Do labor market policies have displacement effect? Evidence from a clustered randomized experiment *

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Abstract

"Active Labor Market" policies, where job seekers are given help to identify job offers and receive interview have become popular, particularly in Europe. The impact of job seeker counseling policies on its beneficiaries may be due to a combination of an increase in the number of job offers or to displacement effects. In this paper, we report on the results of a randomized experiment designed to evaluate the impact of reinforced job market counseling on the labor market outcomes of young, educated jobseekers in France. In order to identify both the direct and displacement effects, we use a two-step design. Before the experiment start, the proportions of jobseekers to be assigned to treatment are randomly drawn for each labor market (*e.g.* cities). Then, in each agency, jobseekers are assigned to treatment randomly, according to this previously drawn proportion. The program significantly improves the chance that a "treated" job seeker finds a job, but there is no evidence of any crowding-out effects on non-treated job seekers even where the proportion of treated people is high.

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1 Introduction

Job search assistance program are popular in many industrialized countries, and a growing literature attempts to assess its effects. Unlike other Active Labor Market Policies, whose effects are globally considered as weak, most studies tend to find a significant and positive (impact of counseling especially for jobseekers with low risks of long-duration unemployment see reviews in Kluve (2006), Card, Kluve, and Weber (2010)) . These studies are based on the comparison of generally short-run labor market outcomes between counseled and non-counseled jobseekers (see Blasco and Rosholm (2010) for a paper on long-run outcomes). Studies backed on experimental data are still relatively rare (Van den Berg and Van der Klaauw (2006), Rosholm (2008), Behaghel, Crépon, and Gurgand (2010)) but the estimates also tend to be positive.

However, an important criticism often expressed against these studies is that potential displacement effects are not taken into account: job seekers who benefit from counseling may be more likely to get a job, but at the expense of other unemployed worker. This may be particularly likely in the short run, when vacancies do not adjust: non-treated unemployed workers (the control group, in randomized experiments) could be partially crowded out.

The presence and the magnitude of these displacement effects are key policy questions (see for example Cahuc and Le Barbanchon (2010) for a theoretical recent contribution): if all a policy does is to lead to a musical chair games between unemployed workers, then the estimated imparts are of course gross over-estimate of its welfare implications.

This issue has long been a concern for economists and policy makers. For example, Johnson (1979), Atkinson (1987), Meyer (1995) point out that displacement effect would alter effects of training or unemployment insurance policies. Heckman, Lochner, and Taber (1999) makes a similar argument in the case of a tax and tuition subsidy policy and point out that the general equilibrium effect of such policies would be lower than the partial equilibrium effects, because it would lower the returns to education. Lise, Seitz, and Smith (2004) develop a framework designed to evaluate the potential magnitude of global impact of the Canadian Self Sufficiency Program. They compare the results of a randomized experiment designed to evaluate partial equilibrium effects of this program with a structural model taking into account equilibrium effects and calibrated on the same experimental data. They show that the externalities may cancel the apparent positive effects and could even lead to negative global effects. They do not directly measure these externalities, however.

There have also been attempts to directly measure externalities. For example, Angelucci and Giorgi (2009) use a village-level randomized experiment to put into evidence positive impact on the consumption of ineligible households of a cash transfer program. They compare ineligible households from treated villages to households from untreated villages. Finkelstein (2007) exploits the geographic variability of insurance coverage to compute the real effects of Medicare on health consumption. She shows that they are six times larger than the results issued from the individual-level random experiment lead from 1974 to 1982.

There are fewer studies focusing specifically on the labor market: In their evaluation of the British New Deal for Young Unemployed, Blundell, Dias, Meghir, and Reenen (2004) compare ineligible people in the areas affected by the program to those in areas not affected by the program. They do not get significant estimates for indirect effects on untreated youngsters of treated areas. Ferracci, Jolivet, and van den Berg (2010) study the how the effect of a training program for young employed workers in France varies with the fraction of treated workers, and do find that the effect diminishes. Pallais (2010) estimates the market equilibrium effect of a short term employment opportunity given to workers in on on-line market place, and find surprisingly little displacement.

One potential issue with all these studies is that, even when the individual treatment is randomly assigned, or as good as randomly assigned, the number of people who are "treated" within a market is itself not randomly assigned. The comparison across markets may thus not lead unbiased estimates of the equilibrium effects.

To address this issue, we implement a two-level randomized level to the evaluation of a job seeker counseling program in France.

