when the price mark-up is high, and when competition is perfect, are reported in the middle and the bottom panel respectively. These counterfactual environments are detailed in appendix F.3.

is implemented instead of increasing the hiring wage as long as the labor market tightness remains unchanged.⁴³ In this context, an increase in the reservation wage of workers, induced by the hike in the labor market tightness, is the only channel through which the hiring credit exerts an impact on wages. Namely, in the period where the hiring credit is implemented, firms are induced to post more job vacancies, which pushes the labor market tightness up. The improvement in job finding raises the reservation wages and then the equilibrium wages. The size of this effect is smaller when the hiring credit is temporary rather than permanent, as discussed above. Indeed, in our setup, this mechanism has a small impact on employment creation when the hiring credit is temporary since it increases the cost per job created by about one point of percentage. The small impact of the reaction of wages relies in particular on the relative low elasticity of the job matching function with respect to the unemployment rate, which is estimated at 0.45 in our framework.

Nevertheless, the reaction of wages has a very strong impact when hiring credits are permanent: the cost per job created by an economy-wide permanent hiring credit is about 2 times higher than when wages are rigid.

The market power of firms

When firms have more power in the product market, the wage elasticity of employment is smaller in absolute value (α is greater in absolute value - see details in appendix F.3). This contributes to raising the cost per job created. The market power of firms has another effect that goes in the opposite direction. It increases unemployment, which decreases the hiring costs and exerts a downwards pressure on wages, and then reduces the cost per job created.

As shown by the lower panels of Table 9, the cost per job created increases with the market power of firms in all cases except when the hiring credit is permanent and wages are endogenous. It is clear that the market power of firms has a small impact on the cost per job created for relevant values of the markup when the wage is rigid or when the hiring credit is temporary. However, when the hiring credit is permanent and wages react, the market power of firms can have a significant impact on the cost per job created, which

⁴³See the discussion in appendix, equation (F32). This mechanism is the consequence of decreasing marginal productivity of labor. It also holds in a model with wage bargaining instead of wage posting. It has been highlighted by Stole and Zwiebel (1996) and examined further in a search and matching model by Cahuc et al. (2008).

decreases by 100 percentage points when the markup goes from 1 to 1.6. In this case, the further away from the constrained efficient equilibrium, the more effective the hiring credit is to close the employment gap.

7.3.2 Countercyclical hiring credits

Our previous analysis compares (non-expected, one-off) temporary hiring credits with permanent time-invariant hiring credits. Since the cost of job creation is smaller in recession, because the labor market tightness is smaller when unemployment is higher, the returns of expenditure on hiring subsidies is larger in recession. This can justify a more intensive use of hiring subsidies in recessions.⁴⁴ In order to analyze countercyclical hiring credits, provided when the economy slows down only, we assume that the economy fluctuates according to a Markov process between two states: a high-productivity low-separation state with 7% steady state unemployment rate and a low-productivity high-separation state with 11% steady state unemployment rate, consistent with the French economy over the last 40 years (see appendix F.4). Table 10 reports the cost per job created by temporary hiring credits lasting one year during a recession (in columns 1 and 4), by time-invariant permanent hiring credits (in columns 2 and 5) and by countercyclical hiring credits triggered during the whole recession periods which last 25 months on average in France (in columns 3 and 6).⁴⁵ Table 10 shows that the cost per job created by countercyclical hiring credits is much higher than that of one-off non-anticipated temporary hiring credits lasting one year only.

A first reason is that slowdowns last 25 months on average, implying that a larger share of employees benefit from the countercyclical credit than from the temporary hiring credit (which by definition benefits hires occurring over a one-year period only). This effect approximately doubles the cost of countercyclical hiring credits with respect to temporary hiring credits lasting one year. Obviously, it would be smaller if the countercyclical hiring

⁴⁴In the context of our model, recessions are driven by productivity shocks and there is no specific source of inefficiency during recessions which could justify the use of hiring credits. Countercyclical markups could create such inefficiencies. While there is some evidence that markups are countercyclical (Rotemberg and Woodford, 1991, Chevalier and Scharfstein, 1996 and Wilson and Reynolds, 2005, Ravn, Schmidt-Grohe and Uribe, 2006), there is no consensus of the empirical literature on this issue (Nekarda and Ramey, 2013 and the articles they cite provide evidence for acyclicity or procyclicality of markups).

⁴⁵The differences between the costs per job created by temporary/time-invariant hiring credits in Table 9 and 10 are due to the fact that the baseline economies differ in both simulations sets.

Table 10: Cost per job created (in percentage of the average wage) by temporary, time-invariant and counter-cyclical hiring credits, along the business cycle.

Hiring credit:	To small subset of firms			To all firms		
	(1)	(2)	(3)	(4)	(5)	(6)
	Temporary	Invariant	Countercyclical	Temporary	Invariant	Countercyclical
Exogenous wage	25.2	97.2	49.9	26.1	106.1	53.4
Wage posting	25.2	97.2	49.9	26.8	256.2	101.0

Note: Hiring credits which are targeted at "small subset of firms" entail no equilibrium effects. Hiring credits which are available to all firms entail equilibrium effects. Temporary hiring credits are non-anticipated, one-off and provided for hirings occurring over a period of one year during recessions. Time-invariant hiring credits are provided at every moment. Countercyclical hiring credits are only provided in periods of recession. The price markup of firms is set to 1.3. The cost per job created is computed thanks to simulations of the 2-state search and matching model, described in appendix F.4. Details about the calibration of the model are provided in appendix F.4.3. The cost per job created is gross.

credit applied on shorter periods.

A second reason is that, since labor is less expensive during slowdowns thanks to the countercyclical hiring credit, firms substitute jobs in slowdowns for jobs in expansions. The effect of this channel is quantitatively very small to the extent that it changes the timing of job creation without modifying significantly its impact on overall job creation when wages remain constant.⁴⁶

The third reason comes from the upward pressure on wages exerted by the increase in hires induced by the countercyclical hiring credit in recessions. The last column of Table 10 shows that this last effect has a strong impact. When the hiring credit is economy-wide, this last effect approximately doubles the cost per job created when the wage is endogenous compared to the exogenous wage case. Then the countercyclical hiring credits is 4 times more costly than the one-off temporary hiring credits. Such a difference cannot be explained by different shares of subsidized hires over employment only, as the share of subsidized hires in countercyclical policies is twice the one in temporary credits.⁴⁷

⁴⁶ The comparison of columns 1 and 3 (or 4 and 6, row 1) of table 10 shows that the impact of this channel on the cost per job created is quantitatively small, as the cost per job created by the countercyclical hiring credit is about twice as high as that of the temporary credit. This factor almost maps the ratio of average recession length – 25 months – to the shorter duration of the temporary credit, which lasts 12 months. Consequently, the first reason, related to the share of infra-marginal employees in the firm, explains almost all the difference in the cost per job created between one-off non-expected hiring credits and countercyclical hiring credits when the wage is exogenous.

⁴⁷ Moreover, in the economy-wide endogenous-wage case, assuming that slowdowns last 12 months -

Therefore, the cost per job created by economy wide countercyclical hiring credits remains high, even if it is significantly lower than that of time-invariant hiring credits.

All in all, this analysis suggests, in line with the findings of Kaas and Kircher (2015), that there is little room for economy-wide permanent hiring credits, either time-invariant or countercyclical, when wages can react.

8 Conclusion

This paper shows that a hiring credit targeted at small firms and low wage workers did have a significant impact on employment in France during the 2008-2009 recession. All in all, the hiring credit was very effective. It allowed the government to create jobs at zero net cost in a short period of time.

The estimation of a search and matching model shows, however, that the effectiveness of this hiring credit relied on very special circumstances: it was (one-off, non-anticipated) temporary, it was targeted at a small subset of firms and it was implemented in a context with high binding wage floors and high unemployment. Among all these elements that have favored the effectiveness of the hiring credit, it appears that its temporary nature was key. The one-off non-anticipated temporary nature of the hiring credit allows the government to lower the cost of entrants but not that of incumbent workers with limited effects on wages which need time to adjust. This implies that hiring credits can be effective to boost job creation at a low cost if they are non-anticipated and implemented on short periods of time. The search and matching model suggests that this conclusion also holds true for economy-wide hiring credits and even when the minimum wage is not binding. Nevertheless, a counterpart of this positive conclusion is that hiring credits create jobs at very high costs when they are permanent, either time-invariant or countercyclical, especially when there are no high wage floors. This suggests that they should be avoided in such circumstances.

Future research should aim at clarifying the differential impact of hiring credits and wage subsidies on job separations. In this paper, we found that job separations were

instead of 25 months- entails a cost per job created of 49.5% of the labor cost by countercyclical hiring credits, which is about twice as high as that of the one-off non expected temporary hiring credits of the same duration.

unaffected by the one-off hiring credit implemented during the 2008-2009 recession. Consistently, job destruction is considered exogenous in the model used to obtain structural estimates. However, in other settings, job destruction could be reduced by wage subsidies or permanent hiring credits. In that case, their costs per job created could be lowered compared with one-off, non-anticipated hiring credits.

References

- Abowd, J., Corbel, P. and Kramarz, F. 1999. The Entry and Exit of Workers and the Growth of Employment, *Review of Economics and Statistics*, vol 81(2), pp.170-187.
- Abowd, J., Kramarz, F. Margolis, D. and Philippon, T. 2006. The Tail of Two Countries: Minimum Wage and Employment in France and the United-States, Crest working paper.
- Attanasio, Orazio P., Costas Meghir, and Ana Santiago. 2012. Education Choices in Mexico: Using a Structural Model and a Randomized Experiment to Evaluate PROGRESA." *Review of Economic Studies* 79 (1): 37-66.
- Beaudry, P., Green, A. and Sand, B. 2012. Does Industrial Composition Matter for Wages? A test of search and bargaining theory, *Econometrica*, 80(3), pp. 1063-1104.
- Beaudry, P., Green, A. and Sand, B., 2014, In Search of Labor Demand, NBER working paper w20568
- Becker, G. 2010. Subsidies to Small Business?, www.becker-posner-blog.com.
- Benmmarker, H., Mellander, E. and Öckert, B. 2009. Do regional payroll tax reductions boost employment?, *Labour Economics*, Elsevier, vol. 16(5), pp. 480-489, October.
- Bertrand, M., Duflo, E. and Mullainathan, S. 2004. How Much Should We Trust Differences-in-Differences Estimates?, *Quarterly Journal of Economics* 119(1), pp. 249-275.
- Bishop, J. H. 1981. Employment in construction and distribution industries: The impact of the New Jobs Tax Credit. In S. Rosen (Ed.), *Studies in labor markets* (pp. 209-46). Chicago: University of Chicago Press.
- Blundell, R., Costa Dias, M., Meghir, C., Van Reenen, J., 2004. Evaluating the employment impact of a mandatory job search program. *Journal of the European Economic Association* 2 (4), 569-606.
- Borowczyk-Martins, D., G. Jolivet, and F. Postel-Vinay, 2013. Accounting For Endogeneity in Matching Function Estimation, *Review of Economic Dynamics*, 16, pp. 440-51.
- Bouis, R., 2008. Competition within sectors in France, *Tresor-Economics*, n°27.
- Brugemann, B., Gautier, P., and Menzio, G., 2016. Intra Firm Bargaining and Shapley Values., NBER Working Paper 21508.
- Cahuc, P, Carcillo, S. and Zylberberg, A. 2014. *Labor Economics*, the MIT Press, second edition.
- Cahuc, P., Marques, F. and Wasmer, E. 2008. A Theory of Wages and labor demand with intrafirm bargaining and matching frictions, *International Economic Review*, vol 48 (3), pp. 943-72.
- Cameron A. C., and Miller, D. L., 2015, A Practitioner's Guide to Cluster-Robust Inference, *Journal of Human Resources*, forthcoming.
- Card, David, 1992, Using Regional Variation in Wages to Measure the Effects of the Federal Minimum Wage, *Industrial and Labor Relations Review*, 46, pp. 22-37.
- Ceci-Renaud, N., and Chevalier, P. 2010. L'impact des seuils de 10, 20 et 50 salariés sur la taille des entreprises françaises, *Économie et Statistique*, Programme National Persée, vol. 437(1), pp. 29-45.
- Chetty, R., 2009. Sufficient Statistics for Welfare Analysis: A Bridge Between Structural and Reduced-Form Methods, *Annual Review of Economics*, 1:451-488
- Chevalier, J., and Scharfstein, D. 1996. Capital-Market Imperfections and Countercyclical Markups: Theory and Evidence. *The American Economic Review*, 86(4), pp. 703-725.
- Crépon, B., and Desplatz, R. 2001. Une nouvelle évaluation des effets des allègements de charges sociales sur les bas salaires, *Économie et Statistique*, Programme National Persée, vol.

348(1), pp. 3-34.

Crépon, B., Duflo, E., Gurgand, M., Rathelot, R. and Zamora, P. 2013. Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment. *Quarterly Journal of Economics*, 128(2), pp. 531-580.

