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ABSTRACT

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In this paper we estimate by matching techniques the effects of a French retraining program on the reemployment rate of displaced workers. This program, called “Conventions de conversion”, was intended to improve reemployment prospects of displaced workers by proposing them retraining and job seeking assistance for a period of six months beginning just after the dismissal. Our empirical analysis is based upon non-experimental data collected by the French Ministry of Labour. Matching estimates show that this program succeeded in increasing the employment rate of trainees by approximately 6 points of percentage in the medium-term, namely in the second and third years after the date of entry into the program. This improvement is essentially due to an increase of their reemployment rate in regular jobs, namely jobs under long-term labour contracts.

JEL Classification: C41, J24, J64, J68

Keywords: evaluation, retraining program, displaced workers, matching estimates

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1. Introduction

Improving the effectiveness of active labour market programs (ALMPs) is a main policy area identified by the OECD Jobs Strategy for helping to reduce structural unemployment (OECD, 1994 and 1996). In an era of tight government budgets and a growing disbelief regarding the positive effects of ALMPs, evaluation of these programs becomes imperative.[1] On the one hand, the persistently high level of unemployment rates stresses the necessity of assistance programs in helping unemployed participants to find a job faster. On the other, it is often doubted whether the positive effects of active programs outweigh their costs. Moreover, advocates of a pessimistic perspective even argue that participation in such programs in periods of high unemployment may be seen as a negative signal for some employers and could therefore have a counter-productive impact on the employment performance of participants.

Active labour market policies have been increasingly introduced in France since the mid-seventies, when unemployment started to increase. These policies were targeted to workers with high unemployment risks, such as young people, older or displaced workers. They are similar to those implemented in other European countries, France being a median user. In France, the scope of public interventions is rather diversified. Most of the programs consist in providing training, job seeking support, vocational course, and adaptation training (for the youngest). Microeconometric studies carried out by Bonnal *et al.* (1997), Fougère *et al.* (2000), and Brodaty *et al.*(2001) have investigated the impact of such policies on employment prospects of young people and unskilled workers. However, few contributions have addressed the issue of employment programs for laid off or displaced workers[2].

Our study concerns a retraining program, called "Convention de conversion", which was set up in France during the eighties in order to improve labour market prospects of displaced workers.[3] It consists in providing an immediate and individual support to

[1] A summary of both empirical strategies and evidence on the effects of ALMPs can be found in Heckman *et al.* (2000). In general, these studies find mixed evidence regarding the relevance of programs on both unemployment duration and earnings of participants.

[2] Margolis (1999) is an exception since he examines the impact of displacement on unemployment duration in France. However he does not evaluate the effect of active labour market programs proposed to displaced workers.

[3] Here displacement is defined as a layoff for economic reasons, i.e. because of a reduction in the workload or a lost position or a shift. This category does not include layoffs for personal reasons, such as inadequate performance or misconduct.

displaced workers for a period of six months (beginning just after the dismissal) by proposing retraining and job seeking assistance. This support is granted to workers up to 57 years old who have at least two years of seniority in their former firm. US training programs mainly focus on increasing the productivity and earnings of low-income individuals. In contrast, the main purpose of this French program is to prevent or to reduce unemployment by increasing the participants' employment rates rather than their earnings. Thus our study boils down to the following question: do program participants manage to find a job, and more precisely a permanent job, faster? Hence, the outcome of interest is not only the re-employment probability, but also the probability to find a long-term labour contract.

Our empirical analysis uses non-experimental data collected by the French Ministry of Labour from 1995 to1998. A first survey, called "Trajectoires des demandeurs d'emploi et marché local du travail" ("Event histories of unemployed workers and local labour markets"), has collected information on workers who entered unemployment between April and June 1995 in eight local labour markets belonging to three administrative regions: Paris - Ile de France, Nord - Pas de Calais, and Provence - Alpes Côte d'Azur. This survey has been completed by another one, called "Trajectoires des adhérents à une convention de conversion" ("Event histories of displaced workers participating to the 'Convention de conversion' program"). This second survey was conducted in the same local areas with the same questionnaire, but exclusively on displaced workers joining the program during the same initial period.

The major challenge of any evaluation study using non-experimental data is to treat the potential selection bias. Displaced workers who have decided to join the program might have individual characteristics that would be different from those who have not joined it. Workers participating in such a program may be less or more able to find a permanent job (namely, a job with a long-term labour contract) compared to otherwise identical non-participants. The difference in post-program outcomes between participants and the control group of non-participants may thus reflect those individual factors rather than a causal effect of the program. To estimate the effect of the training program on the re-employment

probability of displaced workers, we use statistical matching techniques.[4] In fact, the absence of valid instruments does not allow us to evaluate this effect by estimating parametric or semiparametric selection models. Moreover, we think that our data verify the three conditions put forth by Smith and Todd (2001) for a satisfying application of matching estimators: *(i)* for both groups (treated and controls), the data come from the same administrative source, so that outcomes are measured in an analogous way, *(ii)* participants in the program and nonparticipants reside in the same local labour markets, and *(iii)* the data contain a sufficiently rich set of variables relevant to modelling the program-participation decision.

The paper is organized as follows. The next section surveys the literature on displaced workers. In the third section, we give a brief summary of the French institutional framework concerning layoffs, and then we present the main specific features of the retraining program that we evaluate. The fourth section presents the database. The fifth section presents the statistical strategy. A sixth section presents and comments the estimates. The last section summarizes the main results.

2. Literature review

Worker displacement involves an involuntarily job separation caused by adverse economic conditions. In such a case, the job separation is initiated by the employer and not caused by the individual worker's performance. In general displaced workers are more likely to experience longer unemployment spells and to incur higher search costs, including possibly costs involved by retraining and moving to areas with higher employment opportunities.

Displaced workers have been the subject of an extensive literature (see surveys by Fallick, 1996, and Kletzer, 1998). The basic stylised facts are: 1) displaced workers experience longer unemployment spells and higher earning losses than the other unemployed workers (Swaim and Podgursky, 1991, Jacobson *et al.*, 1993); 2) the duration of the subsequent unemployment spell increases with job tenure (Fallick, 1996), because workers with high

[4] Papers by Gerfin and Lechner (2002), Sianesi (2004) and Ichino *et al.* (2008) are good examples of the use of matching estimation techniques for the evaluation of active labour market policies implemented in European countries.

tenure have a higher level of specific human capital investment in their firms and in their industry or occupational sectors; 3) the duration of this unemployment spell is affected by the cause of displacement; for instance, Swaim and Podgursky (1991) found that workers displaced because of plant closures experience one third fewer weeks of unemployment than those who are laid off by ongoing establishments. The econometric analysis conducted by Gibbons and Katz (1991) has shown that this result could not be attributed solely to differences in observable worker characteristics. These authors argued that it is due to a "lemon" effect: prospective employers perceive laid-off workers as being of low ability compared to people who lose their job due to plant closure. But, as noticed by Fallick (1996), "such comparisons cannot address how displacement differs from other potential movements into unemployment. A useful direction for future research would be to compare displaced workers to workers who enter unemployment in other ways – for example, new entrants and re-entrants to the labor force, workers who quit, workers whose previous job was explicitly temporary, workers who are permanently discharged for cause, and those who experience temporary layoffs."