In the particular program we consider. Private providers are contracted to provide placement services for young graduates that had spent at least 6 months in unemployment. The private provider is paid partially on delivery, i.e. conditional on the individual finding a job that lasts for at least 6 months. From August 2007 to June 2009, 10 000 young graduates were to be treated over 235 local areas in 10 regions [1]

One of the main innovations of this experiment rests on a two-level randomization. The first randomization is at the area-level. In a first stage, before the experiment started, each one of the 235 local employment was randomly assigned the proportion P of jobseekers that were going to be assigned to treatment: either 0%, 25%, 50%, 75% or 100%. The randomization was done in a fully stratified way: areas were assign to quintuplets, each was assigned to a different treatment group. The second level of randomization took place within each treated area: in each area, a fraction P of the eligible unemployed were randomly selected to be assigned to treatment. Jobseekers assigned to treatment are offered the option enroll in the program (about a third actually enrolled). For those who are assigned to the control group, nothing changed: they keep on being followed by ANPE counselors.

A similar two-steps design was employed in Duflo and Saez (2002) to measure spillover effects of an information fair for benefits, and Banerjee, Duflo, Keniston, and Singh (2010) to measure spillover effects of training policemen in India. However, to the best of our knowledge, there have not been similar experiments in the labor market literature. Our experiment is also unusual for its coverage (about half of France) and scale (10,000).

This design allows us to test for externalities on untreated workers (by comparing untreated workers in areas with some treated workers and those in areas with no treated workers), but also, thanks to the variability of P, to investigate whether the effect of the treatment on the treated, and on the untreated, varies with P.

Overall, we find that the program has a significant impact on the probability to be employed after 8 months, although the effects are not persistent in the longer term. Interestingly, however, we find no evidence for any negative externality, even in the short run when the direct effects were large.

The program and the institutional context are described in the next section. Section 3 gives details for the experimental design and the data. Section 4 presents the basic evidences (impact

[1] These areas correspond to the areas covered by each ANPE local employment agencies.

on the treated, global effects on the different groups of areas). Section 5 is devoted to the estimation of externalities. Section 6 concludes.

2 Institutional context and description of the program

2.1 Background: Placement Services in France

Until 2005, the French Public agency ANPE (*Agence Nationale Pour l'Emploi*) had, from a legal point of view, the monopoly of placement services. In particular, employers were legally obligated to communicate their vacancies to ANPE. This monopoly was though mainly theoretical. ANPE outsourced a part of its counseling and placement activity to external private operators. Some segments of unemployed were guided by others networks: *e.g.* APEC (*Agence Pour l'Emploi des Cadres*) specialized in placement for executives and managers or *Missions Locales* for unskilled youth.

In 2005, the Social Cohesion Law broke this virtual monopoly. In particular, temporary work agencies were allowed to openly propose their counseling and placement services to any job-seeker. The public operator (which was renamed *Pole Emploi* in 2008) has nevertheless retained a prominent role, because every unemployment insurance (UI) recipient must meet her ANPE caseworker at least once per month and follow her prescriptions, in order to remain eligible for benefits.

As a private placement market did not spontaneously emerge, the government and unions decided to increase the number of partnerships between the public operator and private actors.Some specific segments of job seekers were targeted, starting with those that ANPE was known to have more trouble to deal with. The idea was borrowed from the German Hartz reforms (Jacobi and Kluve, 2007), in which each local employment office must contract with a "Personal Service Agentur" (PSA), often a temporary work agency, and subcontract to them the responsibilities of dealing with a certain number of jobseekers. The private provider receives a lump sum payment if the jobseeker is successfully placed in employment.

The French experience with private counseling has not been as systematic as the German one. But, unlike the German case, French policy makers set up two randomized experiments to evaluate the effects of subcontracting placement services to private providers: one was dedicated to jobseekers with high risk to stay a long time in unemployment (Behaghel, Crépon, and Gurgand, 2009) while the other one focused on young graduates that spent at least six months in unemployment. This paper focuses on the latter experiment.

There is a growing recognition in France that universities may not prepare their graduates very effectively for finding employment. In 2007, three years after they completed their course, only 68%-75% of University graduates had a stable job. Reports (Commission Hetzel, 2005) emphasized the lack of any job market experience among young graduates from the universities (internships or summer jobs are rare) and recommended introducing specialized counseling services for this segment of job seekers.

In 2007, the Ministry of Labor decided to experiment with the delegation to private providers of placement services for young graduates that have spent at least six months in unemployment. It was believed that private providers (particular temp agencies) would be better able to find jobs for young graduates than the public provider, due to their experience in this particular segment of the market.