DARES, 2012. L'indemnisation par le régime d'assurance chômage, Mars 2012, n°019.

Davis, S., Faberman, J. and Haltiwanger, J. 2012. Labor market flows in the cross section and over time, *Journal of Monetary Economics*, vol 59, pp. 1-18.

Davis, S.J., Haltiwanger, J.C., Schuh, S. 1996. *Job Creation and Destruction*. MIT Press.

Dixit, A., Stiglitz, J. 1977. Monopolistic competition and optimum product diversity, *American Economic Review*. vol 67 (3), pp. 297-308.

Egebark, J. and Kaunitz, N. 2013. Do payroll tax cuts raise youth employment?, IFAU working paper 2013:27.

Ferrall, C. 2012. Explaining and Forecasting Results of the Self-sufficiency Project, *Review of Economic Studies* 79 (4): 1495-1526.

Flinn, C., 2006. Minimum Wage Effects on Labor Market Outcomes under Search, Bargaining, and Endogenous Contact Rates, *Econometrica* 74, pp. 1013-1062.

Gali, J., 2013. Note for a new guide to Keynes, wages aggregate demand and employment, *Journal of the European Economic Association*, vol 11(5), pp. 973-1003.

Galiani, S., Murphy, A., and Pantano, J, 2015. Estimating Neighborhood Choice Models: Lessons from a Housing Assistance Experiment. *American Economic Review*, 105(11): 3385-3415.

Garicano, J., Lelarge, C., and van Reenen, J., 2013. Firm Size Distortions and the Productivity Distribution : Evidence from France, NBER Working Papers 18841, National Bureau of Economic Research, Inc.

Gautier, P., Muller, P, van der Klaauw, B., Rosholm, M. and Svarer, M. 2012. Estimating Equilibrium Effects of Job Search Assistance,” *Tinbergen Institute Discussion Papers* 12-071/3.

Giroud, X., and Mueller, H., 2017. Firm Leverage, Consumer Demand, and Employment Losses During the Great Recession. *Quarterly Journal of Economics*, 132(1), pp. 271-316.

Goos, M., and Konings, J. 2007. The Impact of Payroll Tax Reductions on Employment and Wages: A Natural Experiment Using Firm Level Data, *LICOS Discussion Papers* 17807, LICOS - Centre for Institutions and Economic Performance, KU Leuven.

Gourio, F. and Roys, N., 2014. Size-Dependent Regulations, Firm Size Distribution, and Reallocation, *Quantitative Economics* 5, pp. 377-416.

Hall, R. 2017. High Discounts and High Unemployment, *American Economic Review*, 107(2), pp.305-330.

Hagedorn, M. and Manovskii, I., 2008. The Cyclical Behavior of Equilibrium Unemployment and Vacancies Revisited, *American Economic Review*, 98(4), pp. 1692-1706.

Hairault, J-O, Le Barbanchon, T. and T. Sopraseuth, 2015. The Cyclicalities of the Separation and Job Finding Rates in France, *European Economic Review*, 76, May 2015, pp.60-84.

Hamermesh, D. 2013. Does Labor Cost Affect Companies' Labor Demand, and Who Cares?, *IZA World of Labor*.

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

Jolivet, G., F. Postel-Vinay and J.-M. Robin, 2006, The Empirical Content of the Job Search Model: Labor Mobility and Wage Distribution in Europe and the U.S. *European Economic Review*, 50(4), pp. 877-907.

- Kaas, L., and Kircher, P. 2015. Efficient Firm Dynamics in a Frictional Labor Market, *American Economic Review*, 105(10), pp.3030-60.
- Kitao, S., A. Sahin, and J. Song, 2010. Subsidizing Job Creation in the Great Recession, Federal Reserve Bank of New York Staff Reports, No. 451, May.
- Kramarz, F., Michaud, M.L, 2010. The shape of hiring and separation costs in France. *Labour Economics*, 17, pp. 27–37.
- Kramarz, F. Philippon, T. 2001. The Impact of Differential Payroll Tax Subsidies on Minimum Wage Employment, *Journal of Public Economics*, 82, pp. 115-146.
- Lalive, R., Landaïs, C. and Zweimueller, J. 2013, Market Externalities from Large Unemployment Insurance Benefit Extension Programs, IZA discussion paper n°7650.
- Lichter, A., Peichl, A. and Siegloch, S., 2015, The Own-Wage Elasticity of Labor Demand: A Meta-Regression Analysis, *European Economic Review* 80, pp. 94-119.
- Lise, S, Seitz, S. and Smith, J., 2015. Evaluating search and matching models using experimental data, *IZA Journal of Labor Economics* 2015, 4:16.
- Manning, A., 2011, Imperfect Competition in Labour Markets, in O.Ashenfelter and D.Card (eds) *Handbook of Labor Economics*, volume 4, North-Holland, chapter 11, pp 973-1041.
- Moen, E. 1997. Competitive Search Equilibrium, *Journal of Political Economy*, 105, pp. 385-411.
- Moscarini, G. and Postel-Vinay, F. 2012. The Contribution of Large and Small Employers to Job Creation in Times of High and Low Unemployment, *American Economic Review*, 102(6), 2509-39.
- Nekarda, C., J. and Ramey, V., A. 2013. The Cyclical Behavior of the Price-Cost Markup, mimeo.
- Neumark, D. and Grijalva, D. 2013. The Employment Effects of State Hiring Credits During and After the Great Recession, NBER Working Papers 18928, National Bureau of Economic Research, Inc.
- OECD, 2010. *Employment Outlook*, chapter 1, OECD publishing, Paris.
- Perloff, J.M and Wachter, M.L. 1979. The New Jobs Tax Credit: An Evaluation of the 1977-78 Wage Subsidy Program, *American Economic Review*, American Economic Association, vol. 69(2), pp. 173-79.
- Posner, R. 2010. Subsidies to Small Business?, www.becker-posner-blog.com.
- Ravn, M., Schmitt-Grohé, S., and Uribe, M. 2006. Deep Habits. *The Review of Economic Studies*, 73(1), pp. 195-218.
- Rotemberg, J., J., Woodford, M. 1991. Markups and the Business Cycle, Chapter in National Bureau of Economic Research Macroeconomics Annual, Volume 6 (1991), Olivier Jean Blanchard and Stanley Fischer, editors (p. 63 - 140), MIT Press.
- Schoefer, B, 2015. The Financial Channel of Wage Rigidity, mimeo.
- Skedinger, P. 2014. Effects of Payroll Tax Cuts for Young Workers, Institute of Industrial Economics Working Paper No. 1031.
- Sianesi, B. 2008. Differential Effects of Active Labour Market Programs for the unemployed, *Labour Economics*, 15 (3), pp370-399.
- Stole, L and Zwiebel. L. 1996. Intrafirm Bargaining under Non-binding Contracts, *Review of Economic Studies*, vol 63, pp. 375-410.
- Todd, P. and Wolpin K. 2006. Assessing the Impact of a School Subsidy Program in Mexico: Using a Social Experiment to Validate a Dynamic Behavioral Model of Child Schooling and

Fertility, *American Economic Review*, American Economic Association, vol. 96(5), pp. 1384-1417.

Wilson, B., and Reynolds, S. 2005. Market Power and Price Movements over the Business Cycle. *The Journal of Industrial Economics*, 53(2), pp. 145-174.

Wise, D. 1985. Behavioral Model versus Experimentation: The Effects of Housing Subsidies on Rent. In *Methods of Operations Research 50*, ed. Peter Brucker and R. Pauly, pp. 441-89. Königstein: Verlag Anton Hain.

ONLINE APPENDIX

DATA ACCESS

The French employment registers (DADS) and the fiscal data (FICUS-FARE), used in this paper, can be accessed by researchers. Authorization must be obtained first from the *comité du secret*. The procedure is described at <https://www.comite-du-secret.fr>. Then researchers use a remote secure server (CASD) to work on the data.

The “hiring credit file” that contains information on the firms enrolled in the *zéro charges* program, is produced and owned by the French Public Employment Service (*Pôle emploi*). This is also the case for the vacancy data. Access to these data can be requested directly to *Pôle emploi*.

The French Labor Force survey is produced by France Statistics (*Institut National de la Statistique et des Etudes Economiques, Insee*). Researchers can access individual data from the French LFS directly through *Insee*, or through the *Quetelet network* (<http://www.reseau-quetelet.cnrs.fr>).

A Effects of the 2009 French hiring credit on wages

The hiring credit may raise individual net wages. It may also induce firms to hire workers with fewer skills at lower wages, since the hiring credit decreases with the wage as shown on Figure 1 in the main text. To evaluate the impact of the hiring credit on wages, we use our difference-in-differences approach, where the dependent variable is the difference in log wages.⁴⁸ Let w_{it} be the average hourly wage of workers in firm i in year t and \tilde{w}_{it-1} their average hourly wage in the previous year (if they worked), either in firm i , or in any other firm. Workers who did not work in the previous year are excluded⁴⁹. For each firm i and year t , the dependent variable is $\ln w_{it} - \ln \tilde{w}_{it-1}$ for all workers present in firm i on 30 November of year t . This variable allows us to compare the evolution of wage changes in small and medium-sized firms controlling for individual past wages. If the hiring credit did indeed have an impact on wages, that should be apparent for the entrants eligible for the hiring credit – i.e. workers hired during the current year, paid below 1.6 times the minimum wage, and who worked in the firm at least one month.

Figure A1 shows the evolution of the wages of these workers in the small and medium-sized firms over the years 2006-2009. Contrary to what we see for employment and hours of work, there is no break in the common trend in 2009. This suggests that the hiring credit had no impact on wages. This is confirmed by Table A1 which displays the difference-in-differences estimates for the wages of all workers, for the wages of incumbent workers paid below 1.6 times the minimum wage, and for the wages of entrants eligible for the hiring credit. In all cases, the estimates point to a null effect of the hiring credit on wages. This result is not surprising in the French context, where there is a high minimum wage and collective agreements that cover more than 90 percent of employees and that are most often binding for small firms.

⁴⁸Note that although the DADS is not a panel, it does provide the wage in the previous year for each worker.

⁴⁹This exclusion results in a lower number of observations than in Table 2.

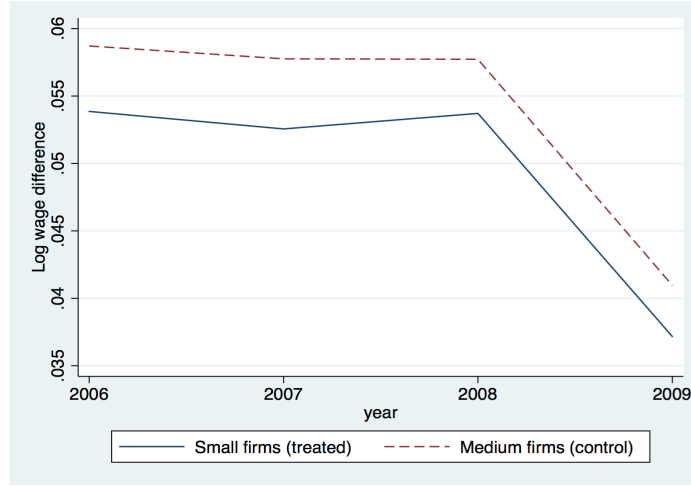


Figure A1: Average log wage difference of entrants eligible for the hiring credit in firms in the treated and control groups.

Note: Eligible entrants are workers hired during the current year, paid below 1.6 times the minimum wage and who worked at least one month in the firm. The average log wage difference for each group is $\frac{1}{N_i} \sum_i \ln w_{it} - \ln \tilde{w}_{it-1}$ where w_{it} is the average hourly wage of eligible entrants in firm i in year t and \tilde{w}_{it-1} their average hourly wage in the previous year, if they worked, either in firm i , or in any other firm; N_i is the number of firms in the group. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in the previous year (average from 1 December to 30 November).

Table A1: Difference-in-differences estimates of the impact of the hiring credit on wages in 2009

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
All wages	.000 (.001)	−.001 (.001)	.000 (.002)
Low wage incumbents	.000 (.001)	−.001 (.001)	.000 (.001)
Eligible entrants	.000 (.002)	.000 (.002)	−.001 (.002)
Nb. Observations	210,652	210,652	105,333

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

B Effects of the 2009 French hiring credit on churning and separations

Table 2 in the main text shows that the hiring credit has a positive, although non-significant, impact on the separation rate. Consistent with this result, the hiring credit has a bigger impact on the hiring rate than on employment growth, although the difference is not significantly different from zero. It may be suspected that this result reflects some strategic behavior of firms which might replace incumbent workers with new workers to benefit from the hiring credit.