In view of the difficulties the displaced workers face in achieving reinsertion, debates arise on the suitable policies to be undertaken. As there is little prospect of returning to a comparable job within a reasonable period of time (because of limited opportunities in the same industry, occupation or region), displaced workers may need retraining or search assistance. While retraining of displaced workers is not a new policy, programs to assist displaced workers have gained a renewed interest. In general, such programs offer job search assistance along with formal training. In his landmark study on displaced workers, Leigh (1990) concludes that job search assistance is the most cost-effective program for displaced workers. It also appears that training can shorten the periods of unemployment, but it does not affect long-term earnings. Leigh (1990) synthesizes some findings from his examination of labour market policies in Sweden, Germany, Japan, Great Britain and Australia. The main results are the following: job search assistance is relatively cheap and should be made freely available to those recommended by their case managers; quality assessment should be conducted for those wishing to join a retraining program; training should be locally based and characterized by decentralized decision-making to meet local needs more appropriately. Dar and Gill (1998), after studying eleven retraining programs in six countries, concluded that such programs are generally no more effective than job search assistance in increasing re-employment prospects. As a result, they should be

targeted to those who can benefit the most from them: women and minorities (Moore, 1990), industry-switchers (Stock, 1998), laid off workers from manufacturing (Kletzer, 1998), or those with high tenure (Jacobson *et al.*, 1993). In fact job search assistance and training appear to have some impact on the types of jobs that displaced workers obtain. Farber (1999, 2003) found that workers who lose their jobs are more likely to be reemployed in temporary jobs and, when reemployed in a permanent job, they earn significantly less than they did prior to their last job. Thus an obvious important consequence of job loss is the inability to find a new stable job.[5]

Despite of these negative consequences of job displacement, there are only a few studies evaluating the impact of long and intensive training programs on employment and wages of laid-off workers. For instance, Kodrzycki (1997) analyzes a sample of workers laid off between 1991 and 1994 who used assistance centers in Massachusetts operating under the provisions of the EDWAA amendment to Title III of the Job Training and Partnership Act. These centers offer basic readjustment services such as counselling and job market information to all users. In addition, some displaced workers received education and/or job training programs at local colleges, universities, and specialized training facilities. Kodrzycki (1997) restricts the sample to workers who had previously been employed full-time and who became reemployed at a new job. First, she finds that job training only (as opposed to job training combined with general education classes) tended to draw the workers with the highest reading abilities and previous earnings, while job training combined with education tended to draw less promising candidates. Then, applying ordinary least squares to nonexperimental data, she concludes that, "even if some training programs can be shown to provide positive job changes that eventually result in higher job satisfaction or greater income for displaced workers, they may still turn out not to be socially beneficial." In a more recent paper, Jacobson *et al.* (2005) have evaluated the effects of community college schooling offered to laid-off workers in the Washington State. This program includes a broad variety of courses, ranging from "basic skills" and vocational training to academic courses in math and science. Their estimates indicate earnings increases of 7% for males and 13% for females. The returns are up to three times larger for technically-oriented courses.

[5] In 2003 in France, one year after their displacement, only 15% of laid off workers have found a new permanent job, 15% are occupied in temporary jobs, 10% are in training programs or pre-retired, and 60% are still unemployed.

Besides these articles focusing on programs targeted to displaced workers, there is a huge literature devoted to the evaluation of active labour market policies (ALMP hereafter), especially programs (often targeted to long-term unemployed) which offer both job-search assistance and intensive (re)training to participants. For instance, in a recent study, Stenberg and Westerlund (2008) have evaluated the effects of comprehensive adult education on wage earnings of Swedish long-term unemployed. Their estimates, obtained with propensity score matching techniques, suggest that more than one semester of study results in substantial increases in post program annual earnings for both males and females. However, several studies find that programs promoting subsidized jobs are more effective than those offering training or education periods. For instance, Dorsett (2006), who compares the effectiveness of the four options of the New Deal for Young People in the UK, finds that the employment option performs better than other options, namely full-time education and training, the voluntary sector and the environmental task force. Sianesi (2008) investigates the differential performance of six Swedish active labour market programs for the unemployed. She finds that employment subsidies perform best by far, followed by trainee replacement and, by a long stretch, labour market training. More recently, Jespersen *et al.* (2008), who examine the long-term effects of Danish ALMP, conclude that classroom training does not significantly improve employment or earnings prospects in the long-run, contrary to private job training programs which have substantial positive effects.

However, these negative results should be counterbalanced by some studies which find that training programs have mixed effects (see, for instance, Gerfin and Lechner, 2002, and Lechner *et al.*, 2005), but also by the main conclusions of the recent meta-analysis, based on 97 international studies of active labour market policies, conducted by Card *et al.* (2009). Card *et al.* (2009) point out in particular that longer-term evaluations tend to be more favourable than short-term evaluations and that classroom and on-the-job training programs appear to be particularly likely to yield more favourable medium-term than short-term impact estimates. Let us remark that our study contributes to this debate since the program that we evaluate involves classroom training and since we focus on its medium-term (2^{nd} year) and long-term (3^{rd} year) effects.

3. The institutional framework

French labour law distinguishes between layoffs for economic reasons and layoffs for personal reasons, such as inadequate performance or misconduct. A layoff for economic reason is defined as a displacement resulting from a reduction in the workload or a lost position or shift. This category excludes then laid-off workers due to own behaviour, but also quits, entries into unemployment due to the termination of a short-term labour contract, and new entries (or re-entries) into the labour force. This is a wider definition than the one proposed, for instance, by Fallick (1996) who notices that 1) displaced workers do not include workers fired for cause, 2) the displacement should have a structural cause, 3) displaced workers have a limited ability to return to a comparable job within a reasonable span of time, and 4) they are strongly attached to the sector in which they were employed. Our definition of displacement is closer to that proposed by the Bureau of Labor Statistics, which characterizes a displaced worker as someone at least 20 years old, with at least three years of tenure on a job, who lost that job (without being recalled) due to slack work, abolition of a position or shift, or plant closing or relocation.

During the 1990s the French labour market was characterized by numerous job losses; for instance, each month in 1998, on average 25,000 employees were laid off. One common characteristic of all layoffs for economic reasons is that employers are required to propose the option of participating in a retraining scheme (partially employer funded) to all employees who will be displaced. This clause became an actual right inscribed into the Labour code. The retraining program called “Convention de conversion” was introduced by a decree in date of 3rd and 4th April 1987 as a compensation for the administrative authorization to lay off. It was then cancelled in June 2001.

The employee joining the retraining program was made redundant but was not registered as being actually unemployed. The initial goal of this program was to avoid long unemployment spells for employees who were laid off for economic reasons. It consisted in providing an immediate and individual support to the displaced workers for a period of six months beginning just after the dismissal. What were the eligibility conditions? Participation was voluntarily, but support was granted to workers up to 57 years old, having at least 2 years of seniority in the firm. Technical Units of Reinsertion (“Unités techniques de reclassement”) were in charge of accompanying and reinserting participants. They assessed the employee’s professional records and then, proposed appropriate actions

including job-seeking sessions, stressing self-employment opportunities, on-the-job assessment and extra training (computer, accounting, management, languages, etc…). The benefit of extra education and support in job seeking throughout the program was meant to reduce the subsequent unemployment spell and more generally to improve the conditions of reinsertion into the labour market. During the first two months of the program, the worker received a specific allowance representing 83% of his or her previous wage. This percentage fell down to 70% during the four following months.