2.2 Description of the program

In each of the ten regions selected for the experiment, an invitation to tender was issued. Private operators were selected on the basis of the services they proposed to provide and their price. Six regions were delegated to for-profit operators, among which five were subsidiaries of temp agencies. In four regions, not-for-profit organizations were selected.[2]

The program breaks down into two main phases:

- Phase I aims to place jobseekers in employment. Within the six first months of the program,

[2] Regions in which for-profit operators were selected are: Haute-Normandie, Lorraine, Ile de France, Pays de la Loire, Picardie, Réunion. Regions in which nonprofits were selected are: Centre, Nord-Pas de Calais, Provence Alpes Cote d'Azur, Rhône-Alpes.

the private employment agency counsels the job seeker and helps her to find a durable job (lasting more than six months).

- Phase II aims to stabilize the former jobseeker in his job. During the first six months of the job, the individual is followed and advised by the agency. The aim of this phase is to help her to maintain in her job or to find a new one if she resigns.

The program also includes an incentive remuneration scheme of private agencies. Specifically, for each enrolled job seeker, the provider get paid in three stages, conditional on fulfilling three consecutive objectives.

- Enrollment: when a job seeker is enrolled in the program, the private agency receives the first payment.
- Entering a durable job: when a job seeker signs a contract for job lasting more than six months, the second payment occurs.
- Being employed after six months: six months after the entry in a durable job, the last third is given to counseling company if the former jobseeker is still employed.

The total amount (all three payments together) ranges from 1600 to 2100 euros, depending on the company's initial bid.

3 Experimental design and Data

3.1 Experimental design

As described in the introduction, the randomization took place at two levels: labor market area, and individuals within market.

The experiment took place in 235 public unemployment agencies, scattered into 10 administrative regions (about half of France). Each agency is considered to represent a small labor market, within which individual situations may interfere. On the other hand, the agencies cover an area that is sufficient large, and workers in France are sufficiently immobile, that we assume that no spillover can take place across agencies.

In order to improve precision, we first formed groups of five agencies that are similar in size and characteristics of the local population: we obtain 47 5-tuplets. Within each 5-tuplets, each agency was randomly assigned a proportion of treated $P = 0, 0.25, 0.50, 0.75, 1$: one agency has no treated, one has 25% of its target population treated, and so on.
Individuals were then randomized at the individual level, following the proportion drawn in the first stage. Every month from September 2007 to November 2008, the youth that had entered the target population (having spent 6 month unemployed or 12 month over the last 18 months) were identified in each agency. The list was transmitted to us and we randomized the relevant proportion independently in each agency. The list of individuals thus selected to be potential beneficiaries of the program was then passed to the relevant counseling firm for the area, who was in charge of contacting the youth and offer her to enter the program. Entry was however voluntary, and the youth could elect to continue receiving services from the regular local agency instead. No youth from the control group could be approached by the firm at any time.

We have 14 consecutive cohorts, of which we use cohorts 3 to 12 in this paper.[3] Overall, 30,343 individuals took part in the experimental design (Table 1).

3.2 Data

There are three sources of data for this experiment. First, administrative files with the list of jobseekers reaching the eligibility conditions were provided by Pôle Emploi to the Ministry of Labor. This dataset is rich and exhaustive. It contains age, number of months spent unemployed during the current spell, id of the public employment agency, postal address, as well as the kind of job the person is searching.

A second set of data comes from the administrative files transmitted by private counseling firms. In order to claim payment, the firms would send the lists of jobseekers who actually entered the

[3]Data collection was focused on these cohorts for several reasons. First, cohorts 1 and 2 were given up as it appeared that it took a couple of weeks to the private operators before they could propose a stable treatment. Cohorts 13 and 14 were given up as, at the same time, the Ministry opened a profitable call for tender for jobseeker counseling. Anecdotal evidence suggest that private firms were more focused on this second operation and stopped paying attention to the first program.

counseling scheme. Acceptance was conditional upon the jobseeker filling and signing a form. Copies of these forms were returned and checked to ensure that firms were not overdeclaring the number of jobseekers they were actually counseling. We use this dataset only to measure the take-up of the program.