Let us provide evidence which suggests that this is not the case. Using French data over the period 1987-1990, Abowd et al. (1999) estimate that each job created in a given year is associated with 3 hires and 2 separations. Davis et al. (2012) also find that hires rise more than one-for-one with job creation in the US. This relation indicates that a higher incidence of recently formed matches at more rapidly growing firms generates higher separation rates. There are two reasons for this. One is purely mechanical: at given quit rate, the separation rate, equal to the number of separations during the period divided by employment at the beginning of the period (or by the average of employment at the beginning and at the end of the period), increases when employment grows faster. Another reason flows from the fact that filling a job requires finding the right match with the right worker, which is not always the case with the first hire. Accordingly, if the hiring credit fosters job creation, it may also increase churning, even in the case where firms do not strategically raise their separations in order to hire new workers at lower cost.⁵⁰

The upper chart of Figure A2 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 rate⁵¹ by growth rate bins. Hires increase more than one-for-one with job creation in all firms. Over the period 2006-2008, the relation between hires and employment growth is similar in small-size and in medium-size firms.

If the hiring credit had induced employers to replace incumbent workers with new workers to benefit from the subsidy in 2009, the hiring rate, at a given employment growth rate, would have been higher in small firms, eligible for the hiring credit, than it was in medium-size firms not eligible for the hiring credit. The bottom panel of Figure A2 shows that this is not the case. The relation between hires and employment growth is similar in small-size and medium-size firms before and after 2009. This means that the hiring credit did not induce firms to increase labor turnover in order to benefit from the subsidy.

⁵⁰ 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).

⁵¹ 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 $t - 1$.

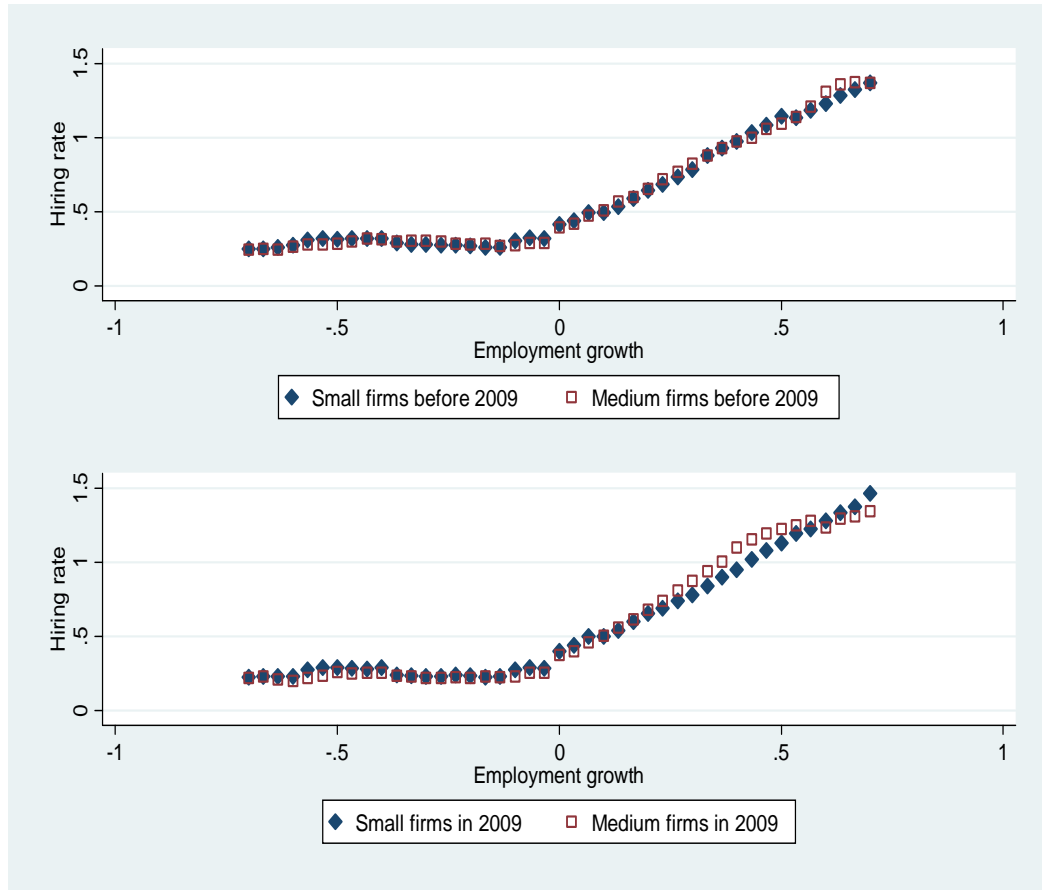


Figure A2: Hiring rate and employment growth rate in small size and medium size firms. Note: The upper chart displays the average of the mean hiring rate by employment growth rate bins over 2006-2008. The bottom chart displays the average hiring rate by employment growth rate bins in 2009. Dots represent 6-bin moving averages. Small size firms have 6-10 (excluded) full-time equivalent employees in the previous year. Medium size firms have 10-14 full-time equivalent employees in the previous year. Source: DADS.

C Robustness checks

In this appendix we perform a number of additional estimations to check the robustness of our baseline results. We run year placebo tests. We also consider placebo size cutoffs and changes in the bandwidth.

C.1 Year placebo tests

We perform a series of year placebo tests using cohorts from 2006 to 2008. We use the specification of column 3 in Table 2 as if the policy had been implemented in December 2006 (using cohorts 2006 and 2007) or December 2007 (using cohorts 2006, 2007 and 2008). Table A2 shows that employment, hours, hires and separations of the treatment and the control groups did not evolve differently either in 2007 or in 2008, contrary to 2009 when *zéro charges* was introduced. These results reinforce the relevance of the common trend assumption. They also rule out the possibility that our estimates of the impact of *zéro charges* are driven by reversion to the mean.

We also perform year placebo tests of our alternative IV strategy. We consider the following reduced-form equation on three different samples (cohorts 2007, 2008 and 2009):

$$Y_{jkt} = \alpha + \delta E_{jk}^{t-1} + \beta X_{jkt} + v_{jkt} \quad (C1)$$

where Y_{jkt} is the employment growth rate between November $t - 1$ and November t averaged across eligible firms (with 6 to 10 employees in year t) in commuting zone j and producing in sector k , E_{jk}^{t-1} is the share of eligible hires (below 1.6 the minimum wage) in small firms in $t-1$, X_{jkt} is the set of controls at the cell level. Recall that E_{jk}^{t-1} is our instrument. X_{jkt} comprises sector dummies, and cell-level measures of firms' age, of part-time incidence, of sales/value-added/dividend/interest payments per employee in the previous year and average financial leverage ratio over the pre-policy period from 2006 to 2008. It also includes the lagged outcome Y_{jkt-1} , and the employment growth rate of the control group in the same cell -firms with size between 10 and 14. Table A3 reports the estimates of the coefficient δ of the instrument. It is small and non statistically significant when the equation is estimated as if the policy had taken place in 2007 or 2008 (Columns (1) and (2)). In Column (3), it is as expected larger and statistically significant at the 5% level. Overall Table A3 confirms our alternative identifications strategy.

Table A2: Difference-in-differences estimates for various labor market outcomes in placebo years

Placebo	December 2006	December 2007
Cohorts	2006-2007	2006-2008
Covariates	Yes	Yes
Employment growth	−.002 (.002)	.001 (.002)
Hours growth	−.003 (.002)	.001 (.002)
Hiring rate	−.001 (.005)	−.004 (.003)
Separation rate	.001 (.005)	−.005* (.003)
Survival rate	.002 (.001)	.001 (.001)
Nb. Observations	179,381	270,593

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

Table A3: Placebo-year estimates of the impact of the hiring credit on employment growth rates in the IV strategy

	(1)	(2)	(3)
Placebo	December 2006	December 2007	No
Main cohort	2007	2008	2009
Covariates	Yes	Yes	Yes
	Employment growth		
Share of eligible hires in previous year	.009 (.013)	.015 (.012)	.026** (.012)
R^2	.060	.054	.048
Nb. Observations	1, 647	1, 646	1, 655

Source : DADS (Insee). Note : This table presents the reduced-form estimate of our IV strategy (last column, year=2009) and placebo reduced-form estimates from 2007 to 2008 (Columns (1) to (2)). Estimates are thus based on cross-cell differences in the eligible hires -low-wage hires lasting more than one month- in the previous year. Each cell corresponds to one employment pool and one sector. Within each cell the group of interest comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). Each cell is weighted by its relative employment size as measured among 6-10 employees firms. We consider as outcome the growth rate of employment between 30 November of year t-1 and year t. Covariates include the distribution of firms' age in the cell, the share of firms with sales below 2 million euros in the previous year, the share of part-time workers in the previous year, the sales/value-added/interest/dividend payments in the previous year, the financial leverage ratio (debt over equity) in the previous year, the lagged outcome, and the employment growth rate of the control group in the same cell -firms with size between 10 and 14. Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

C.2 Size placebo tests

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

If firms of size between 6 and 10 employees in 2008 behaved differently in 2009 from firms of size between 10 to 14 employees because of differences in size and not because of the hiring credit, we would expect firms with 13 to 16 employees to behave differently from firms with 16 to 19 employees.⁵² Figure A3 compares the average employment growth rate for firms with 13 to 16 (excluded) employees in previous year and firms with 16 to 19 employees in previous year. The difference in employment growth across these groups does not change in 2009. This result is confirmed by the difference-in-differences estimates for these two groups of firms.⁵³ This indicates that the difference in employment growth across our treatment and control groups does not stem from differences in behavior due to differences in size.

C.3 Changing the bandwidth

Our benchmark difference-in-differences estimates are based on a sample which includes some treated firms featuring a lower take-up than others (between 8 and 10 employees), and a residual take-up among control firms (between 10 and 12 employees, see Figure 4). Table A4 presents the estimates for different bandwidths. The difference-in-differences estimates are higher when the treatment group includes firms with higher take-up rates (column 1 to 3). Column 4 shows that the estimates are also higher when the control group excludes firms with residual take-up. The corresponding estimates of elasticity of employment with respect to the change in labor cost induced by *zéro charges* equal -1.43 and -1.95 when the bandwidth goes from 5 to 15 employees and falls in the range $[5,8]$ - $[13,16]$ employees, respectively.⁵⁴ All in all, these results suggest that our benchmark estimate of the elasticity of employment with respect to the change in labor cost induced by the hiring credit is conservative: it is likely a lower bound for the elasticity that might be larger than 4 in absolute value.

⁵²We avoid making comparisons using firms with 10 to 12 employees in the previous year, only a tiny fraction of which have benefited from the subsidy as shown by figure 4.

⁵³We do not present these estimates to save space. The results are available upon request.

⁵⁴As explained in footnote 28 these elasticities imply labor demand elasticity with respect to the wage equal to $-1.43 = -5.5 \times 0.26$ and $-1.95 = -7.5 \times 0.26$ respectively.

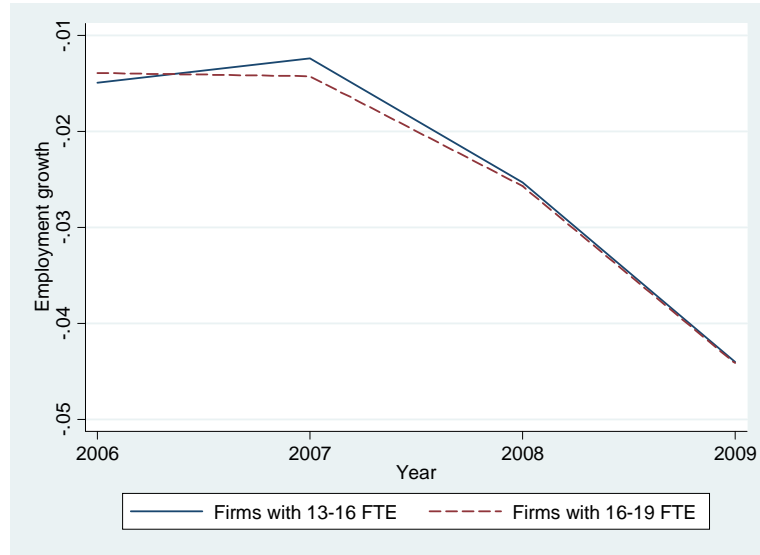


Figure A3: Average employment growth rates in placebo groups.

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

Table A4: Difference-in-differences estimates of the impact of the hiring credit in 2009 on various labour market outcome with a varying bandwidth.

Size bandwidth	7-13	6-14	5-15	[5,8]-[13,16]
Employment growth	.005*** (.002)	.008*** (.002)	.011*** (.002)	.015*** (.002)
Hours growth	.006*** (.002)	.009*** (.002)	.012*** (.002)	.016*** (.002)
Hiring rate	.013** (.005)	.012*** (.004)	.015*** (.004)	.016*** (.005)
Separation rate	.007 (.005)	.004 (.004)	.004 (.004)	.001 (.005)
Survival rate	.000 (.001)	.000 (.001)	.000 (.001)	.000 (.001)
Nb. Observations	284, 483	406, 468	550, 524	364, 029

Source : DADS (Insee). Note : this Table displays the DID estimates varying the bandwidth (in columns). The sample contains all available cohorts (2006-2009), and we include covariates presented in table 2. The 2nd column is identical 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.