Statistics published by the French Ministry of Labour show that, since 1997, the gap between the number of workers who joined the program and the number that is potentially entitled to join has been closing. Indeed, throughout the first semester of 1999, 84.7% of the eligible workers joined it, while they were only 79.1% in 1996 (which was already a peak[6] due to the important number of layoffs that year).

4. The data

The estimation is carried out using data coming from two surveys collected by the French Ministry of Labour, in collaboration with the *Agence Nationale Pour l'Emploi* (ANPE hereafter), which is the French public employment service, and the *Union Nationale pour l'Emploi Dans l'Industrie et le Commerce* (UNEDIC hereafter), which is the institution in charge of the payment of unemployment insurance benefits in France. Information used in our study comes from these two surveys. For instance, at each interview, workers are asked about their seniority in the previous job, about their employment status in the second or the third year after entry into the program, etc. The answers, especially those concerning the unemployment or employment status at each date, are compared (and eventually set in accordance) with the information coming from the ANPE and UNEDIC administrative files. In our opinion, this procedure limits the potential problem of non-random misreporting.

The first one, called "*Trajectoires des demandeurs d'emploi et marché local du travail*", contains information on a random sample of workers entering unemployment between April and June 1995 in three French administrative regions (Paris-Ile-de-France,

[6] There were 148,492 new recipients of this program in 1996, which corresponds to an increase of 11.2% for the number of participants between 1995 and 1996.

Provence–Alpes-Côte d'Azur, and Nord-Pas-de-Calais). Indeed, these surveys are not representative of all the regions in France. These workers were interviewed at three dates, until May 1998. This first survey has been completed by another one, called "*Trajectoires des adhérents à une convention de conversion*", held in the same conditions and with similar questionnaires, but collected on workers joining the program at the same date. The originality of these surveys lies thus in the fact that individuals are randomly sampled in the inflows of displaced workers either joining the retraining program after dismissal or entering unemployment (without joining the program) during a given time interval. As a consequence, unemployment duration is not left censored.

To evaluate the causal effect of the program, one has to contrast the situation of individuals after program participation with the counterfactual situation in the absence of participation. Because the latter situation is not observable, it needs to be estimated based on the outcome of other individuals who do not participate, the so called control group. When choosing or constructing this control group, different adjustment procedures may be applied to ensure that participants and controls are identical with respect to all relevant characteristics except that of not participating. In experimental evaluations, the construction of an adequate control group is done by means of randomisation. When non-experimental data are used, failure to take into account for discrepancies between participants and controls may lead to substantially biased judgements regarding the effect of the program. Here one has drawn the control group from the first survey in order to control for eligibility conditions at first stage. The control group is then only composed of individuals potentially entitled to join the program, which means that they respect the following imposed criteria used when applying to the program: they are under 57 years old, they are displaced and have at least two years of seniority in their previous firm. The whole sample, including both sub-samples (participants and non-participants), includes 1,912 observations. The date from which the effect of the retraining program is measured is chosen to be either the date of entry into the program for trainees, and the date of entry into the sampled unemployment spell for controls.[7] This approach is valid since the treated enter the program immediately after being laid off. Otherwise, bias might arise by the fact

[7] After layoff, the trainees are not registered as unemployed. Empirical evidence shows that, in general, the time spent in a training program is principally devoted to training (see, for instance, Lalive *et al.*, 2000). Fitzenberger *et al.* (2009) show that, despite the lock-in effect resulting from the participation period, some programs may still have significant positive effects on employment rates in the medium and long run. These results confirm those previously obtained by Lechner (2004).

that the most successful dismissed workers immediately find a job and only the unsuccessful ones finally decide to enter the program. At the program start, this population would not necessarily be comparable to the inflow into unemployment.

Descriptive statistics for the two sub-samples (participants and non-participants) are displayed in the appendix (Table A.1). The sub-sample of participants contains higher proportions of older workers with higher seniority (more than 10 years). Among the 1,010 participants, 130 move directly from the program to a permanent job and 164 directly to a temporary job. The 716 remaining enter unemployment at the end of the program. Among these unemployed participants, 189 moved to a permanent job, 156 found a temporary job, the others 371 are either still unemployed at the end of the observation period, either in training or inactivity. Among the 920 unemployed non-participants, 166 have found a permanent job and 236 a temporary labour contract. In fact, roughly 32% of participants have obtained a permanent job (either directly at the end of the program or after the unemployment spell following program participation). Conversely, only 18% of non-participants have moved from unemployment to a permanent job. It seems that participation in the program increases the frequency of transitions to regular (permanent) employment. However, these crude statistics could be subject to a composition bias due to the individual heterogeneity of both subgroups. The statistical approach conducted in the next sections allows us to control for this heterogeneity.

5. Matching estimators

Evaluation methods usually try to compare two potential outcomes which are associated with two regimes, generally called “treatment” and “non-treatment”. The regime is indicated by the value of a dummy variable *D*, which takes value 1 in the treatment regime and value 0 in the non-treatment case. Treatment is associated with an individual outcome denoted Y_1 while non-treatment generates an outcome denoted Y_0. Moreover X denote pre-treatment characteristics verifying the conditional independence assumption (CIA hereafter) which is required for implementing matching estimation techniques. This assumption states that :

$$(Y_0, Y_1) \coprod D | X \ , \forall \ X$$

This assumption means in particular that selection into the program (i.e. the treatment regime) is only based on observable characteristics and that all covariates affecting simultaneously assignment to treatment and potential outcomes are observed by the analyst. Rosenbaum and Rubin (1983) have shown that the CIA assumption implies that:

$$(Y_0, Y_1) \coprod D | P(X), \forall X$$

where the propensity score $P(X) = \Pr(D = 1 | X) = E(D | X)$ must verify that $0 < P(X) < 1, \forall X$. This last condition means that all individuals with the same X have the same probability to be treated or non-treated, and that a match can be found for all $D = 1$ persons. The CIA assumption also implies that the propensity score $P(X)$ is balancing, namely:

$$D \coprod X | P(X), \forall X$$

This last property can help in determining which interactions and higher order terms to include for a given set of *bqca* covariates in the propensity score model (say, a logit or a probit model), even if it does not aid to choose which variables X to include. In particular, it implies that, if after conditioning on the estimated values of $\Pr(D = 1 | X)$ there is still dependence on X, the model used to estimate $\Pr(D = 1 | X)$ could be misspecified.