Finally, the main source of information is an endline survey.[4] 29,636 jobseekers were sampled in this survey (virtually all of the 30,343 members cohorts 3 to 12). The survey had 4 waves: 8 months after random assignment, then 12 months, 16 months and 20 months after. In order to limit collection costs and to increase the response rate, the survey was short (10 minutes for the first wave, 5 minutes for the others). Moreover, the survey combined three collection modes: internet, telephone, and paper questionnaire. As a result, response rates were high: As shown in table 1, 25,904 people (87%) answered to at least one of these waves and 23,320 (79%) answered at least to the first one, 8 months after.
Table 2 shows the response rate for each of the four waves for everyone, and conditional on having been assigned to treatment or control group. For every wave, the response is above 70%, and the jobseekers assigned to treatment are only 1 percentage point more likely to answer than the ones assigned to control. Table 16 displays the response pattern in panel. 16,514 answered to all the four waves. (je ne sais pas si c'est utile d'avoir un tableau pour ça)

The first survey started in August 2008 and lasted until May 2009; the last survey started in August 2009 and lasted until May 2010. The surveys included questions about the employment situation at the time of the survey (wage, type of contract, part time or not, occupation) as well as the one at the time of the random selection. Highest degree obtained, family situation, as well individual nationality or parents' nationality are also detailed. We also asked how many times they met their counselor and what kind of intervention they benefitted from. Finally, individuals assigned to treatment were asked the reasons why they thought they would benefit

[4]Unfortunately, administrative data on employment could not be linked to the experimental group for administrative/confidentiality reason, and administrative data on unemployment is not reliable, since young people who are not registered at the employment office could either be employed or have stopped looking for a job.

from entering the program (if they accepted) or why they refused (if they did).

Table 3 presents summary statistics for all 29,636 jobseekers that were sampled in the survey. The last two columns provide balancing tests run to ensure that assignment to treatment was indeed random. Column four presents the coefficient of treatment assignment in a regression in which the explained variable is the variable of interest and strata of employment agencies interacted with cohorts dummies are controlled for. At 5%, balancing tests reject coefficient equality to zero only once, for gender. In the appendix, we provide another check that selection was indeed random (see figure 2).

Most individuals in the sample are in their twenties. This is not surprising as one of the eligibility conditions was to be aged less than 30. The median age is 26, and the distribution looks skewed to the right. Another eligibility condition involved length of the unemployment spell; to be eligible, individuals has to be jobseeker for more than 6 months or to have cumulated more than 12 months of jobseeking in the last 18 months. This condition explains why individuals unemployed for 7 months or more are overrepresented in the sample. Note that only 9% of the sample have been unemployed for 18 months, or more. Because these jobseekers are still young and have had jobs for some very limited periods, most of them (69%) are not receiving unemployment benefits. Another striking fact is that nearly two thirds of those jobseekers are women. finaly, one third of the sample has a vocational degrees at the college level ("Bac+2"). Higher university degrees ("Bac+3" and more) represent another third. In contrast, degrees from engineering and business schools (which are mostly elite institutions) remain scarce: they make less than 2% of the sample.

Table 13 presents more summary statistics for variables that have no reason to be balanced, linked to location or cohort. Regions Rhone-Alpes and Nord Pas de Calais are the two largest contributors the experiment. 62% of the sample population are in regions in which the counseling provider is a for-profit firm. Cohorts 3 and 4 are more numerous than the following ones. The number of individuals then reflects the short-term trend in the labor market.

3.3 Take up

Once they are assigned to treatment, some jobseekers end up in the program, while others do not. Although a take up of about 50% was expected, the take up rate ended up around 35%. This rate is remarkably stable across cohorts (Figure 1).

Take up rates are similar across agencies with different proportions of individuals assigned to treatment (see Table 4): between 34.3%, in agencies with assignment rates of 75%, and 35.5% in agencies with assignment rate of 25%. It suggests that there are no spillover effects over take up behavior.

Tables 14 and 15 present the characteristics that are associated to the probability to take up, from a probit regression. Some individual characteristics are strongly correlated with low take up: being a female, having a high education level, or receiving unemployment benefit, probably because benefit recipients are more compliant to counselor's advice, as they may loose benefit otherwise. There is also some heterogeneity in take up rates accross regions (see also Figure 1). Take up is somewhat higher for cohorts 4 to 10, but orders of magnitude remain very similar.

Part of the reason for the relatively low take up is that, at the time the young were identified in the inflow, a significant proportion of them had already found a job (58%). There can be two reasons for this: either identification of the youth was concomitent with job finding; or the job they hold is compatible with registration with the unemployment services (a feature known as "activité réduite"). Both Table 14 and 4, indicate that take up rates are significantly higher for individuals who did not have a job upon assignment to the program. In this population take up rises to 44%. The follow up survey asked for reasons not to enter the scheme: 46% of the assigned who did not enter treatment reported doing so because they already had or were about to start a job, and 11% more because they were studying. Only about 17% of them answered that they felt that the counseling program was useless or too costly to enter.