C.4 Equilibrium effects beyond the commuting zone \times sector cells

Our benchmark analysis of equilibrium effects restricts their scope to commuting zone \times sector cells. In this appendix, we show that there are no evidence that the 2009 hiring credit triggered equilibrium effects beyond these boundaries. We augment model (5) in the main text with the share of subsidized hires in the same commuting zone, but in different sectors, and with the share of subsidized hires in the same sector, but in different commuting zones. The augmented model writes:

$$Y_{jk} = \alpha + \gamma_1 S_{jk} + \gamma_2 S_{j(-k)} + \gamma_3 S_{(-j)k} + \beta X_{jk} + u_{jk} \quad (\text{C2})$$

where Y_{jk} stands for the employment growth of firms with 10 to 20 employees in 2008 in employment pool j and sector k , S_{jk} is the ratio of subsidized hires in 2009 to all hires observed in the same year among firms with 0 to 20 full-time equivalent employees in 2008, $S_{j(-k)}$ is the number of subsidized hires in 2009 divided by all hires in the same year in firms with 0 to 20 employees operating in employment pool j and belonging to sectors other than k ; and $S_{(-j)k}$ is the number of subsidized hires in 2009 divided by all hires in 2008 in firms with 0 to 20 employees operating in employment pools other than j but belonging to sector k . The term u_{jk} is a residual. In this setting S_{jk} and $S_{j(-k)}$ together account for the equilibrium effects that may occur within the employment pool j whatever the sector, while $S_{(-j)k}$ accounts for the equilibrium effects that could arise from interactions with firms in the same sector as the unit under consideration but outside the employment pool j . We also include a number of cell-specific controls X_{jk} as in the main model. If the sum of coefficients γ is significantly different from zero, this indicates the presence of equilibrium effects. As in the main text, the ratios S of subsidized hires in 2009 are instrumented by the corresponding shares of eligible hires in 2008 among all hires the same year (when the subsidy was not yet implemented).

Column 1 of Table A5 present the results of the OLS estimation of equation (5) when controls are not included. The sum of coefficients γ is then statistically significant different from 0. However, the level of significance largely increases when the shares are instrumented, even without further controls (see Column 3). As soon as controls are included, the sum of coefficients γ is no longer statistically significant, whether they are estimated according to an OLS or they are obtained with an IV strategy. Table A6 shows the estimation results of the first stage equations and confirms that our instrument correlates well with the shares S . All in all, these results suggest that the hiring credit had no impact on the medium-sized firms, even in markets larger than the commuting zone \times sector cell.

Table A5: Cross-unit estimates of equilibrium effects on the growth rates of employment in 2009 among firms with 10-20 employees, based on OLS and IV

	(1)	(2)	(3)	(4)
	OLS		IV 2SLS - Second stage	
Covariates	No	Yes	No	Yes
S_{jk}	.068** (.027)	.041 (.025)	.111* (.067)	.069 (.068)
$S_{j(-k)}$	-.040 (.030)	-.032 (.029)	-.045 (.072)	-.019 (.061)
$S_{(-j)k}$.095** (.044)	.021 (.452)	.063 (.074)	-.357 (.500)
Test $S_{jk} + S_{j(-k)} + S_{(-j)k} = 0$ (p-value)	.007	.946	.089	.542
R^2	.026	.105		
Nb. Observations	1, 686	1, 686	1, 686	1, 686

Source : DADS (Insee). Note: The dependent variable is the average growth rate of employment over 12 months from 1 December 2008 to 30 November 2009 in each employment pool x sector unit, among firms with 10 to 14 full-time equivalent employees in the previous year. The independent variables are the ratios of subsidized hires, which correspond to three variables: (1) the number of subsidized hires in 2009 divided by the number of hires in the employment pool x sector unit in 2008, among firms with 0 to 14 full-time equivalent employees in the previous year; (2) the same ratio but measured among firms belonging to the same employment pool and to other sectors than the one considered for the dependent variable; (3) the same ratio but measured among firms belonging to the same sector and to other employment pools than the one considered for the dependent variable. In the IV regressions, these ratios are instrumented by the corresponding shares of eligible hires in 2008, i.e. the ratios of the eligible hires in 2008 to total hires in 2008, among firms with 0 to 14 full-time employees in the previous year. As covariates, we include dummies for distribution of firms' age, the share of firms with sales below 2 million euros in the previous year, the share of part-time workers in the previous year, change in the survival rate of firms within the cell between 2008 and 2009, as well as the employment growth rate in 2009 in the same sector as the cell but in employment zones located nearby. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard errors in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table A6: First stage of the instrumental variable estimates of extended equilibrium effects.

Covariates	IV 2SLS - First stage					
	No	Yes	No	Yes	No	Yes
	S_{jk}		$S_{j(-k)}$		$S_{(-j)k}$	
E_{jk}	.404*** (.025)	.341*** (.028)	.183*** (.047)	.062*** (.020)	.004 (.004)	.003*** (.001)
$E_{j(-k)}$.441*** (.037)	.345*** (.031)	.693*** (.026)	.647*** (.027)	-.000 (.009)	-.011*** (.003)
$E_{(-j)k}$.286*** (.027)	.239 (.237)	-.057* (.030)	-.464* (.248)	.617*** (.004)	.554*** (.052)
Test $E_{jk} = E_{j(-k)} = E_{(-j)k} = 0$ (p-value)	.0000	.0000	.0000	.0000	.0000	.0000
Nb. Observations	1, 686	1, 686	1, 686	1, 686	1, 686	1, 686

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

D Computation of net cost per job created

In order to evaluate the savings permitted by *zéro charges* on social benefits, we use a survey conducted by the Public Employment Service *Pôle Emploi* in November - December 2009 on the beneficiaries of *zéro charges*. *Pôle Emploi* interviewed 3,083 firms and a total of 3,996 employees who benefited from *zéro charges* between 1 January and 30 June 2009, out of 270,755 beneficiaries recorded during that period. The survey collected the gender, age, and education of the recruits, the main reason for recruitment (creation of a new job, replacement of another worker, contract renewal, temporary needs, etc.), as well as the type of contract (permanent or temporary), the profession, the monthly wage and the sector of firms. More interestingly, it also included a question on the personal situation of workers immediately *before* the recruitment took place: employed, registered or unregistered unemployed, in training or at school, on sick or maternal leave, or inactive. The corresponding breakdown is presented in Table A7 for workers less than 26 years old (64% of the recruits) and those 26 years old or more. We use this information to estimate the savings on social benefits induced by the jobs created by *zéro charges*. To do so we compute the social benefits that would have been received by the beneficiaries if they had remained on the dole.

Table A7: The situation of workers hired with *zéro charges*, immediately before recruitment

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

Source : Pole Emploi.

In 2009, the average unemployment insurance benefit (called *Allocation de Retour à l'Emploi*) was 970 euros per month, but only 50% of the registered unemployed received it (DARES, 2012). About 10% received unemployment assistance (called *Allocation de Solidarité Spécifique*, a means tested scheme) which amounted to 450 euros. Another 10% received the minimum income (called *Revenu de Solidarité Active*, also about 450 euros for a single person without children), and 30% did not receive any benefit. This gives a (weighted) average cost of 575 euros for the registered unemployed. As for those not registered, they do not receive unemployment benefits as registration is a prior condition. But they are eligible for the minimum income of 450 euros per month, which inactive people are as well, for which studies show a typical take-up rate of 2/3.⁵⁵ This provides an average cost of 300 euros per month for the unregistered unemployed and the other inactive individuals, but only for those 26 years old or older, since younger unemployed / inactive people are not eligible for this minimum income scheme. Students may be eligible for scholarships, but these are rather rare. The main benefit for students is one of the three main housing benefits schemes, the average amount of which is about 200 euros per month. We apply the same take-up of 2/3, as for the minimum income, which gives an average benefit of 133

⁵⁵See http://www.social-sante.gouv.fr/IMG/pdf/1_Le_non-recours_au_rSa_et_ses_motifs.pdf

euros per month for students. For trainees, there is a specific benefit (called *ARE formation*) for those unemployed and eligible for the insurance benefit, which was 975 euros on average in 2009. Since only about half of the unemployed are eligible for the insurance benefit, we apply a take-up rate of 50%, which gives a monthly cost of about 485 euros. There might be other benefits for non-employed trainees but they are scarcer and we neglect them. Finally, we consider that, in the absence of the jobs created by *zéro charges*, those employed immediately before being hired on these jobs would have been unemployed otherwise, and would then have received the same average benefit as the registered unemployed (since they would have just ended an employment period, they would probably have registered rather than forgo job search support and unemployment benefits). Adding all these benefits, and using the weights of the various populations (less or more than 26 years old, and by status), as provided in Table A7, gives an average benefit per worker of 460 euros per month. To these savings one must add the social contributions paid by the additional *employees* hired on jobs created by *zéro charges*, which amount to 23% of gross wages, or about 235 euros per month on average given the observed hiring wages. All in all, each job created by *zéro charges* generates monthly net savings of 695 euros. This estimate excludes the cost of social in-kind services (such as counselling, case-management and health services) typically more important for unemployed and inactive persons than for those in employment. It also takes into account only the basic amount of the minimum income, excluding all supplements for couples and children.

E Estimation of the search and matching model

This appendix presents the estimation of the parameters of equations (7) and (8).

E.1 Matching technology

We first estimate the parameters of the matching function m_t : \bar{m} and μ . Taking logs of the definition of the matching technology, we obtain the following underlying structural relation :

$$\log(H/U) = (1 - \mu) \log(V/U) + \nu$$

where $\nu = \log \bar{m}$. We use variations across employment pools over time to identify the parameter μ . Let us denote j the employment pool (commuting zone). Yearly data on unemployment stocks (U_{jt}) at the employment pool level are computed by the French statistical institute (*Insee*) using the French Labor Force Survey (*Enquête Emploi*). Vacancies data (V_{jt}) come from the French Employment Service (*Pôle emploi*). *Pôle emploi* posts vacancies that firms send to the Agency. This is a free service and *Pôle emploi* estimates that they deal with almost 50% of the total of French vacancies. Combining these data, we measure the tightness θ_{jt} at the employment pool level. Hiring data (H_{jt}) are observed in our main dataset *DADS*. Let us denote f_{jt} the yearly job finding rate (H_{jt}/U_{jt}). We estimate the following equation

$$\log f_{jt} = a_1 \log \theta_{jt} + \sum_t b_t 1[\text{year} = t] + c_j + \nu_{jt} \quad (\text{E3})$$

where j is one of the 348 employment pools (commuting zones) and the year t varies from 2006 to 2009, as in the main text. The estimation controls for year dummies and employment pool

fixed effects (c_j). The equation is estimated both by standard (within) OLS and IV regression. As an instrument for the current labor market tightness we use past values of the labor market tightness. This addresses potential endogeneity issues (see Borowczik et al. 2013). For instance, an improvement in the matching technology (increase in \bar{m}) can increase θ and reduce the unemployment rate.

Table 7 shows the estimates of the coefficient a_1 using OLS in column 1 and using IV in column 2. Both estimates are highly significant. Taking the IV estimation as our preferred estimate, μ , the elasticity of the matching function with respect to the unemployment rate, amounts to 0.45.