Under these assumptions, the average effect of the treatment on the treated (ATT) can be written as:

$$\begin{aligned} ATT &= E(Y_1 - Y_0 | D = 1) \\ &= E(Y_1 | D = 1) - E_{X|D=1}\{E_Y(Y_0 | D = 1, P(X))\} \\ &= E(Y_1 | D = 1) - E_{X|D=1}\{E_Y(Y_0 | D = 0, P(X))\} \end{aligned}$$

where the first term can be estimated from the treatment group and the second term from the average outcome of those persons in the comparison group who are matched on $P(X)$. Equivalently, the average effect of the treatment on the untreated (ATU) can be written as:

$$\begin{aligned} ATU &= E(Y_1 - Y_0 | D = 0) \\ &= E_{X|D=0}\{E_Y(Y_1 | D = 1, P(X))\} - E(Y_0 | D = 0) \end{aligned}$$

The average treatment effect (ATE) for a randomly chosen individual is:

$$ATE = E(Y_1 - Y_0 | D = 1) \times \Pr(D = 1) + E(Y_1 - Y_0 | D = 0) \times \Pr(D = 0)$$

All matching estimators of the ATT parameter take the form:

$$ATT_m = \frac{1}{n_1} \sum_{i \in I_1 \cap S_p} \left(Y_{1i} - \hat{E}(Y_{0i} | D_i = 1, P_i) \right)$$

where

$$\hat{E}\left(Y_{0i} \middle| D_i = 1, P_i\right) = \sum_{j \in I0} W(i, j) Y_{Oj}$$

and where I_1 denotes the set of participants, I_0 the set of nonparticipants, S_P the region of common support, and n_1 the number of persons in the set $I_1 \cap S_P$. The counterfactual expectation for each individual $i \in (I_1 \cap S_P)$ is estimated as a weighted average over the outcomes of nonparticipants, the weight $W(i, j)$ depending on the distance between the estimated propensity scores of individuals i and j, denoted P_i and P_j respectively.

Let $C(P_i)$ denote a neighbourhood of the propensity score of individual i belonging to the participant sample. The neighbours of participant i are nonparticipants j whose score P_j belongs to $C(P_i)$. Matched nonparticipants belong to the subset $A_i = \{j \in I_0 \mid P_j \in C(P_i)\}$. Alternative matching estimators differ in how the neighbourhood $C(P_i)$ and the weights $W(i, j)$ are defined.

For instance, the *nearest-neighbour matching estimator without replacement* is based on neighbourhoods:

$$C\left(P_i\right) = \min_j \left\| P_i - P_j \right\|, j \in I_0$$

and weights $W(i, j) = \frac{1}{n_i^C}$ if $j \in A_i$, 0 otherwise. In this expression, n_i^C is the number of nonparticipants in A_i. Typically, the nonparticipant with the value of P_j that is closest to P_i is selected for matching and A_i is a singleton set ($n_i^C = 1$). Implementation of this estimator does not impose any common support condition. In the case without replacement, each $D = 0$ observation can serve as the match for at most one $D = 1$ observation.

Caliper matching imposes a tolerance on the maximum distance $\left\| P_i - P_j \right\|$ allowed. More precisely, a match for individual i is selected only if $\left\| P_i - P_j \right\| < \varepsilon, j \in I_0$, where ε is a pre-specified tolerance. Here the neighbourhoods are :

$$C\left(P_i\right) = \left\{ P_j \middle| \left\| P_i - P_j \right\| < \varepsilon \right\}, j \in I_0$$

Dehejia and Wahba (2002) have proposed a variant of caliper matching, called *radius caliper matching*, in which the counterfactual is calculated as the mean outcome of all nonparticipants within the caliper (namely, belonging to the subset A_i), rather than just the closest neighbour.[8]

[8] In our application, the tolerance ε is set equal to 0.06.

The *kernel matching estimator* constructs a match for each program participant by using a kernel-weighted average over multiple persons in the control group. Its general expression is:

$$ATT_{km} = \frac{1}{n_1} \sum_{i \in I1 \cap Sp} \left(Y_{1i} - \frac{\sum_{j \in I0} Y_{0j} G\left(\frac{P_j - P_i}{a_n} \right)}{\sum_{k \in I0} G\left(\frac{P_k - P_i}{a_n} \right)} \right)$$

where $G(.)$ is a kernel function and a_n is a bandwidth parameter. Here the weights are defined by:

$$W(i, j) = \frac{G\left(\frac{P_j - P_i}{a_n} \right)}{\sum_{k \in I0} G\left(\frac{P_k - P_i}{a_n} \right)}$$

and the neighbourhood $C(P_i)$ depends on the specific kernel function that is chosen.

In our application, we produce these three types of matching estimates by using the STATA modules *psmatch2* (Leuven and Sianesi, 2003) and *pscore* (Becker and Ichino, 2002). Since the samples used in our evaluation are choice-based with program participants oversampled, we match on the odds ratio $P/(1-P)$, as suggested by Smith and Todd (2005, p. 319).[9]

6. Results

6.1 Validity of the matching procedure

Before implementing the matching procedure, we must argue about its validity in our context. For that purpose, we first need to detail the process through which workers may have access to the program. In principle, each employer firing workers for economic reasons, whatever their number and their wages, *should* propose to these workers to participate in the program.[10] More precisely, the employer must notify the employees concerned by registered mail (with acknowledgement of receipt) the proposal to participate

[9] Estimates obtained by matching on the propensity score P are close to those obtained by matching on the odds ratio. They are not reproduced but are available from the authors.

[10] Workers laid-off for personal reasons, such as inadequate performance or misconduct, cannot participate in the program. It is thus possible that some employers could have been tempted to transform layoffs for economic reasons into layoffs for personal reasons.

in the program, with the photocopy of the agreement the firm has signed with the State (i.e. with the local representatives of the Ministry of Labour). In this letter, the employer must also detail the re-employment possibilities of the laid-off workers within the firm (for instance, after participation in the program). However, the employer is not constrained to choose this option. If he/she decides not to propose the program to the laid-off workers, he/she has to pay to the ASSEDIC (which is the institution in charge of raising unemployment insurance contributions in France) a penalty whose amount is equal to one monthly gross wage for each laid-off worker. Consequently it is likely that firms with sufficiently "deep pockets" (in particular, the biggest ones) could be more willing to pay the penalty. This argument leads us to control for the size of the firm when choosing the regressors incorporated into the propensity score model.[11] When layoffs result from a plant closing, the employer has probably much more difficulties to detail the possibilities of future re-employment within another plant (if the firm has several plants). Moreover, in the case of a plant closing, it is generally more difficult to organize a program session because of the limited capacity of training centres. These last arguments could explain why the occurrence of a plant closing makes the participation in the program less likely.

Participation in the program is not mandatory. The worker has three weeks from the receipt of the employer's letter in which he may accept (or eventually refuse) the proposal to participate in the program. Nevertheless, he/she has strong incentives to participate: during the program, he/she receives an allowance equal to 83.4% of his/her previous wage in the first two months, and then equal to 70.4% of this wage as from the third month. For comparison, if he/she refuses to participate and enters unemployment after his/her layoff, the amount of his/her UI benefit is equal to 57.4% of his/her previous wage. Moreover, if he/she does not finds a new job at the end of the program (which generally lasts six months), he/she is still eligible to UI (with the usual UI replacement ratio), and the length of his/her overall period of eligibility to UI is only reduced by two months and a half. Thus, for explaining why the worker may not enter the program, we have to control for the determinants that influence both his/her decision to join the program as well as his/her future labour market performance.