Because the effects of the program could potentially be quite different for people who already had a job and for people who did not, in what follows, we will present the results below for individual characterized by their situation at baseline (employed or not).

4 Conceptual Framework

In this section, we propose a very simple model that will help guiding the interpretation of the results. In the model, the caseworker (or the firm that employ them) can do two things: generate net new jobs by improving the rate at which workers are matched to firms (for example, by calling specific firms who may have posted a job that correspond to the profile of the youth they work with and suggest they create a job; or by giving advice to youth on how to behave in an interview, that increase the rate at which firms are willing to hire workers); and help the people they work with jump in front of the "queue" of unemployed workers.

To fix ideas, denote λ the inflow rate into long-term employment for a particular individual:

$$\lambda = P[y = 1|y_{-1} = 0]$$

λ varies as a function of the environment (notably, the local proportion of treated) and the individual treatment status,

- The baseline case is for areas where no one is treated

$$P[y = 1|y_{-1} = 0, P = 0] = \lambda(0)$$

- In areas where the proportion of treated p is positive, denote the inflow rate of control-group individuals as:

$$P[y = 1|y_{-1} = 0, T = 0, P = p] = \lambda(p)$$

- In these areas, the inflow rate of treated individuals is:

$$P[y = 1|y_{-1} = 0, T = 1, P = p] = (1 + \alpha)\lambda(p)$$

In the last equation, we assume that the treatment provides a multiplicative bonus α on the inflow rate into long-term employment. This may be interpreted as a relative search efficiency gain for the treated (this is the "queue jumping" effect). The baseline entry rate $\lambda(p)$ is a function of the proportion of treated, for two reasons:

1. Keeping the number of vacancies equal, increasing the search efficiency of some jobseekers harms those who do not benefit from the treatment.

$$\begin{aligned} pP[y=1|y_{-1}=0,T=1,P=p]+(1-p)P[y=1|y_{-1}=0,T=0,P=p] \\ = P[y=1|y_{-1}=0,P=0], \forall p \end{aligned}$$

   or

$$p(1+\alpha)\lambda(p)+(1-p)\lambda(p)=\lambda(0), \forall p$$

2. In a simple matching model (see Cahuc and Le Barbanchon, 2009), increasing the proportion of treated also increases the total search efficiency. This drives employers to increase the supply of vacancies. The higher the proportion of treated, the higher the increase in vacancies posting. Let $r(p)$ be the effect on the inflow rate into long-term employment, an increasing function of p such that $r(0)=0$, so that:

$$p(1+\alpha)\lambda(p)+(1-p)\lambda(p)=(1+r(p))\lambda(0), \forall p$$

This last equation gives an expression for $\lambda(p)$:

$$\lambda(p)=\lambda(0)\frac{1+r(p)}{1+\alpha p}$$

If $r(p)\approx rp$ and if α and r are small, a first order approximation gives:

$$\begin{aligned} P(y=1|y_{-1}=0,T=1,P=p) &\approx \lambda(0)[(1+\alpha)-(\alpha-r)p] \\ P(y=1|y_{-1}=0,T=0,P=p) &\approx \lambda(0)[1-(\alpha-r)p] \end{aligned}$$

Note that the slopes of the two lines are the same: it proceeds from the hypothesis that the introduction of a new counseling operator makes available new vacancies for every eligible people (treated or non-treated).

The standard hypothesis of a inelastic stock of vacant jobs corresponds to the case $r=0$. If $\alpha>0$ the situations of the control and treated group are summarized in the figure 6. Externalities are then maximal. If $r=\alpha$, there are no externalities at all. In this case, situations for control and treated groups do not depend of P. When $r\leq\alpha$, other unemployed workers are partially displaced. One could conceivably also get $r\geq\alpha$. As P increases, the activity of the new operator might generate more and more vacant jobs in a way that is more than proportional to the number of offers they have.

The empirical counterparts to the empirical moments can be written:

$$y = \lambda(0)\alpha T + \lambda(0)(r - \alpha)P + \lambda_0 + \varepsilon \tag{1}$$

Our empirical design provides a number of instruments to estimate this equation (treatment assignment, and treatment assignment interacted with the fraction of people assigned to treatment). In what follows, we start with presenting reduced form estimates, and conclude by estimating the parameters of the model.

5 Results

5.1 Did the program help its intended beneficiaries?

5.1.1 Estimation

A first step is to establish whether or not the program improved the probability that its intended beneficiaries get a job.