E.2 Revenue function and vacancy posting costs

We now estimate the parameters of the revenue function and the search cost: A, α, c_V . Assuming that c_V/m is small with respect to w the labor demand in period t can be written:

$$\log R_L(L_t) = \log(w_t) + \frac{c_V}{w_t} \left[\frac{1}{m_t} - \frac{\beta(1 - q_t)}{m_{t+1}} \right] - \zeta_t, \quad (\text{E4})$$

where $\zeta_t = \log(A_t)$. Estimating directly (E4) is plagued with well-known endogeneity issues between the wage level and the productivity level. Relying on our specific exogenous policy shock, we set out an alternative structural estimation. In 2009, when the hiring credit is implemented, the structural demand equation can be written:

$$\log R_L(L_t) = \log(w_t - \sigma) + \frac{c_V}{w_t - \sigma} \left[\frac{1}{m_t} - \frac{\beta(1 - q_t)}{m_{t+1}} \right] - \zeta_t. \quad (\text{E5})$$

We can then compute the first difference between equation (E5) (in 2009) and equation (E4) (in 2008) for firms eligible for the hiring credit.

$$\Delta \log R_L(L_t) = \log \left(\frac{w_t - \sigma}{w_{t-1}} \right) + c_V \left(\frac{1}{w_t - \sigma} \left[\frac{1}{m_t} - \frac{\beta(1 - q_t)}{m_{t+1}} \right] - \frac{1}{w_{t-1}} \left[\frac{1}{m_{t-1}} - \frac{\beta(1 - q_{t-1})}{m_t} \right] \right) - \Delta \zeta_t,$$

where Δ is the difference operator: $\Delta x_t = x_t - x_{t-1}$. We take a first order approximation of $\log R_L(L_t)$ in the neighborhood of $L_t = L_{t-1}$ and obtain that $\Delta \log R_L(L_t) = \alpha \Delta \log L_t$. This simplifies the left-hand side of the above expression. Then, as σ/w_{t-1} is negligible compared to 1, we can simplify the right-hand side and get the the following structural equation:

$$\alpha \Delta \log L_t = -\frac{\sigma}{w_t} + \log \left(\frac{w_t}{w_{t-1}} \right) + \frac{c_V}{w_t} \left(\frac{1}{1 - \sigma/w_t} \left[\frac{1}{m_t} - \frac{\beta(1 - q_t)}{m_{t+1}} \right] - \frac{w_t}{w_{t-1}} \left[\frac{1}{m_{t-1}} - \frac{\beta(1 - q_{t-1})}{m_t} \right] \right) - \Delta \zeta_t. \quad (\text{E6})$$

We aggregate the analysis at the employment pool \times sector level. This is the finer level at which we observe the duration of vacancies. We again use administrative data on the vacancies posted at the Public Employment Service. We then compute the average duration of vacancies ($1/m_{jkt}$) posted by firms of commuting zone j in sector k . We compute the average wage w_{jkt} and the average job separation rates q_{jkt} from the DADS database. The amount of the hiring credit obtained by small firms σ_{jk} comes from the “hiring credit file” which contains information

on the firms enrolled in the *zéro charges* program. Assuming that $\beta = 0.95$, we compute – from these aggregates – the difference in hiring costs:

$$\Delta HirCost_{jkt} = \left(\frac{1}{1 - \sigma_{jk}/w_{jkt}} \left[\frac{1}{m_{jkt}} - \frac{\beta(1 - q_{jkt})}{m_{jkt+1}} \right] - \frac{w_{jkt}}{w_{jkt-1}} \left[\frac{1}{m_{jkt-1}} - \frac{\beta(1 - q_{jkt-1})}{m_{jkt}} \right] \right).$$

We estimate the following econometric equation:

$$\Delta \log L_{jkt} = a_1 \frac{\sigma_{jk}}{w_{jkt}} + b_1 \Delta HirCost_{jkt} + c_1 \Delta \log w_{jkt} + c_2 \Delta \log L_{jkt}^{med} + c_3 X_{jkt} + \nu_{jkt} \quad (E7)$$

where j stands for employment pool and k for sector, and t is year 2009. $\Delta \log L_{jkt}^{med}$ is the employment growth rate of medium-sized firms between 2008 and 2009 in employment pool j and sector k . X_{jkt} is a set of covariates characterizing the population of small firms in the corresponding cell.

The above specification directly relies on variation in the labor cost due to the hiring credit to estimate the labor demand elasticity, as such it circumvents the classical endogeneity issue of the wage. However, it can still be the case that the take-up rate, and consequently the amount of hiring credit, is correlated with local shocks in 2009 or unobserved factors of the cell associated with employment growth. For example, a positive productivity shock stimulates both employment and the average amount of subsidy (via a greater number of subsidized hires). This is the same endogeneity issue as in the IV identification strategy presented in the main text. The identification strategy is then as follows: first, we control for market-level covariates and market-level employment growth shift-share predictions, and for the employment growth in the control group; second, we use the average values of the share of eligible hires, of the duration of job vacancies and of the job separation rate of firms with 6 to 10 full-time employees over 2006-2008, as instruments for the amount of the subsidy received in 2009 and for the hiring cost. Estimates of a_1 and b_1 using this strategy are shown in table 8. They are significantly different from zero.

The mapping between the coefficients of the econometric model and the parameters of the structural equation yields: $a_1 = -\frac{1}{\alpha}$ and $b_1 = \frac{1}{\alpha} \frac{c_V}{w_t}$. This shows that the parameters α and $\frac{c_V}{w_t}$ are identified as $\alpha = -\frac{1}{a_1}$ and $\frac{c_V}{w_t} = -\frac{b_1}{a_1}$. Taking the IV estimation of Table 8 as our preferred estimate (column 2), we obtain $\hat{\alpha} = -.964$, $\hat{c}/\hat{w} = .058$.

F Employment elasticities

This appendix presents the computation of the elasticity of employment with respect to changes in the labor cost induced by the hiring credit in different contexts. First, we show how these elasticities can be computed from the parameters of the structural model where the wage is either exogenous or endogenous. Second, we introduce aggregate shocks in the model to analyze the cost per job created of countercyclical hiring credits.

F.1 Elasticity of employment with exogenous wage

F.1.1 Micro elasticity of employment with exogenous wage

This appendix presents the model of the firm used to compute the elasticity of employment with respect to the average labor cost per worker and the cost per job created induced by a hiring credit. We consider a discrete time economy with a large number of labor pools or “islands” in which a representative firm produces a differentiated product with labor only as in the Dixit and Stiglitz (1977) model. Each firm produces a differentiated product in quantity $Y_{i,t}$ sold at price $P_{i,t}$ at date t and faces a demand for its product of the form:

$$Y_{i,t} = P_{i,t}^{-E}, E > 1.$$

where E is the price elasticity of demand. The demand function is derived from the maximization problem of a representative consumer who maximizes the discounted utility of the aggregate consumption index:

$$C_t = \left[\sum_{i,t} Y_{i,t}^{\frac{E-1}{E}} \right]^{\frac{E}{E-1}}$$

subject to his budget constraint. Let us assume that the production function of the firm is:

$$F(L_{i,t}) = B_t L_{i,t}^{1+\chi}, \text{ with } B_t > 0.$$

We can write the revenue function of the firm:

$$A_t R(L_{i,t}) \equiv P_{i,t} F(L_{i,t}) = \frac{A_t}{1+\alpha} L_{i,t}^{1+\alpha}.$$

where

$$\alpha = \frac{\chi(E-1)-1}{E}; A_t = \frac{(\chi+1)(E-1)}{E} B_t^{\frac{E-1}{E}}$$

Henceforth, subscript i is discarded when not necessary for the sake of simplicity. The firm needs to post vacant jobs to hire workers. A vacant job costs c_V units of output per period. In each period, the sequence of decisions is as follows: 1) an exogenous proportion q_{t-1} of workers quit the firm; 2) vacant jobs are posted; 3) workers are hired; 4) production takes place and wages are paid. A vacant job posted in period t is matched with a worker with probability $m_t \in [0, 1]$ in the period and remains vacant with probability $1 - m_t$. The probability to fill a vacant job is determined by a matching function: $m(\theta_t) = \bar{m} \theta_t^{-\mu}$, where $\theta_t = V_t/U_{t-1}$, equal to the ratio of the number of job vacancies V_t over the number of unemployed workers U_{t-1} , denotes the labor market tightness; \bar{m} is a positive parameter and μ belongs to the interval $(0, 1)$.

Let us denote by w_t the wage in period t and by $\Pi(Z_t, L_{t-1})$ the value function of the firm, where $Z_t = (A_t, w_t, m_t, q_{t-1})$. Let β denotes the discount factor and \mathbb{E}_t the expectation operator. The value function of the firm satisfies

$$\Pi(Z_t, L_{t-1}) = \max_{V_t} A_t R(L_t) - w_t L_t - c_V V_t + \beta \mathbb{E}_t \Pi(Z_{t+1}, L_t)$$

subject to the law of motion of employment:

$$L_t = (1 - q_{t-1})L_{t-1} + m_t V_t \tag{F8}$$

For the sake of simplicity, it is assumed that the constraint $V_t \geq 0$ is never binding. The first order condition is

$$A_t R_L(L_t) - w_t + \beta \mathbb{E}_t [\Pi_L(Z_{t+1}, L_t)] = \frac{c_V}{m_t} \quad (\text{F9})$$

The envelope theorem together with the first order condition (F9) implies that in every period

$$\Pi_L(Z_t, L_{t-1}) = \frac{(1 - q_{t-1})c_V}{m_t} \quad (\text{F10})$$

Thus, the first order condition implies that employment is defined by

$$A_t R_L(L_t) = w_t + \frac{c_V}{m_t} - \beta \mathbb{E}_t \left[\frac{(1 - q_t)c_V}{m_{t+1}} \right]$$

This equation implies that the elasticity of employment with respect to the contemporaneous wage is, in the neighborhood of steady state:

$$\varepsilon = \frac{dL_t}{dw_t} \frac{w_t}{L_t} = \frac{w}{\alpha \left(w + \frac{1 - \beta(1 - q)}{m} c_V \right)} \quad (\text{F11})$$

where $\alpha = LR_{LL}(L)/R_L(L)$ stands for the elasticity of the marginal productivity of labor with respect to labor. Now, assume that there is a non-anticipated temporary hiring subsidy in period t denoted by σ_t . The profit in period t is

$$A_t R(L_t) - w_t L_t - (c_V - m_t \sigma_t) V_t \quad (\text{F12})$$

This implies that the first order condition in period t for the optimal choice of V_t is:

$$A_t R_L(L_t) = w_t - \sigma_t + \frac{c_V}{m_t} - \beta \mathbb{E}_t \left[\frac{(1 - q_t)c_V}{m_{t+1}} \right] \quad (\text{F13})$$

The average labor cost per worker in a firm with L_t workers and H_t hires that gets a hiring credit σ_t per hire boils down to:

$$\psi_t = w_t - \sigma_t \frac{H_t}{L_t} \quad (\text{F14})$$

The elasticity of employment with respect to the average labor cost per worker when the change in labor cost is due to the temporary hiring subsidy σ_t , is:

$$\varepsilon_\sigma = - \frac{dL_t}{d\sigma_t} \frac{d\sigma_t}{d\psi_t} \frac{\psi_t}{L_t}$$

Equations (F11), (F13) and (F14) imply, in the neighborhood of steady state and $\sigma = 0$, where $\psi = w$, that:

$$\varepsilon_\sigma = \frac{L}{H} \varepsilon \quad (\text{F15})$$

In order to interpret this expression when the take-up of the hiring credit is smaller than one, let us denote by $\mathcal{L}_t = \bar{L}_t + L_t$ total employment where \bar{L}_t stands for employment of firms that do not benefit from the hiring credit and L_t for employment of firms that benefit from the hiring credit. Assuming that the wage is identical in all firms, the average cost per employee

becomes $\psi_t = w_t - \sigma_t \frac{H_t}{L_t}$ where H_t denotes the hires of firms with positive take-up. It is easily checked that we get, in the neighborhood of steady state and $\sigma = 0$:

$$\varepsilon_\sigma = -\frac{d\mathcal{L}}{d\sigma} \frac{d\sigma}{d\psi} \frac{\psi}{\mathcal{L}} = \frac{L}{H} \varepsilon$$

where H stands for the number of hires of firms with positive take-up. Let us denote η the corresponding ratio of subsidized hires over employment H/L .

We can now write the employment elasticity with respect to a change in labor cost due to a temporary or permanent hiring credit when wages are exogenous and only a small fraction of firms are targeted (micro elasticity):

$$\begin{aligned} \varepsilon_\sigma(\text{micro}, \text{temp}, \text{exo}) &= \frac{1}{\eta\alpha \left(1 + \frac{c_V}{m(\theta)w} [1 - \beta(1 - q)]\right)} \\ \varepsilon_\sigma(\text{micro}, \text{perm}, \text{exo}) &= \frac{1}{\alpha \left(1 + \frac{c_V}{m(\theta)w} [1 - \beta(1 - q)]\right)} \end{aligned}$$

Let us now determine the cost per job created by a temporary subsidy on all wages in period t and by the hiring subsidy σ_t in the neighborhood of steady state and $\sigma_t = 0$. The cost per job created by the subsidy which decreases all wages by an amount $-dw$ is:

$$c = -\frac{Ldw}{dL} = -\frac{w}{\varepsilon} \quad (\text{F16})$$

The cost per job created by the hiring credit is equal to

$$c_\sigma = \frac{Hd\sigma}{dL} = \frac{H}{L} \frac{w}{\frac{dL}{d\sigma} \frac{w}{L}} \quad (\text{F17})$$

Since $\frac{dL}{d\sigma} = -\frac{dL}{dw}$ (see equation (F13)) and $\varepsilon = \frac{H}{L} \varepsilon_\sigma$ (see equation (3)), the cost per job created by the hiring credit is

$$c_\sigma = -\frac{w}{\varepsilon_\sigma}$$

F.1.2 Macro elasticity of employment with exogenous wage

This appendix computes the macro elasticity of employment with respect to the change in the contemporaneous labor cost in the neighborhood of steady state of the search and matching model presented in section 7.2.1. We first compute the micro elasticity with respect to a temporary change in the wage w_t . The equilibrium values of employment and of the labor market tightness in period t are given by equations (7) and (8), which can be written, in the neighborhood of steady state, denoting without time subscript the steady state values of the unemployment rate u , of the labor market tightness θ and of the job separation rate q :

$$A_t R_L(L_t) = w_t + \frac{c_V}{\bar{m}\theta_t^{1-\mu}} - \beta(1 - q) \frac{c_V}{m(\theta)} \quad (\text{F18})$$

$$1 - L_t = u + q(1 - u) - \bar{m}\theta_t^{1-\mu}u \quad (\text{F19})$$

where $L_t = 1 - u_t$. Differentiation of these equations yields the macro elasticity of contemporaneous employment with respect to labor cost when there is a temporary change in the wage w_t . Then, applying relation (3) which states that $\varepsilon_\sigma = \varepsilon/\eta$, where⁵⁶ $\eta = H/L$ we get the macro elasticity of contemporaneous employment with respect to labor cost when there is a temporary change in labor cost induced by a temporary hiring credit in a context where the wage is exogenous:

$$\varepsilon_\sigma(\text{macro, temp, exo}) = \frac{1}{\eta\alpha \left[1 + \frac{c_V}{m(\theta)w} \left([1 - \beta(1 - q)] - \frac{\mu}{q\alpha(1-\mu)} \right) \right]} \quad (\text{F20})$$

where $\alpha = LR_{LL}(L)/R_L(L)$.