Like in previous studies using matching techniques for the evaluation of ALMPs (see, for instance, Lechner *et al.*, 2005, Sianesi, 2008, Stenberg and Westerlund, 2008, Jespersen *et*

[11] Unfortunately, we have no information on the market value of the firm (like its profit or its market share).

al., 2008, and Fitzenberger *et al.*, 2009), we control for a whole list of variables characterizing the worker's past employment history as well as his/her current employment prospects. In our dataset, several variables describe the worker's past history: his/her potential labour market experience, the number of unemployment spells that he/she previously experienced, the duration of his/her longest previous unemployment spell, and his/her seniority within the firm. Demographic factors such as age, gender, number of children in the household and citizenship, are also important determinants of the labour market prospects. Information about individual human capital is available through variables indicating the educational level (college and above) and the professional category (unskilled or skilled blue-collar worker, white-collar worker, intermediate profession, executive). Variability of the local socioeconomic contexts is restricted here since we consider only eight local labour markets belonging to three administrative regions. However, to control for the remaining differences in terms of unemployment rate and of labour demand across these eight labour markets, we take into account the value of the unemployment/vacancy ratio *U/V* defined as the number of unemployed persons divided by the number of vacant jobs in the local labour market where the individual lives. The participation in the program being voluntary, its evaluation by a matching procedure (based on the conditional independence assumption and on the hypothesis of selection on observables) could be subject to a motivation bias, which could still be present after controlling for the whole list of observable variables that we have presented.[12] When discussing the results, we should keep in mind this problem which possibly produces an overestimation of the average treatment effect.

6.2 The propensity score

The first step of the statistical analysis consists in estimating the probability to participate in the program. The set of control variables affecting this probability includes individual characteristics such as age, gender, marital status, number of children, education, nationality (French citizenship or not), and characteristics of the previous job such as its skill level, the size of the firm, the seniority in this job, and the reason for the layoff (plant closing or not).[13] We also include in this list some indicators of the local labour market

[12] If motivation affects both the probability to work and to enter the program, the treatment effect could be confounded by unobservables.

[13] We tried to robustify the specification of the selection model by including interaction terms (for instance, between gender and the number of children, between gender and age, etc.) and the past employment history

conditions. For that purpose, in a first specification, we incorporate the ratio U_j/V defined as the number of unemployed persons of type j divided by the number of vacanbs in the local labour market where the individual lives. The value of this ratio is taken either at the date of entry into the program (for trainees) or at the date of entry into the unemployment spell (for controls). For constructing the numbers U_j of unemployed persons, we consider six socio-demographic groups ($j = 1,\ldots,6$), each gender being split into three age groups (less than 30, between 30 and 49, 50 and more). In an alternative model, these ratios are simply replaced by indicators of the region of residence. The probability to participate in the retraining program is assumed to be generated by a logit model whose parameter estimates are reported in Table 1a. This table reveals that parameter estimates are quite similar under both specifications (in model 1 with U_j/V variables, and in model 2 with regional dummies).

First, we observe that the probability to join the program is lower for women, foreigners (non-French citizens), persons having at least three children, blue-collar workers, and for workers previously employed in a firm with more than 200 employees. Conversely, this probability is significantly higher for young workers (less than 25 years old), executives, those who incurred a collective layoff not associated with a plant closure, and for those who stayed more than three years in the previous firm. The ratio of the number of young unemployed workers over the number of vacancies in the local labour market has a significant effect on the probability of entering the program. However, this effect has opposite signs for both genders. The probability of participating in the retraining program increases with the relative number of young unemployed men, while it decreases with the relative number of young unemployed women. The same contrast is observed for the tightness ratios concerning older unemployed workers, but the associated parameter estimates are statistically significant at the 10%-level only. Model 2 reveals that the probability to participate in the retraining program is statistically higher in the Nord-Pas-de-Calais region. The region of common support calculated by the STATA module *pscore* appears to be large and similar for both specifications (see the last line in Table 1a). The numbers of deleted observations due to the common support condition are shown in Table

(for instance, the number of unemployment spells prior to the last job, and the duration of the longest previous unemployment spell) as additional covariates. None of these interaction terms or additional covariates (except the duration of the longest previous unemployment spell) appears to be statistically significant. However, when introducing the duration of the longest previous unemployment spell in the

1b; these numbers are quite low. Figures 1 and 2 represent the distributions of propensity scores estimated in each group (treated and non-treated) with models 1 and 2, respectively. These distributions look alike, except in the lower (respectively, upper) tail of the distribution since low (respectively, high) values of the estimated propensity score are more frequent for non-treated (respectively, treated) individuals.

Propensity scores estimated with models 1 and 2 verify the balancing property (according to tests implemented with STATA modules *psmatch2* and *pscore*). However this is not the case for the estimated propensity score resulting from the more general logit model in which ratios U_j /V and region dummies are simultaneously introduced as regressors. Consequently, matching estimates that are reported hereafter are those obtained with the propensity score resulting from model 1 (with ratios U_j /V). Matching estimates deduced from model 2 are quite similar.[14]

propensity score, the balancing score property is no longer verified. Consequently, this additional variable has been finally excluded from the list of regressors affecting the selection equation.

[14] They are not reported here, but are available from the authors.

Table 1a

Estimated parameters of the propensity score (logit models)

	Model 1		Model 2	
Variables	Parameter	Standard Error	Parameter	Standard error
Intercept	-2.1205	0.3433***	-2.0636	0.2500***
Mass layoff without plant closure	0.4898	0.1137***	0.4821	0.1131***
Woman	-0.2514	0.1248*	-0.2446	0.1243*
Less than 25 years old	0.4921	0.2798	0.4440	0.2805
Three children and more	-0.4772	0.1484***	-0.5110	0.1473***
Education: college and above	0.4406	0.1642**	0.3954	0.1630**
Foreigner (no French citizenship)	-0.3018	0.1452*	-0.2649	0.1438
White-collar worker	0.2665	0.1454	0.2997	0.1450*
High-skilled worker	0.3495	0.1640*	0.3753	0.1628*
Executive, top-manager	0.4287	0.2246*	0.4814	0.2236*
Seniority between 3 and 5 years	1.6228	0.1872***	1.6081	0.1859***
Seniority higher than 5 years	1.6932	0.1778***	1.6985	0.1767***
Firm size:				
- less than 50 employees	0.6999	0.1489***	0.7226	0.1479***
- between 50 and 100 employees	0.5526	0.2009**	0.6067	0.1997**
- between 100 and 200 employees	0.3908	0.2267	0.4631	0.2252*
Ratios U_j/V :				
- males below 30 years old	0.6104	0.2108**		
- males above 50 years old	1.8754	1.0424		
- females below 30 years old	-0.6750	0.2479**		
- females above 50 years old	-2.6068	1.6466		
Region of residence:				
- Nord - Pas de Calais			0.2788	0.1344*
- Ile-de-France			0.0420	0.1260
Log - likelihood	-1 055.14		-1 062.99	
Number of observations	1,912		1,912	
Region of common support	[0.1260154, 0.87726554]		[0.1132257, 0.87044135]	

Remarks: Statistical significance levels that are indicated correspond to 0.1% (***), 1% (**) and 5% (*).