The randomization took place into steps, both within strata (quintuplet) and within each city. A first possible analysis would be to treat all the treatment and control group members symmetrically, regardless of the city they were drawn from.

$$y_{ic} = \alpha_1 + \beta_1 TP + controls + \epsilon \tag{2}$$

Where y_{ic} is a labor market outcome for individual i in city c. The two outcomes we consider are : employed, and "durably employed" (e.g. employed in a indefinite duration contract, or a fixed length contract of more than 6 months).

TP a dummy equal to 1 if the individual effectively enrolls into the program The control variables include a set of quintuplets dummy, a dummy for entry date into the program, and individual level control variables (age, gender, and education). Since treatment assignment is endogenous, it is instrumented with a dummy $ZP = 1$ if the individual was assigned to the treatment group, 0 otherwise. Standard errors are clustered at the labor market level.

The reduced from equivalent is thus:

$$y = \alpha_2 + \beta_2 ZP + controls + \upsilon \tag{3}$$

A standard active labor market policy experiment would typically not include sites (cities) with just treatment or just control variables, and would consider the second level of randomization (the city) as the strata:

$$y_{ic} = \alpha_3 + \beta_3 TP + d_c + controls + \epsilon \tag{4}$$

In this regression, d_c is a set of "ALE" dummies: we are now compare treated and control workers within each labor market.

Comparing the results of estimating equations (2 and 4) will give us a first indication of the importance of externalities: In the absence of externality, we won't be able to reject the equality of β_1 and β_3, although β_1 will be more precisely estimated (since the 100% sites and 0% sites) do not contribute to the estimation of β_3. However, with negative externalities on the control group in treated areas, one would expect to find $\beta_1 < \beta_3$, since the difference between workers *within* labor market should be larger than the different between workers in treated areas and workers in completely untreated areas.

5.1.2 Results

The results are presented in table 5.

Panel A shows the overall effect on the treated individuals, comparing to all the control individuals. Panel A1 shows the reduced from estimate (of being assigned to the treatment group) and panel A2 shows the IV estimate (of actually participating). Overall, the program had no significant effect on employment (column (1)) or durable employment (column (4)). However, recall that about half of the sample was actually already employed at baseline, and the main objective of the program is to help unemployed individuals finding a job.

Focusing on the unemployed, the program increased the probability that youth that were previously unemployed found a job by 3.5 percentage points, or 6.5 percent (Panel B, column 2), and a durable job by 3.9 percentage points or 9.5 percent (panel B, column 5). For profit operators had larger impacts than non for profit operators. Compared to other programs of this type this is a reasonably large effect.

Panel B includes ALE dummies, so that the comparison is now done within a specific labor market: in the presence of negative externalities on the control groups, these estimate would be larger. In fact, the point estimates are essentially identical (and the estimates are of course statistically indistinguishable), although they are less precise (this is not surprising since two fifth of the sample does not contribute to the estimation).

This first set of estimates suggest that this program has significant effects in helping those who were previously unemployed find a job. The preliminary evidence does not suggest that externalities should be large.

To increase the precision of the estimates and estimate the two parameters α and r, we now turn to a more systematic exploitation of our experimental design.

5.2 Reduced form and Instrumental variable estimates estimates: direct effects and externalities

5.2.1 Reduced form

We now estimate the effect a fully unconstrained reduced form model, and test whether the effect varies being assigned to treatment or to control by assignment probability. It is worth to note that this is possible because our experimental design involves two types of controls: control located in areas with non zero probability of assignment, and a super control group composed of individuals located in areas randomly chosen to have zero probability of assignment to program. The specification we consider is the following.

$$\begin{aligned} y &= \alpha_{25} Z P_{25} + \alpha_{50} Z P_{50} + \alpha_{75} Z P_{75} + \alpha_{100} Z P_{100} \\ &+ \beta_{25} (1-Z) P_{25} + \beta_{50} (1-Z) P_{50} + \beta_{75} (1-Z) P_{75} \\ &+ controls + u \end{aligned} \tag{5}$$

where Z is the assignment to treatment variable and P_x is the dummy variable at the area level associated with an assignment rate of $x\%$. Control variables are individual characteristics (gender, education...) and the set of the 46 dummy variables for each 5-uplets. Standard errors account for within area correlations between residuals and are robust to heteroskedasticity. The parameter α_x measures the effect of being assigned to treatment in an area where $x\%$ of the eligible population was assigned to treatment compared to the super control where no one was assigned to treatment. Coefficient β_x measures the effect of being assigned to control in an area where $x\%$ of the eligible population was assigned to treatment compared to the super control where no one was assigned to treatment. This parameter measures the externality effect of the program. Note that there are four parameters α but only three parameters β as there is no room to estimate the effect on non assigned when the whole eligible population is assigned to the program.