Similarly, the macro elasticity of steady state employment with respect to labor costs when the change in labor cost is induced by a permanent hiring credit in a context where the wage is exogenous, is:

$$\varepsilon_\sigma(\text{macro, perm, exo}) = \frac{1}{\alpha \left[1 + \frac{c_V}{w} \frac{[1 - \beta(1 - q)]}{m(\theta)} \left(1 - \frac{\mu}{(1-\mu)\alpha u} \right) \right]} \quad (\text{F21})$$

F.2 Elasticity of employment with wage posting

This appendix presents the impact of the hiring credit in a directed search and matching model with wage posting in the spirit of Moen (1997) described in the main text. We characterize the steady state equilibrium when directed search and wage posting is integrated in the model of appendix F.1 in a first step. Then, we analyze the impact of temporary and permanent hiring credits.

F.2.1 Stationary equilibrium without hiring credit

The hypothesis of directed search by workers and perfect mobility implies that the expected utility of an unemployed person is the same in all labor pools, so it will simply be denoted by $W_{u,t}$. Assuming further that the job destruction rate q_t is identical in each labor pool, the expected utility $W_{ei,t}$ of a person employed in labor pool i in period t satisfies:

$$W_{ei,t} = w_{i,t} + \beta [(1 - q_t)W_{ei,t+1} + q_t W_{u,t+1}] \quad (\text{F22})$$

If the instantaneous gain b of an unemployed person is the same everywhere, the expected utility $W_{u,t}$ of a person in search of work satisfies:

$$W_{u,t} = b + \beta [\theta_{i,t+1} m(\theta_{i,t+1}) W_{ei,t+1} + (1 - \theta_{i,t+1} m(\theta_{i,t+1})) W_{u,t+1}] \quad \forall i, t \quad (\text{F23})$$

Differentiating the previous equation (holding $W_{u,t}$ constant) defines a relation between $W_{ei,t}$ and $\theta_{i,t}$:

$$\frac{\partial \theta_{i,t}}{\partial W_{ei,t}} = \frac{-\theta_{i,t}}{(1 - \mu)(W_{ei,t} - W_{u,t})} \quad (\text{F24})$$

⁵⁶Since all firms are identical in the model, H/L can be interpreted indifferently as the average hiring rate of all firms or of the firms that benefit from the hiring credit. In the simulations of the impact of the various hiring credits, it is assumed that the take-up rate is constant and that H/L is the average hiring rate of all firms that benefit from the hiring credit.

The employers post wages, which are constant and non renegotiable. We focus on the stationary equilibrium where:

$$W_{ei} = \frac{w_i + \beta q W_u}{1 - \beta(1 - q)}$$

From the definitions of W_{ei} and W_u we have

$$W_{ei} - W_u = \frac{w_i - (1 - \beta)W_u}{1 - \beta(1 - q)} \quad (\text{F25})$$

and therefore:

$$\frac{\partial \theta_i}{\partial w_i} = \frac{-\theta_i}{(1 - \mu)(w_i - (1 - \beta)W_u)}$$

For a given number of unemployed persons in pool i , the optimal strategy for the entrepreneurs present in this pool consists of offering, at each date t , a non renegotiable constant wage $w_{i,t}$. The problem of the firm, which takes W_u as given, can be written:

$$\begin{aligned} \max_{\{V_{i,t}, w_{i,t}\}_{t=1, \dots, \infty}} \quad & \sum_{t=1}^{\infty} \beta^t \left[AR(L_{i,t}) - \prod_{\tau=1}^t (1 - q_{\tau-1})^t L_{i,0} w_{i,0} \right. \\ & \left. - \sum_{\tau=0}^{t-1} w_{i,t-\tau} m(\theta_{i,t-\tau}) V_{i,t-\tau} \prod_{x=0}^{\tau} (1 - q_{t-x}) - c_V V_{i,t} \right] \\ \text{s.t. } \quad & L_{i,t} = \prod_{\tau=0}^t (1 - q_{\tau}) L_{i,0} + \sum_{\tau=0}^{t-1} m(\theta_{i,t-\tau}) V_{i,t-\tau} \prod_{x=0}^{\tau} (1 - q_{t-x}) \text{ and } \frac{\partial \theta_{i,t}}{\partial w_{i,t}} = \frac{-\theta_{i,t}}{(1 - \mu)(w_{i,t} - (1 - \beta)W_{u,t})} \end{aligned}$$

The first order conditions with respect to $V_{i,t}$ and $w_{i,t}$, around the steady state (θ_i and q constant across periods), are respectively

$$-c_V + m(\theta_i) \sum_{\tau=0}^{\infty} \beta^{\tau} (1 - q)^{\tau} [AR_L(L_{i,t+\tau}) - w_{i,t}] = 0 \quad (\text{F26})$$

$$m'(\theta_i) \frac{\partial \theta_i}{\partial w_{i,t}} \sum_{\tau=0}^{\infty} \beta^{\tau} (1 - q)^{\tau} [AR_L(L_{i,t+\tau}) - w_{i,t}] - m(\theta_i) \sum_{\tau=0}^{\infty} \beta^{\tau} (1 - q)^{\tau} = 0 \quad (\text{F27})$$

From these two equations we obtain the steady state value of the wage which is identical in all labor pools, in the symmetric equilibrium:

$$w = \mu AR_L(L) + (1 - \mu)(1 - \beta)W_u$$

From (F23), (F24), (F25), (F26) and (F27) we get

$$W_u(1 - \beta) = b + \beta \frac{\mu}{(1 - \mu)} \theta_{c_V}$$

so that the wage equation is

$$w = \mu [AR_L(L) - b + \beta \theta_{c_V}] + b$$

Using the labor demand equation

$$AR_L(L) = w + \frac{c_V [1 - \beta(1 - q)]}{m(\theta)}$$

we can determine the wage:

$$w = b + \frac{\mu}{1 - \mu} c_V \left(\frac{[1 - \beta(1 - q)]}{m(\theta)} + \beta\theta \right)$$

and the employment level

$$AR_L(L) = b + \frac{1}{1 - \mu} \frac{c_V [1 - \beta(1 - q)]}{m(\theta)} + \beta\theta c_V \frac{\mu}{1 - \mu}$$

F.2.2 Underemployment in the wage posting model

Let us now verify that there is underemployment in the economy with some degree of monopolistic competition compared to the constrained efficient allocation. As the optimal allocation of products is symmetric, this is equivalent to maximizing the discounted production of each product net of the cost of job vacancies. We solve:

$$\begin{aligned} \Omega(u_{t-1}) &= \max_{V_t} F(1 - u_t) + bu_t - c_V V_t + \beta\Omega(u_t) \\ \text{s.t. } u_t &= u_{t-1} + q(1 - u_{t-1}) - \frac{V_t}{u_{t-1}} m\left(\frac{V_t}{u_{t-1}}\right) u_{t-1} \end{aligned}$$

The solution satisfies:

$$F_L(1 - u) = b + \frac{1}{1 - \mu} \frac{c_V [1 - \beta(1 - q)]}{m(\theta)} + \beta\theta c_V \frac{\mu}{1 - \mu}$$

The solution of the wage posting model is

$$\frac{E - 1}{E} F_L(1 - u) = b + \frac{1}{1 - \mu} \frac{c_V [1 - \beta(1 - q)]}{m(\theta)} + \beta\theta c_V \frac{\mu}{1 - \mu}$$

where we use the fact that: $R_L = \frac{E-1}{E} F_L(L) F(L)^{\frac{-1}{E}} = \frac{E-1}{E} F_L(L) P$ and $P = 1$ in the symmetric equilibrium of the wage posting model. As $E > 1$, the allocation of the wage posting model yields underemployment. Hiring credits can thus decrease the unemployment rate to narrow the gap to the constrained efficient allocation.

F.2.3 Temporary hiring credit

Now, let us assume that there is a non-anticipated temporary hiring subsidy in period t in the neighborhood of steady state. Employers post wages, which are constant and non renegotiable as assumed above, and a bonus $z_{i,t}$ for the current period. This bonus enables the employer and the employee to share the supplementary surplus provided by the hiring credit. In this context, the current labor earnings in period t , denoted by $\omega_{i,t}$, are the sum of the stationary wage w and of the bonus $z_{i,t}$. We still have

$$\frac{\partial \theta_{i,t}}{\partial W_{ei,t}} = \frac{-\theta_{i,t}}{(1 - \mu)(W_{ei,t} - W_{u,t})}.$$

The economy is in the neighborhood of steady state at date t . Thus, q_t , $W_{u,t+1}$, $W_{ei,t+1}$ and $\theta_{i,t+1}$ are at their steady state value. Accordingly, we have from equations (F22) and (F23):

$$W_{ei,t} - W_{u,t} = \omega_{i,t} - b + \beta(1 - \theta m(\theta) - q)(W_e - W_u) \quad (\text{F28})$$

and, therefore,

$$\frac{\partial \theta_{i,t}}{\partial z_{i,t}} = \frac{\partial \theta_{i,t}}{\partial W_{ei,t}} \frac{\partial W_{ei,t}}{\partial z_{i,t}} = \frac{-\theta_{i,t}}{(1 - \mu)(W_{ei,t} - W_{u,t})}.$$

The problem of the firm is to choose the bonus and the number of job vacancies that maximize its profits:

$$\Pi(Z, L) = \max_{(V_{i,t}, z_{i,t})} AR(L_{i,t}) - wL_{i,t} + (\sigma_t - z_{i,t})m(\theta_{i,t})V_{i,t} - c_V V_{i,t} + \beta \Pi(Z, L_{i,t}),$$

where $Z = (A, w, q)$, subject to the law of motion of employment:

$$L_{i,t} = (1 - q)L + m(\theta_{i,t})V_{i,t} \text{ and } \frac{\partial \theta_{i,t}}{\partial z_{i,t}} = \frac{-\theta_{i,t}}{(1 - \mu)(W_{ei,t} - W_{u,t})}. \quad (\text{F29})$$

The first order conditions with respect to $V_{i,t}$ and $z_{i,t}$ can be written

$$AR_L(L_{i,t}) - \omega_{i,t} + \sigma_t + \beta \Pi_L(Z, L_{i,t}) = \frac{c_V}{m(\theta_{i,t})} \quad (\text{F30})$$

$$\frac{\mu}{(1 - \mu)} [AR_L(L_t) - \omega_{i,t} + \sigma_t + \beta \Pi_L(Z, L_{i,t})] = W_{ei,t} - W_{u,t}. \quad (\text{F31})$$

From equations (F30) and (F31) we get

$$W_{ei,t} - W_{u,t} = \frac{\mu}{(1 - \mu)} \frac{c_V}{m(\theta_{i,t})}. \quad (\text{F32})$$

This equation shows that the hiring subsidy σ_t has no direct impact on the utility, and therefore on the remuneration obtained by the employee. The subsidy has an impact only through congestion effects associated with the labor market tightness that changes the average recruitment costs $c_V/m(\theta_{i,t})$ and the reservation utility $W_{u,t}$. Looking at equation (F30) we see that firms increase the number of job vacancies up to the point where the marginal discounted net revenue of employment equals the average recruitment costs $c_V/m(\theta_{i,t})$. Hence, choosing the number of vacancies when the marginal revenue of labor is decreasing allows the firm to avoid sharing the hiring subsidy with the incumbent workers. More generally, this mechanism, which has been highlighted by Stole and Zwiebel (1996), Brügemann et al. (2016), and examined further in a search and matching model by Cahuc et al. (2008), arises when there are decreasing marginal returns to labor and when firms can commit to employment either at the same time as they set the wage offer or before they bargain the wage.