Table 1b

Numbers of deleted observations due to the common support requirement

	Before matching			After matching			Deleted
	All	Treated	Untreated	All	Treated	Untreated	All
Model 1	1,689	728	961	1,686	725	961	0.18%
Model 2	1,689	728	961	1,673	712	961	0.95%

Figure 1
Distributions of propensity scores estimated with model 1

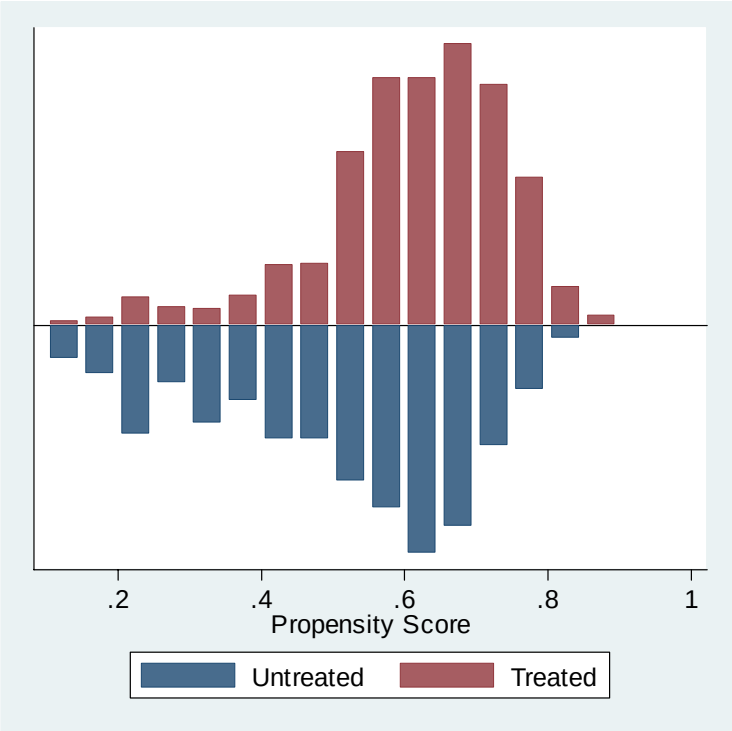
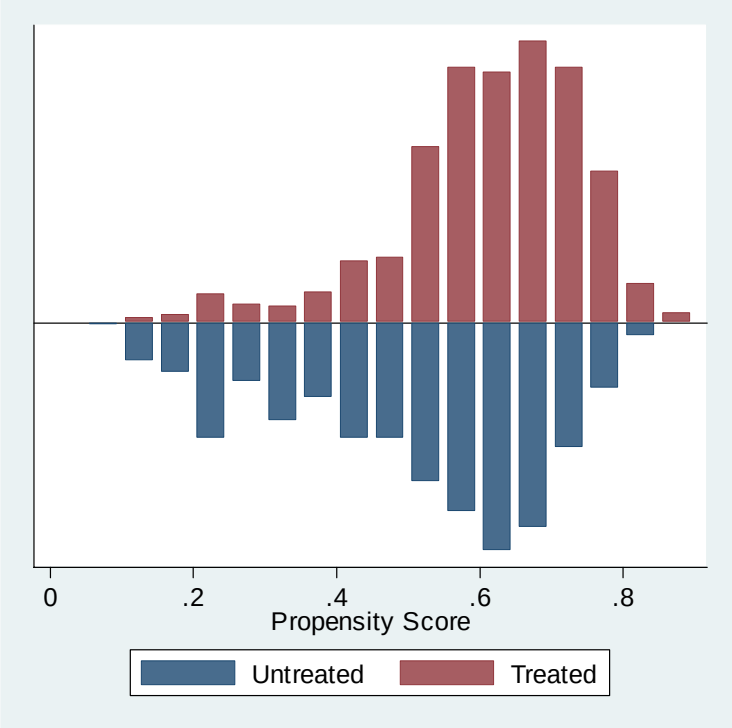


Figure 2
Distributions of propensity scores estimated with model 2



6.3. Matching estimates

The first outcome that we consider is the proportion of time spent in employment during the second and third years after the entry into the program (for the trainees) and into unemployment (for controls).[15] The results are reported in Tables 2. All tables give various matching estimates, obtained with different matching techniques: nearest-neighbour matching (without replacement), radius caliper matching, and kernel matching, this last method being implemented with three different kernel functions (Gaussian, biweight, and uniform). At each date, three average effects have been estimated with each matching method:

- the average treatment effect on the treated (ATT), or equivalently, the average effect of the program for the workers who actually participate in this program,
- the average treatment effect on the untreated (ATU), or equivalently, the average effect of the program for those who do not participate in the program,
- the average treatment effect (ATE), or equivalently, the average effect of the program for a randomly chosen laid-off worker.

The matching estimates are always statistically different from zero, at least at the 1% level of significance, and they indicate that the retraining program increases the proportion of time spent in employment by approximately 6.5 points during the second year and by approximately 5.7 points of percentage during the third year after the date of entry into the program. These estimated effects are similar for trainees and controls. Consequently, the estimated average effect for the whole sample has the same value.

As noticed in the introduction, Farber (1999, 2003) found that workers who lose their jobs are more likely to be reemployed in temporary jobs.[16] Thus it is particularly important to assess whether the retraining program help workers who participate to find a permanent job (namely, a job with a long-term labour contract) compared to otherwise identical non-participants. Consequently, we have also used matching techniques to estimate the effect of the retraining program on the proportion of time spent in regular employment during the

[15] Unfortunately the information about wages in this data set is too imprecise to consider the post-training wage level as the relevant outcome.

[16] This feature is crucial in a country like France where, according to Blanchard and Landier (2002), fixed-duration labour contracts have substantially increased turnover, without a substantial reduction in unemployment duration.

2nd and 3rd years after entry into the program.[17] Tables 3 contain the results of this exercise. They show that the retraining program has increased the time that the trainees have spent in regular employment by approximately 8 points of percentage during the 2nd year and by an amount between 5.5, and 6 points during the 3rd year. Thus, the increase in their reemployment rate is due to an increase of their reemployment probability in regular jobs, namely jobs under long-term contracts the increase.[18] The time spent in regular employment would have also increased for non-participants, by approximately 7.5 points of percentage during the 2nd year and by 5.7 during the 3rd year.

In a further analysis, we have compared the estimated effects of the program for different subgroups. More precisely, we have conducted the analysis by distinguishing genders, skill levels (two categories: white-collars, skilled workers and executives on one side, and blue-collar workers on the other) and age (three age groups). Outcome is the time spent in employment in the 2nd and 3rd years after the date of entry into the program. Results are reported in Tables 4, 5, 6a and 6b. First, matching estimates show that, in terms of this outcome, the retraining program is principally beneficial for men (Tables 4) and for adult workers between 30 and 50 years old (see Tables 6a and 6b). Within these categories, both trainees and non-trainees have, or would have, benefited from their participation in the program. This result could mean that the program has been mainly beneficial (and possibly better adapted) to the displaced workers with a higher labour market experience. This interpretation is in line with the conclusion of the study conducted by Kodrzycki (1997) who observes that, in the case of job training programs proposed to displaced workers in Massachusetts in the early nineties, "different types of training are used by different types of displaced workers and have different degrees of effectiveness": relatively short training programs (i.e. less than one year) which consist mainly in vocational training benefit mainly to workers with a higher ability and a longer work history, while training combining vocational and general education, which corresponds generally to longer programs (i.e. more than one year), is better adapted to workers with lower past

[17] In our study, permanent jobs correspond to long-term labour contracts. Fixed-term contracts, temporary agency work and subsidized jobs are excluded from this category. The limited size of our sample prevents us to do a more precise analysis by distinguishing other categories such as full-time jobs versus part-time jobs. The distinction between permanent jobs versus temporary jobs appears to be relevant in order to examine the average quality of the corresponding jobs. For instance, in 1996, which is the median year of our survey, the average net monthly wage of workers occupied in permanent jobs was equal to 10,170 French francs, and 87 % among them worked full-time; the same year, the average net monthly wage of workers occupied in temporary jobs was equal to 6,810 French francs, and 69 % only were employed full-time (source: "*Enquête sur l'emploi*", INSEE, Paris, 1996).

experience. Finally, let us remark that, in the 3rd year after entry, women who have been retrained spend more time in employment (4 percentage points more), but those who do not participate would have also gained (by the same amount). For the two skill groups that we consider (blue-collars vs. white-collars, high-skilled workers and executives), estimated effects of the program are statistically significant, but they are quite similar (see Tables 5).