5.2.2 Average Treatment on the Treated: Instrumental Variable Estimate

Before moving to the estimation of the structural model, we estimate a simple "summary" model of the effect of the program on those actually treated and on the control group:
The model constrains the treatment effect to be independent of the assignment probability.

$$y = \alpha T.\mathbb{1}\{P > 0\} + \beta(1 - T).\mathbb{1}\{P > 0\} + controls + u \tag{6}$$

Under the assumption that the externality is the same for those who are assigned to the control group and those who refuse the treatment, α is an estimate of the average effect of the treatment for the treated, and β an estimate the average externality.
The set of instument ZP_{25}, ZP_{50} , ZP_{75}, ZP_{100}, $(1-Z)P_{25}$, $(1-Z)P_{50}$ and $(1-Z)P_{75}$. Equation refeq:att:str is over identified, and an overidentification test (Sargan test) tests the hypothesis that both the treatment effect and the assignments are independent of the number of people assigned to treatment.

5.3 Model estimation

The empirical counterpart of the model given us the following equation:

$$y = \lambda(0)\alpha TP + \lambda(0)(r - \alpha)F + \lambda_0 + \varepsilon \tag{7}$$

or, rewriting:

$$y = \lambda(0)\alpha(TP - F) + \lambda(0)F + \lambda_0 + \varepsilon \quad (8)$$

where F is the fraction of unemployed that effectively enter treatment. We can obtain $\lambda(0)\alpha$, and $\lambda(0)r$ from an instrumental variable regression, of y on $TP - F$ and F, using ZP_{25}, ZP_{50} , ZP_{75}, ZP_{100}, $(1 - Z)P_{25}$, $(1 - Z)P_{50}$ and $(1 - Z)P_{75}$ as instruments.

5.4 Results

5.4.1 Reduced form

Tables 7 presents the results of estimation on the outcome "being employed in a durable fixed term contract" (durable fixed term contract are contracts with duration of at least six months). This is a key outcome variable as obtaining such a contract is the minimum requirement for operators to be paid for a successful placement.

As before, the four columns correspond to two different types of operators and two different populations. Column 1 gives the results for the the entire sample, column 2 gives the results for the individuals who were unemployed at baseline (the sample of interest). Column 3 gives the results for the entire sample, but includes only for profit operator, and column 4 gives the results for the for profit operator for those who were not employed before.

The seven first rows present the estimated coefficients for the detailed model of equation 5 and their standard errors.

Focusing, as before, on the sample of individuals who were unemployed at baseline, we find an increase in 0.006 to 0.027 percentage point in the probability to be employed across all operators, and 0.014 to 0.056 for the private operators. The estimates are significant only in places where 100% of the eligible youth were assigned for all operators, or where 75% or 100% of the youth were assigned.

The assignment to treatment effect in 100% areas is a key policy parameter. It measures the average of the program effect on the treated and the potential negative externality on these that refused to enter the program, assuming that everyone is eligible in an area. It just tells us what the effect of a "scaled up" program would be.

For those who were previously unemployed, the effects is 2.7% for all operators, and 5.6% for the private operator (a large proportional effect since the control group is 16%).

5.4.2 Instrumental variables estimates: treatment on the treated, and model estimates

Table 8 presents the results for the effect of treatment on the treated and the externalities on the untreated (non-compliers or assigned to control). The estimated effect on people who were employed at baseline is 4.3 percentage points for all operators, and 7.8 percentage points for the private operators (recall that the mean of this variable is roughly 16%, suggesting a large effect, about 0.20 of a standard deviation, and a more than 50 percent increase in the probability of finding this type of contracts).

Two fact suggests that crowding out effects are small: first, the estimate of the effects for the untreated people in treated areas are uniformly negative. Second, the Sargan test does not reject the hypothesis that the treatment effects and all the externalities effects are the same in all groups, which again is consistent with absence of crowd outs.

Finally, table 9 report the estimates of equation 8. The upper part of the two tables report the coefficients of $T - P_T$ and F that enable to estimate the coefficients α and r. The bottom part gives the estimate for $r - \alpha$ and test the hypothesis $H_0 : r \equiv \alpha$.