Using equations (F28), (F30) and (F31), together with the steady state value of $(W_e - W_u)$, we obtain:

$$\omega_{i,t} = b + c_V \frac{\mu}{(1 - \mu)} \left(\frac{1}{m(\theta_{i,t})} - \beta \frac{1 - \theta m(\theta) - q}{m(\theta)} \right).$$

Substituting in equation (F31) and using (F30), we get in symmetric equilibrium in period t

$$\omega_t = b + \mu [A_t R_L(L_t) + \sigma_t + \beta \theta c_V - b]. \quad (\text{F33})$$

So that (7) and (8) can be written for period t :

$$A_t R_L(L_t) = b - \sigma_t + \frac{c_V}{(1-\mu)} \left[\frac{1}{m(\theta_t)} - \frac{\beta(1-q)}{m(\theta)} \right] + \frac{\mu}{1-\mu} \beta \theta c_V \quad (\text{F34})$$

$$1 - L_t = u_{t-1} + q(1 - u_{t-1}) - m(\theta_t)u_{t-1}. \quad (\text{F35})$$

Differentiation of these equations provides the macro elasticity of contemporaneous employment with respect to labor costs when there is a temporary change in labor costs induced by a temporary hiring credit in a context where the wage is endogenous:

$$\varepsilon_\sigma(\text{macro,temp,endo}) = \frac{1}{\eta\alpha \left[1 + \frac{c_V}{m(\theta)w} \left([1 - \beta(1-q)] - \frac{\mu}{q\alpha(1-\mu)^2} \right) \right]}. \quad (\text{F36})$$

where $\eta = H/L$ and $\alpha = LR_{LL}(L)/R_L(L) < 0$.

F.2.4 Permanent hiring credit

Let us analyze the consequence of a permanent hiring credit on steady state employment. Assuming that the hiring subsidy σ is permanent, we get in steady state, from the first-order conditions of the maximization problem of the firm:

$$\frac{AR_L(L) - w}{1 - \beta(1-q)} + \sigma = \frac{c_V}{m(\theta)} \quad (\text{F37})$$

$$\frac{\mu}{(1-\mu)} [AR_L(L) - w + [1 - \beta(1-q)]\sigma] = W_e - W_u. \quad (\text{F38})$$

In this equation, the term $w - [1 - \beta(1-q)]\sigma$ represents the instantaneous labor cost per employee, equal to the wage minus the flow of benefits of the hiring credit spread out on the expected duration of the job. Proceeding as in the previous subsection, it is easily shown that these two equations imply

$$w = b + \mu(AR_L(L) + \sigma[1 - \beta(1-q)] + \beta\theta c_V - b). \quad (\text{F39})$$

This wage equation is similar to equation (F33) except that σ is now multiplied by the factor $[1 - \beta(1-q)]$. It is worth stressing, however, that a permanent hiring credit has a different effect on wages than a temporary hiring credit because the steady state labor market tightness θ which appears in the wage equation (F39) reacts to a permanent hiring credit, but not to a temporary hiring credit. This implies that a permanent hiring credit has a stronger effect on wages than a temporary hiring credit. This effect comes through the impact of the labor market tightness on the expected value of unemployed workers, which can be written, using equations (F23) and (F32):

$$W_{u,t} = \frac{b}{1-\beta} + \frac{\mu}{1-\mu} c_V \sum_{\tau=1}^{\infty} \beta^\tau \theta_{t+\tau} \quad (\text{F40})$$

assuming that the transversality condition $\lim_{t \rightarrow \infty} \beta^t W_{u,t} = 0$ is fulfilled. This expression for $W_{u,t}$ shows that a temporary increase in labor market tightness has a lower impact on $W_{u,t}$, and therefore on wages, than a permanent increase.

Using the same method as in the previous subsection, we get, from the differentiation of equations (F37), (F38) and (F39), the macro elasticity of employment with respect to labor cost when the change in labor cost stems from a permanent hiring credit in a context where the wage is endogenous:

$$\varepsilon_{\sigma}(\text{macro,perm,endo}) = \frac{1}{\alpha \left[1 + \frac{c_V[1-\beta(1-q)]}{wm(\theta)} \right] - \frac{c_V[1-\beta(1-q)]}{wm(\theta)} \frac{\mu}{(1-\mu)^2 u} - \frac{q(1-u)\mu c_V}{w(1-\mu)^2 u^2 m(\theta)}}$$

F.3 Simulations of the cost per job created

The costs per job created of different hiring credits can be expressed as functions of α , c_V , μ , β , q , of the equilibrium values of the duration of vacant jobs, $1/m(\theta)$, and of u at baseline (i.e. without hiring credits).

We showed above that $\alpha(E, \chi)$ is the only parameter that is a function of the market power of firms measured by the price mark-up $E/(E-1)$. When simulating the cost per job created of hiring credits under different market powers, we vary E and hold constant the labor elasticity of the production function χ . This leads to variations in α , while all other parameters are hold constant. We assume that the data observed in France from 2006-2009 were generated by the wage posting model with a price mark-up $E/(E-1) = 1.3$ ($E = 4.3$, see Bouis, 2008). Given our estimate of α , we can then back out the implied elasticity of the production function χ .

We consider two other environments: one with a very high mark-up equal to 1.6 ($E = 2.7$) and one with perfect competition. When we compute the cost per job created in these counterfactual environments, we update the before-credit equilibrium values of $m(\theta)$ and u , using the wage-posting equilibrium conditions (F37) and (F39). To do so, we need to further pin down the matching efficiency \bar{m} , the productivity parameter and the instantaneous gain b of an unemployed person. \bar{m} is identified using the Beveridge curve from 2006-2009 and our estimate of μ . We scale the economy by setting the wage in this reference environment equal to 1. Then, using the labor demand equation (F37), we derive a consistent productivity parameter $A = .927$. Using the model solution - equation (F39), we obtain the replacement ratio $b = .945$. This order of magnitude is consistent with the ratio computed by Hagedorn and Manovskii (2008) including the utility derived from extra leisure when unemployed.

Table A8 summarizes the parameters values and the resulting baseline equilibrium values under different degrees of market power (across columns). The first column corresponds to the reference environment where the market power is medium (price mark-up equal to 1.3). The second column corresponds to a high market power and the last column to the perfect competition environment. The first row reports the price elasticity of demand and the second row the implied price mark-up. The third row reports the resulting elasticity of the marginal revenue. Then, the last rows show that under perfect competition (last column), the unemployment rate goes down to 4%, while the average vacancy duration is more than halved compared to the intermediate-competition case (from 0.2 to 0.07 years). This corresponds to the constrained efficient allocation. When the market power is high, unemployment surges to over 20% and the vacancy duration more than triples.

The parameter and equilibrium values in table A8 are plugged in the different formulas of employment elasticities, detailed in appendix F.1 and F.2, to obtain the cost per job created in

table 9.

Table A8: Parameters and baseline equilibrium under different degrees of market power

Market power	Medium	High	Zero
Price elasticity of demand: E	4.3	2.7	∞
Mark-up: $E/(E - 1)$	1.3	1.6	1
Elasticity of marginal revenue: α	-.964	-.971	-.953
Unemployment rate: u	8.3%	21.6%	4.1%
Labor market tightness: θ	1.1	.10	6.15
Average vacancy duration (year): $1/m(\theta)$.2	.70	.07

Source: Pole emploi and Insee. Authors computation.

F.4 Elasticity of employment in the business cycle and counter-cyclical hiring credits

In this appendix, we explicitly include macro shocks in the search and matching model. We first derive the behavior of firms when wages are exogenous. Second, we solve for the equilibrium employment level and wages when firms post wages. Third, we simulate the micro- and macro-effects of countercyclical policies and the resulting cost-per-job-created.

F.4.1 Two state model with exogenous wages

We assume 2 states of nature, corresponding to 2 levels of the productivity parameter $A_G > A_B$. The instantaneous transition probability from state A_G to state A_B is denoted e_G , while the instantaneous transition probability from state A_B to state A_G is denoted e_B . The ratio $1/e_G$ (or $1/e_B$) represents the average length of time the economy remains in state A_G (or A_B): it is a measure of the persistence of state A_G (or A_B). We assume that the 2 states also differ in their job separation rate: $q_G < q_B$

We first derive the solution of the model with exogenous wages, denoted by w_B and w_G . We assume that there is no layoff beyond the level $q_j L$ where q_j stands for the exogenous job separation rate in state $j = A, B$. We start by presenting the model without the hiring credits. The value function of the firm in each state satisfies:

$$\begin{aligned}\Pi^B(L_{t-1}) &= \max_{(V_t)} A_B R(L_t) - w_B L_t - c_V V_t + \beta [e_B \Pi^G(L_t) + (1 - e_B) \Pi^B(L_t)], \\ \Pi^G(L_{t-1}) &= \max_{(V_t)} A_G R(L_t) - w_G L_t - c_V V_t + \beta [e_G \Pi^B(L_t) + (1 - e_G) \Pi^G(L_t)],\end{aligned}$$

subject to the law of motion of employment:

$$L_t = (1 - q_j) L_{t-1} + m(\theta_t) V_t, j = B, G. \quad (\text{F41})$$

The first order conditions are:

$$\begin{aligned} A_B R_L(L_t) - w_B + \beta [e_B \Pi_L^G(L_t) + (1 - e_B) \Pi_L^B(L_t)] &= \frac{c_V}{m(\theta_t)}, \\ A_G R_L(L_t) - w_H + \beta [e_G \Pi_L^B(L_t) + (1 - e_G) \Pi_L^G(L_t)] &= \frac{c_V}{m(\theta_t)}. \end{aligned}$$

The envelope conditions are:

$$\begin{aligned} \Pi_L^B(L_{t-1}) &= (1 - q_B) [A_B R_L(L_t) - w_B + \beta [e_B \Pi_L^G(L_t) + (1 - e_B) \Pi_L^B(L_t)]], \\ \Pi_L^G(L_{t-1}) &= (1 - q_G) [A_G R_L(L_t) - w_G + \beta [e_G \Pi_L^B(L_t) + (1 - e_G) \Pi_L^G(L_t)]]. \end{aligned}$$

We solve for the employment level without taking into account the transition path from one state to the other. This is a relevant approximation, since we have linear adjustment cost at the level of the firm, and since labor flows are large empirically (see Jolivet et al. (2006)). Then there are only 2 levels of employment, L_G and L_B , and 2 levels of labor market tightness, θ_G and θ_B . Using the first-order conditions, the envelope conditions yield:

$$\begin{aligned} \Pi_L^B(L_B) &= (1 - q_B) \frac{c_V}{m(\theta_B)}, \\ \Pi_L^G(L_G) &= (1 - q_G) \frac{c_V}{m(\theta_G)}, \end{aligned}$$

so that the first-order conditions yield:

$$\begin{aligned} A_B R_L(L_B) &= w_B - \beta e_B (1 - q_G) \frac{c_V}{m(\theta_G)} + \frac{c_V}{m(\theta_B)} [1 - \beta (1 - e_B) (1 - q_B)], \\ A_G R_L(L_G) &= w_H - \beta e_G (1 - q_B) \frac{c_V}{m(\theta_B)} + \frac{c_V}{m(\theta_G)} [1 - \beta (1 - e_G) (1 - q_G)]. \end{aligned}$$

The law of motion of employment further implies that

$$q_j L_j = \theta_j m(\theta_j) (1 - L_j), \quad j = B, G. \quad (\text{F42})$$

We now introduce hiring credits in the 2-state model. There are different hiring credits in each state: σ_G and σ_B . The hiring subsidy in state $j = B, G$ is equivalent to an increase in the marginal revenue of labor in state j equal to s_j such that the discounted expected value of the flow of subsidy s_j on the whole expected job duration is equivalent to a one shot subsidy σ_j . To define the equivalent variations s_j , we also assume that the flow of subsidy is paid to the jobs only in the current state. Accordingly, we have $s_j = \sigma_j [1 - \beta (1 - q_j) (1 - e_j)]$. The labor demand equations for each state then become:

$$A_B R_L(L_B) = w_B - s_B - \beta e_B (1 - q_G) \frac{c_V}{m(\theta_G)} + \frac{c_V}{m(\theta_B)} [1 - \beta (1 - e_B) (1 - q_B)], \quad (\text{F43})$$

$$A_G R_L(L_G) = w_G - s_G - \beta e_G (1 - q_B) \frac{c_V}{m(\theta_B)} + \frac{c_V}{m(\theta_G)} [1 - \beta (1 - e_G) (1 - q_G)]. \quad (\text{F44})$$

Equations (F42), (F43) and (F44) enable us to compute numerical simulations of employment elasticities along the business cycle and of cost per job created by different hiring credits, when wages are exogenous. We present these simulations in more details below, after we endogenize wages.