Tables 2

Matching estimates of the effect of the retraining program on the time spent in employment

Outcome: proportion of time spent in employment during the 2nd year:

Matching methods	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.065** (0.022)	0.067*** (0.021)	0.067*** (0.020)	0.067*** (0.019)	0.067*** (0.020)
ATU	0.065** (0.022)	0.065** (0.023)	0.067*** (0.019)	0.067*** (0.021)	0.065** (0.023)
ATE	0.065** (0.022)	0.067*** (0.017)	0.067*** (0.017)	0.067*** (0.017)	0.066*** (0.019)

Remarks: Bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect. Sample sizes: 745 trainees, 576 controls.

Outcome: proportion of time spent in employment during the 3rd year:

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.058*** (0.015)	0.056*** (0.013)	0.056*** (0.014)	0.055*** (0.014)	0.055*** (0.013)
ATU	0.064*** (0.014)	0.058*** (0.015)	0.058*** (0.017)	0.057*** (0.019)	0.058*** (0.014)
ATE	0.062*** (0.015)	0.057*** (0.014)	0.057*** (0.012)	0.055*** (0.014)	0.057*** (0.014)

Remarks: Bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect. Sample sizes: 699 trainees, 572 controls.

Tables 3

Matching estimates of the effect of the retraining program on the time spent in regular employment (in long-term labour contracts)

Outcome: proportion of time spent in regular employment during the 2nd year:

Matching methods	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.092*** (0.018)	0.081*** (0.017)	0.082*** (0.016)	0.079*** (0.19)	0.081*** (0.015)
ATU	0.093*** (0.018)	0.075*** (0.017)	0.077*** (0.019)	0.073*** (0.019)	0.074*** (0.017)
ATE	0.093*** (0.017)	0.079*** (0.016)	0.080*** (0.015)	0.077*** (0.014)	0.078*** (0.017)

Remarks: Bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect. Sample sizes: 745 trainees, 576 controls.

Outcome: proportion of time spent in regular employment during the 3rd year:

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.069*** (0.014)	0.059*** (0.011)	0.060*** (0.011)	0.055*** (0.013)	0.057*** (0.012)
ATU	0.065*** (0.014)	0.057*** (0.012)	0.058*** (0.012)	0.056*** (0.012)	0.057*** (0.011)
ATE	0.066*** (0.012)	0.058*** (0.011)	0.059*** (0.012)	0.056*** (0.012)	0.057*** (0.012)

Remarks: Bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect. Sample sizes: 699 trainees, 572 controls.

Tables 4

Matching estimates of the effect of the retraining program on the time spent employment (by gender)

Outcome: proportion of time spent in employment during the 2nd year

Men: 404 trainees, 300 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.107*** (0.027)	0.099*** (0.027)	0.096*** (0.029)	0.098*** (0.022)	0.098*** (0.027)
ATU	0.070** (0.026)	0.071* (0.036)	0.075* (0.032)	0.076* (0.033)	0.075* (0.037)
ATE	0.090*** (0.029)	0.088*** (0.027)	0.088*** (0.027)	0.090*** (0.026)	0.090*** (0.027)

Women: 341 trainees, 276 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.028 (0.029)	0.047 (0.031)	0.044 (0.031)	0.054 (0.029)	0.049 (0.026)
ATU	0.045 (0.027)	0.051 (0.034)	0.047 (0.036)	0.051 (0.035)	0.051 (0.036)
ATE	0.036 (0.030)	0.049 (0.029)	0.045 (0.028)	0.052 (0.032)	0.050 (0.031)

Outcome: proportion of time spent in employment during the 3rd year

Men: 378 trainees, 297 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.086*** (0.022)	0.075*** (0.018)	0.074*** (0.015)	0.075*** (0.017)	0.075*** (0.016)
ATU	0.063** (0.019)	0.070** (0.028)	0.072** (0.023)	0.073** (0.029)	0.072** (0.024)
ATE	0.073*** (0.020)	0.073*** (0.018)	0.073*** (0.021)	0.074*** (0.023)	0.073*** (0.019)

Women: 321 trainees, 275 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.041* (0.017)	0.046* (0.022)	0.042* (0.020)	0.047* (0.022)	0.046 (0.025)
ATU	0.042 (0.022)	0.042 (0.022)	0.041 (0.023)	0.040 (0.024)	0.042 (0.023)
ATE	0.041* (0.020)	0.044* (0.019)	0.042* (0.019)	0.044* (0.021)	0.044* (0.021)

Remarks: In each table, bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect.

Tables 5

Matching estimates of the effect of the program on the time spent in employment (by skill level)

Proportion of time spent in employment during the 2nd year

White-collars, skilled workers, executives: 447 trainees, 320 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.064** (0.027)	0.070** (0.023)	0.071** (0.026)	0.072** (0.023)	0.072** (0.025)
ATU	0.069* (0.029)	0.065* (0.032)	0.065* (0.029)	0.068* (0.033)	0.063* (0.028)
ATE	0.068** (0.025)	0.068** (0.023)	0.069** (0.025)	0.070** (0.023)	0.069** (0.025)

Blue-collar workers: 298 trainees, 256 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.064* (0.028)	0.066 (0.034)	0.067** (0.026)	0.071* (0.035)	0.067* (0.036)
ATU	0.065 (0.035)	0.055 (0.042)	0.058 (0.041)	0.054 (0.039)	0.055 (0.038)
ATE	0.064 (0.038)	0.062 (0.035)	0.063* (0.028)	0.064* (0.032)	0.062* (0.031)

Proportion of time spent in employment during the 3rd year

White-collars, skilled workers, executives: 422 trainees, 320 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.064* (0.028)	0.066 (0.034)	0.067* (0.026)	0.071* (0.035)	0.067* (0.036)
ATU	0.065 (0.035)	0.055 (0.042)	0.058 (0.041)	0.054 (0.039)	0.055 (0.038)
ATE	0.064 (0.038)	0.061 (0.035)	0.063* (0.028)	0.064* (0.032)	0.062* (0.031)

Blue-collar workers: 277 trainees, 252 controls

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.076** (0.024)	0.069** (0.023)	0.069** (0.025)	0.073** (0.022)	0.071** (0.022)
ATU	0.066** (0.025)	0.065** (0.025)	0.068* (0.028)	0.064* (0.031)	0.063** (0.025)
ATE	0.066** (0.024)	0.067** (0.021)	0.068** (0.022)	0.069** (0.024)	0.068** (0.024)

Remarks: In each table, bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect.

Tables 6a

Matching estimates of the effect of the program on the time spent in employment (by age)

Proportion of time spent in employment during the 2nd year

Less than 30 years old: 133 trainees, 108 controls.

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.121* (0.051)	0.080 (0.056)	0.083 (0.053)	0.066 (0.066)	0.074 (0.057)
ATU	0.010 (0.055)	0.063 (0.061)	0.063 (0.061)	0.067 (0.062)	0.054 (0.060)
ATE	0.110 (0.059)	0.073 (0.057)	0.074 (0.044)	0.066 (0.055)	0.066 (0.048)

Between 30 and 50 years old: 503 trainees, 408 controls.

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.075*** (0.014)	0.084*** (0.024)	0.085*** (0.024)	0.086*** (0.021)	0.086*** (0.022)
ATU	0.082*** (0.027)	0.076*** (0.024)	0.079** (0.027)	0.074** (0.025)	0.077*** (0.024)
ATE	0.080*** (0.023)	0.081*** (0.023)	0.082**** (0.022)	0.081*** (0.023)	0.082*** (0.024)

50 years old and more: 109 trainees, 60 controls.

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.044 (0.056)	0.030 (0.044)	0.027 (0.037)	0.031 (0.043)	0.034 (0.046)
ATU	0.024 (0.062)	-0.001 (0.020)	-0.005 (0.064)	-0.011 (0.071)	-0.008 (0.071)
ATE	0.031 (0.060)	0.010 (0.041)	0.009 (0.030)	0.013 (0.041)	0.010 (0.051)

Remarks: In each table, bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect.

Tables 6b

Matching estimates of the effect of the program on the time spent in employment (by age)

Proportion of time spent in employment during the 3rd year

Less than 30 years old: 121 trainees, 108 controls.

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.070 (0.041)	0.070 (0.038)	0.071 (0.042)	0.052 (0.038)	0.065 (0.041)
ATU	0.079* (0.037)	0.059 (0.043)	0.059 (0.046)	0.057 (0.049)	0.055 (0.049)
ATE	0.074 (0.041)	0.064 (0.034)	0.065 (0.036)	0.054 (0.036)	0.060 (0.037)

Between 30 and 50 years old: 476 trainees, 404 controls.

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.060*** (0.017)	0.068*** (0.015)	0.068*** (0.016)	0.068*** (0.016)	0.068*** (0.017)
ATU	0.079*** (0.018)	0.071*** (0.017)	0.066*** (0.017)	0.064*** (0.022)	0.066*** (0.017)
ATE	0.076*** (0.016)	0.070*** (0.017)	0.067*** (0.016)	0.066*** (0.019)	0.067*** (0.017)

50 years old and more: 102 trainees, 60 controls.

Matching method	Nearest neighbour (no replacement)	Radius caliper	Kernel (Gaussian)	Kernel (biweight)	Kernel (uniform)
ATT	0.030 (0.030)	0.014 (0.031)	0.016 (0.030)	0.017 (0.035)	0.021 (0.034)
ATU	0.027 (0.036)	0.027 (0.043)	0.013 (0.055)	0.011 (0.041)	0.016 (0.033)
ATE	0.026 (0.036)	0.017 (0.028)	0.015 (0.028)	0.013 (0.029)	0.020 (0.031)

Remarks: In each table, bootstrapped standard errors are reported between parentheses. ATT is the average treatment effect on the treated. ATU is the average treatment effect on the untreated. ATE is the average treatment effect.

7. Concluding remarks

The aim of this article was to estimate the impact of a retraining program targeted to displaced workers on their return to permanent employment. This program, called "Convention de conversion", was set up in France during the eighties. It consisted in providing an immediate and individual support to displaced workers for a period of six months (beginning just after the dismissal) by proposing retraining and job seeking assistance. Our matching estimates show that this program succeeded in increasing the employment rate of trainees by approximately 6 points of percentage in the medium-term, namely in the second and third years after the date of entry into the program. This improvement is essentially due to an increase of their employment rate in regular jobs (i.e. jobs under long-term labour contracts). This last result is particular important since Farber (1999, 2003) noticed that, in the absence of any intervention, displaced workers are more likely to be reemployed in temporary jobs. We have also found that this French retraining program has been principally beneficial for adult workers between 30 and 50 years old, i.e. for the displaced workers with a higher labour market experience. Consequently, a longer program, combining intensively general and vocational education, could have been more beneficial for displaced workers with the shortest work histories.

Our findings confirm and complement two sets of previous results:

- First, intensive (re)training programs designed for laid-off workers have positive effects not only their subsequent wages (see, for instance, Jacobson *et al.*, 2005, and Stenberg and Westerlund, 2008), but also on their employment rates, and especially on their employment rate in regular jobs (with long-term labour contracts);
- Second, these programs, like some other active labour market programs (see, for instance, Lechner, 2004, Jespersen *et al.*, 2008, and Fitzenberger *et al.*, 2009, for other examples), have medium- and long-run effects on the employment rate of trainees.

In a further research, it would be worthwhile to focus on the effectiveness of retraining programs proposed to displaced workers laid off from shrinking industries (for instance, traditional manufacturing industries). In particular, it should been assessed whether such

workers (in particular, the oldest ones) need longer training programs, with a higher content in terms of general (and possibly vocational) education, and whether such programs help them to change occupations. In economies with changing job opportunities, this issue is crucial for public policy.

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Appendix

Table A.1

Descriptive statistics for non-participants and participants (percentages)

	Non – participants	Participants
Individual characteristics		
Between 16 and 25 years old	3.86	416
Between 26 and 39 years old	29.93	27.78
Between 40 and 49 years old	54.01	56.03
50 years old and more	10.2	14.05
Women	44.62	46.43
Foreigner (no French citizenship)	19.86	14.05
One child	22.95	23.14
Two children	22.10	26.07
Three children and more	18.50	12.60
Region of residence		
Paris-Ile-de-France	35.17	32.88
Nord-Pas-de-Calais	29.24	34.03
Provence-Alpes-Côte-d'Azur	35.59	33.09
Educational level		
Primary education	12.97	13.01
Junior high-school	13.79	10.61
Upper high-school	8.83	7.80
Professional school	42.48	41.31
College	8.00	8.84
University	13.93	18.42
Tenure in the previous job		
Between 2 and 3 years	21.93	6.04
Between 3 and 5 years	25.52	30.80
Between 5 and 10 years	52.55	63.16
Skill level in the previous job		
Unskilled blue-collar worker	22.76	17.07
Skilled blue-collar worker	22.62	21.85
White-collar worker	32.69	33.30
High-skilled worker	14.76	18.02
Manager	7.17	9.78
Reason for layoff		
Mass layoff without plant closure	69.24	70.76
Mass layoff with plant closure	30.76	29.24
Firm size (previous job)		
Less than 50 employees	62.48	65.97
Between 50 and 99 employees	10.90	12.59
Between 100 and 199 employees	7.45	7.80
More than 200 employees	19.17	13.63
Number of observations	728	961

Source: French Ministry of Labour

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