F.4.2 Two state model with wage posting

As above, we assume two states of nature, B and G , with corresponding productivity levels and separation rates. We endogenize wages, as in the 1-state model, by assuming that wages are posted. Each firm chooses the number of vacancies and the labor contract that maximizes its value. The contract stipulates a wage that provides a level of expected utility W_e^j to the employee in each state $j = B, G$. The expected utility in state j of an employee who faces a transition probability e_j to the other macro state j' , writes:

$$W_e^j = w_j + \beta \left[(1 - q_j) \left[(1 - e_j) W_e^j + e_j W_e^{j'} \right] + q_j \left[(1 - e_j) W_u^j + e_j W_u^{j'} \right] \right], \quad (\text{F45})$$

where the index of firm i is omitted for the sake of simplicity. The hypothesis of directed search by workers and perfect mobility imply that the expected utility of an unemployed person is the same in all labor pools. If the instantaneous gain b of an unemployed person is the same everywhere and in each state $j = B, G$, the expected utility W_u^j of a person looking for work in state j satisfies:

$$W_u^j = b + \beta \left[(1 - e_j) \left[\theta_j m(\theta_j) W_e^j + (1 - \theta_j m(\theta_j)) W_u^j \right] + e_j \left[\theta_{j'} m(\theta_{j'}) W_e^{j'} + (1 - \theta_{j'} m(\theta_{j'})) W_u^{j'} \right] \right]. \quad (\text{F46})$$

As in the one-state model, differentiating the previous equation, holding W_u^j constant, defines a relation between W_e^j and θ_j :

$$\frac{\partial \theta_j}{\partial W_e^j} = \frac{-\theta_j}{(1 - \mu) (W_e^j - W_u^j)}. \quad (\text{F47})$$

The problem of the firm can then be decomposed in 2 steps. First, the optimal choice of the number of vacancies implies that the labor demand derived just above in the model with exogenous wage is still valid - equations (F43) and (F44). Second, the firm chooses the wage stipulated in its labor contracts, or equivalently the firm chooses the expected utility promised to the employee. To solve this second step, let us denote by $J^j(L_j)$ the marginal value of a job already matched in the firm, which is equal, by definition, to $\Pi_L^j(L_j)/(1 - q_j)$, or:

$$J^j(L_j) = \frac{\Pi_L^j(L_j)}{(1 - q_j)} = A_j R_L(L_j) - w_j - s_j + \beta \left[(1 - e_j) \Pi_L^j(L_j) + e_j \Pi_L^{j'}(L_{j'}) \right] \quad (\text{F48})$$

where s_j is the flow-equivalent hiring credit in state j , and the last equality is obtained thanks to the envelope theorem. The firm chooses to post labor contracts which provide a value W_e^j to the marginal employee in order to maximize

$$m(\theta_j) J^j(L_j),$$

or such that

$$\frac{\partial \theta_j}{\partial W_e^j} m'(\theta_j) J^j(L_j) + m(\theta_j) \frac{\partial J^j(L_j)}{\partial W_e^j} = 0.$$

The definition of the surplus of the marginal job, equal to $S^j(L_j) = J^j(L_j) + W_e^j - W_u^j$, which is independent of W_e^j , implies that $\frac{\partial J^j(L_j)}{\partial W_e^j} = -1$. Accordingly, using equation (F47), we get the Hosios condition:

$$\mu J^j(L_j) = (1 - \mu) (W_e^j - W_u^j).$$

Consequently, using the envelope condition, we obtain the expressions of the joint surplus and of the surplus of the employee:

$$\begin{aligned} S^j &= \frac{J^j(L_j)}{(1-\mu)} = \frac{1}{(1-\mu)} \frac{c_V}{m(\theta_j)}, \\ W_e^j - W_u^j &= \frac{\mu}{1-\mu} J^j(L_j) = \frac{\mu}{1-\mu} \frac{c_V}{m(\theta_j)}. \end{aligned} \quad (\text{F49})$$

Taking the sum of the Bellman equations defining the value of a marginal job to the firm and the value of employment to the worker – expressions (F48) and (F45) – and subtracting the Bellman equation to the value of unemployment – expression (F46) –, we get:

$$\begin{aligned} S^j &= A_j R_L(L_j) + s_j - b + \beta \left[e_j(1 - q_{j'}) S^{j'} + (1 - e_j)(1 - q_j) S^j \right] \\ &\quad - \beta(1 - e_j) \theta_j m(\theta_j) (W_e^j - W_u^j) - \beta e_j [\theta_{j'} m(\theta_{j'}) + q_j - q_{j'}] (W_e^{j'} - W_u^{j'}). \end{aligned}$$

Then, using the expression (F49), we get

$$A_B R_L(L_B) = b - s_B + \frac{1}{(1-\mu)} \frac{c_V}{m(\theta_B)} [1 - \beta(1 - e_B)(1 - q_B)] - \beta(1 - q_G) e_B \frac{1}{(1-\mu)} \frac{c_V}{m(\theta_G)} \quad (\text{F50})$$

$$+ \beta(1 - e_B) \theta_B m(\theta_B) \frac{\mu}{(1-\mu)} \frac{c_V}{m(\theta_B)} + \beta e_B [\theta_G m(\theta_G) + q_B - q_G] \frac{\mu}{(1-\mu)} \frac{c_V}{m(\theta_G)},$$

$$A_G R_L(L_G) = b - s_G + \frac{1}{(1-\mu)} \frac{c_V}{m(\theta_G)} [1 - \beta(1 - e_G)(1 - q_G)] - \beta(1 - q_B) e_G \frac{1}{(1-\mu)} \frac{c_V}{m(\theta_B)} \quad (\text{F51})$$

$$+ \beta(1 - e_G) \theta_G m(\theta_G) \frac{\mu}{(1-\mu)} \frac{c_V}{m(\theta_G)} + \beta e_G [\theta_B m(\theta_B) + q_G - q_B] \frac{\mu}{(1-\mu)} \frac{c_V}{m(\theta_B)}.$$

Equations (F50) and (F51) enable us to compute numerical simulations of employment elasticities along the business cycle and of the cost per job created by different hiring credits, when wages are endogenous.

F.4.3 Simulations of the two state model

As the analytical expressions of the employment elasticities are more complicated in the 2-state model than in the 1-state model, we simulate the whole model solution numerically, and compute the elasticities and cost per job created by different hiring credits. This section explains the simulations of the model solution.

First, we consider a reference environment where the unemployment rate is 8.3%, the vacancy duration is 0.2 year, and the annual job separation rate is 0.5. This is the same reference environment as in the 1-state model presented in Table 9. We then follow the same calibration strategy as in section F.3. The only difference is that the model is calibrated on a quarterly basis (instead of an annual basis). Consequently, we use a quarterly discount factor $\beta = .988$. The main outcomes and parameters of the reference environment are summarized in the first column of Table A9.

Table A9: Calibration of the 2-state model

	Reference	Bad state	Good state
Unemployment rate u	8.3%	11%	7%
Quarterly separation rate q	.158	.187	.146
Vacancy duration in quarters $1/m(\theta)$	1.25	1.41	1.16
Quarterly job finding rate $\theta m(\theta)$	1.76	1.52	1.93
Labor market tightness θ	1.41	1.07	1.67
Productivity A	.927	.889	.950
Wages w	1	.987	1.01

Source: Pole emploi and Insee. Authors' computation.

From this reference environment, we obtain two states: bad and good. We assume that $u_B = 0.11$ and $u_G = 0.07$. Following the literature on the decomposition of unemployment volatility (Hairault et al, 2015), we attribute 50% of this change from the reference unemployment to changes in separation rates and 50% to changes in job finding rates. The corresponding job separation rates and labor market tightness are reported in the second and third columns of Table A9.

We consider cycles between these 2 states. We assume that the good and bad states are generated by shocks on both the separation rate and the productivity level. All the other parameters are constant over the cycle: α, c_V, μ, m_0, b . The probabilities of entering into and leaving a recession are set such that recessions last 25 months and booms 31 months (average duration in France between peak and trough of the GDP growth over the period 1958-2016). This yields: $e_B = .12$ and $e_G = .097$ on a quarterly basis. Then, we solve for the productivity using the system of equations (F50) and (F51). Recessions are triggered by a 6% shock on productivity. Using the labor demand equations (F43) and (F44), we back out the wage levels in each state. As expected, they are procyclical and decrease by around 3% during recessions.

From the baseline cycle between these two states, we simulate temporary, time-invariant and countercyclical hiring credits, which can either be targeted to a small subset of firms (labor market tightness is then unaffected), or to all firms. When wages are exogenous, we simulate the labor demand equations (F43) and (F44), together with the Beveridge curve. When the wages are endogenous, we simulate equations (F50) and (F51), again with the Beveridge curve.

The temporary hiring credit is a one-year unanticipated credit implemented from the start of the recession. The computation of its cost per job created relies on the employment elasticity with respect to s_B only. Let us denote by ε this elasticity. The cost per job created is then $\eta w / \varepsilon$, where η is the ratio of hires benefiting from the credit to employment. We take $\eta = .26$, as in the computation of the 1-state model.

The time-invariant hiring credit is constant over the cycle, such that $s_G = s_B = 0.01$. We then simulate the changes in employment dL^G , and dL^B . The change in average employment is $d\bar{L} = \frac{e_G}{e_B + e_G} dL^B + dL^G \frac{e_B}{e_B + e_G}$. Consequently, we compute the employment elasticity $\frac{d\bar{L}w}{\bar{L}ds}$. As every employee benefits from the permanent hiring credit when she is hired, the cost per job created is just equal to the wage w divided by the employment elasticity.

The countercyclical hiring credit is available in recessions only: $s_B = 0.01$ and $s_G = 0$. We

then simulate the employment elasticity $\varepsilon(counter)$, following the steps above. As recessions last on average 2 years, we assume that the share $\eta(counter) = .52$ of employees benefit from the hiring credit – extrapolating from the treated share estimated in the one-year temporary hiring credit *zéro charges* ($\eta(counter) = 2.\eta$). The cost per job created reported in Table 10, is then $\eta(counter)w/\varepsilon(counter)$.

G Supplementary Tables

Table A10: Number of eligible/ ineligible firms and employees in the sample in 2008

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

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

Table A11: Difference-in-differences estimates of the impact of the hiring credit on various labor market outcomes in 2009 for surviving firms

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.009*** (.002)	.008*** (.002)	.009*** (.002)
Hours growth	.010*** (.002)	.009*** (.002)	.009*** (.002)
Hiring rate	.014*** (.005)	.012*** (.004)	.020*** (.005)
Separation rate	.005 (.005)	.004 (.004)	.011** (.005)
Nb. Observations	400, 293	400, 293	204, 311

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

Table A12: Difference-in-differences estimates of the impact of the hiring credit on various labor market outcomes in 2009 with weighted observations

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.009*** (.002)	.008*** (.002)	.008*** (.002)
Hours growth	.010*** (.002)	.008*** (.002)	.008*** (.002)
Hiring rate	.014*** (.005)	.012*** (.004)	.019*** (.005)
Separation rate	.005 (.005)	.004 (.004)	.010** (.005)
Survival rate	.000 (.001)	.000 (.001)	-.001 (.001)
Nb. Observations	406,468	406,468	207,379

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

Table A13: Difference-in-differences estimates of the impact of the hiring credit on various labor market outcomes in 2009 by sector

Industries	Services	Others
Cohorts	2006-2009	2006-2009
Covariates	Yes	Yes
Employment growth	.008*** (.002)	.009*** (.003)
Hours growth	.009*** (.002)	.009*** (.003)
Hiring rate	.018*** (.007)	.010** (.004)
Separation rate	.010 (.006)	.001 (.004)
Nb. Observations	261,787	144,681

Source : DADS (Insee). Note : this Table presents our difference-in-differences estimates for different outcomes (rows) for different sector (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in the previous year (average from 1 December to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t and year t-1; the growth rate of the number of hours worked between November of year t and November of year t-1; the number of hires from 1 December of year t-1 to 30 november of year t divided by employment on 30 November of year t-1; the number of separations from 1 December of year t-1 to 30 November of year t divided by employment on 30 November of year t-1. 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 A14: Difference-in-differences estimates of the impact of the hiring credit on employment and hours worked in 2009 based on employment pool x sector units

Cohorts	2006-2009	2006-2009	2008-2009
Covariates	No	Yes	Yes
Employment growth	.008*** (.002)	.007*** (.002)	.008*** (.002)
Hours growth	.008*** (.002)	.008*** (.002)	.008*** (.002)
Nb. Observations	13, 537	13, 537	6, 778

Source : DADS (Insee). Note : this table presents our difference-in-differences estimates for different outcomes (rows) and different specifications (columns) based on averaged labor market outcomes for 5 different sectors in 348 employments pools. Within each employment pool x sector unit the treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in the previous year (average from 1 January to 30 November). For each year, we only consider units for which we have observations in our treatment or our control groups. The control group comprises firms of size between 10 (included) and 14 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year t-1 and year t; the growth rate of the number of hours worked between November of year t-1 and November of year t. As covariates, we include year dummies, sector dummies, region dummies and their interactions. We also include dummies for distribution of firms' age, the share of firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard deviations in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.