Focusing on those who were unemployed at baseline, we estimate $\alpha = 0.035$ and $r = 0.053$. The two estimates are not significantly different. This suggest that the private operators were about equally good at improving the chance that a treated person finds a job, and to increase job offers available for everyone: this effect appears to cancel out, which is why there are not negative externalities on the unemployed. For the not-for profit operators, the point estimate of α is actually smaller than r, and it is insignificant, though we cannot reject equality of r and α in this case either. If we were to take these estimate seriously, it would suggest that the not-for profit

An important caveat is that the externality is measured for workers in our experiment, the other educated unemployed. If they are substitutes with other workers, they may be crowding *them* out. In principle, we can look at the externality on non targeted unemployed in the same labor markets using the same design. In practice, if we find no evidence of externality, it may be because there are not very many people who are eligible for this program, so their number is too small to make a difference.

5.5 Other outcomes

Impact on alternative employment outcome Until now, the news is quite positive for the program: we find fairly large treatment effects, at least for workers that were initially unemployed (the target group), and particularly for for profit operator. And furthermore, there appears to have been no externalities on other people eligible for the same program.
The news become a bit weaker when we also consider alternative outcome variables related to employment, as reported in figure 10. We get no significant effects neither for general employment, nor for indefinite-term contracts. The counseling program only affects the weakest form of the link with employment that allows operators to be paid.

Impact on counseling services We aim here at characterizing enhanced counseling services by private agencies. First of all, meetings with caseworkers are more frequent for counseled people. Whereas the control group have between 2,5 and 3,4 meetings (depending from the initial situation), the number of meetings increase by 1.6 points over the counseling period (six months at most). Human capital services (skill assessment, advise for making resumes, application letters, etc.) also increase (circa +10 points). It should be stressed that these comparisons do not take into account services that young unemployed benefited before entering the experiment. There is no effect on matching with potential employers. This last comparison must not be over-interpreted: even if the number of vacancies proposed by youth does not increase, data do not permit to know the evolution of the number of total attempted matching. Indeed, several young unemployed may be matched with the same vacant jobs and the ratio available jobs/eligible young unemployed may differ between control and treated group.

5.6 Impact on medium-term outcomes

The survey is conducted not only 8 months after random assignment but also 12, 16 and 20 months after. Table 11 provides the ATT effects on employment outcomes on these time horizons. The first striking fact is that the employment situation in the control group (as measured by any of the three outcomes) steadily improves between the 8th and the 20th month. For instance, the proportion of individuals employed in durable jobs grows from 41% to 59%.

While some effect on the employment rate is, in the short run, significant, at least on workers that are initially unemployed, this effect vanishes in the following waves. Likewise, the impact on durable employment rate is only significant at 8 and 12 months, on the subgroup followed by for-profit operators. The only outcome on which there is a long-term effect is the the proportion of workers employed in durable fixed-term contract, unfortunately the least desirable outcome. 20 months after assignment, having been helped by a for-profit operator increases the probability of being in a durable fixed-term contract by around 5 pp.

6 Conclusion

This evaluation of a job seeker's assistance program for young graduates offers a unique opportunity to analyze both the direct impact and the equilibrium effects of counseling a certain proportion of jobseekers in a given market. We obtain several interesting results.

First, the reinforced counseling program does indeed have a positive impact on the employment situation of young jobseekers 8 months after assignment into treatment. This effect is essentially limited to for profit operators. It is plausible that they had better contact with the industry that were susceptible to hire these people.

Second, we find no evidence on externalities on other workers: it seems the operators did increase the efficiency of the matching process, rather than just led some of the unemployed workers to "jump the queue".

Third, however, while the impact is significant on the probability to hold a fixed-term contract, it for unlimited-term contracts, and the difference between treatment and control vanishes over time (mostly because the control individuals eventually find a job too).

This suggests that private operators do respond to financial incentives. As one third of their remuneration was conditional on signing a contract of at least six months, the private operators seem to have focused on the contracts that hardly passed the hurdle and not on longer ones.

Whether this policy passes a cost-benefit test thus depends on whether the gains of getting a job earlier are greater than the cost paid to the private operator.

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Table 1: Number of observations

Number of people that took part in the experiment	57,166
Number of people that were part of cohorts 3 to 12	40,307
Number of people, cohorts 3-12, belonging to a 5-EA group	30,343
Number of people, among those, that were sampled in the endline survey	29,636
Number of people, among the sampled ones, that answered at least once	25,904
Number of people, among the sampled ones, that answered the first survey	23,320

Source: Administrative file (Pôle Emploi) and survey (Dares).

Table 2: Response rates

Wave	Number of answers	Response rate		
		Total	Assigned to control	Assigned to treatment
1	23,320	78.7%	78.2%	79.1%
2	21,970	74.1%	73.5%	74.6%
3	20,791	70.2%	69.5%	70.7%
4	21,521	72.6%	72.2%	73.0%

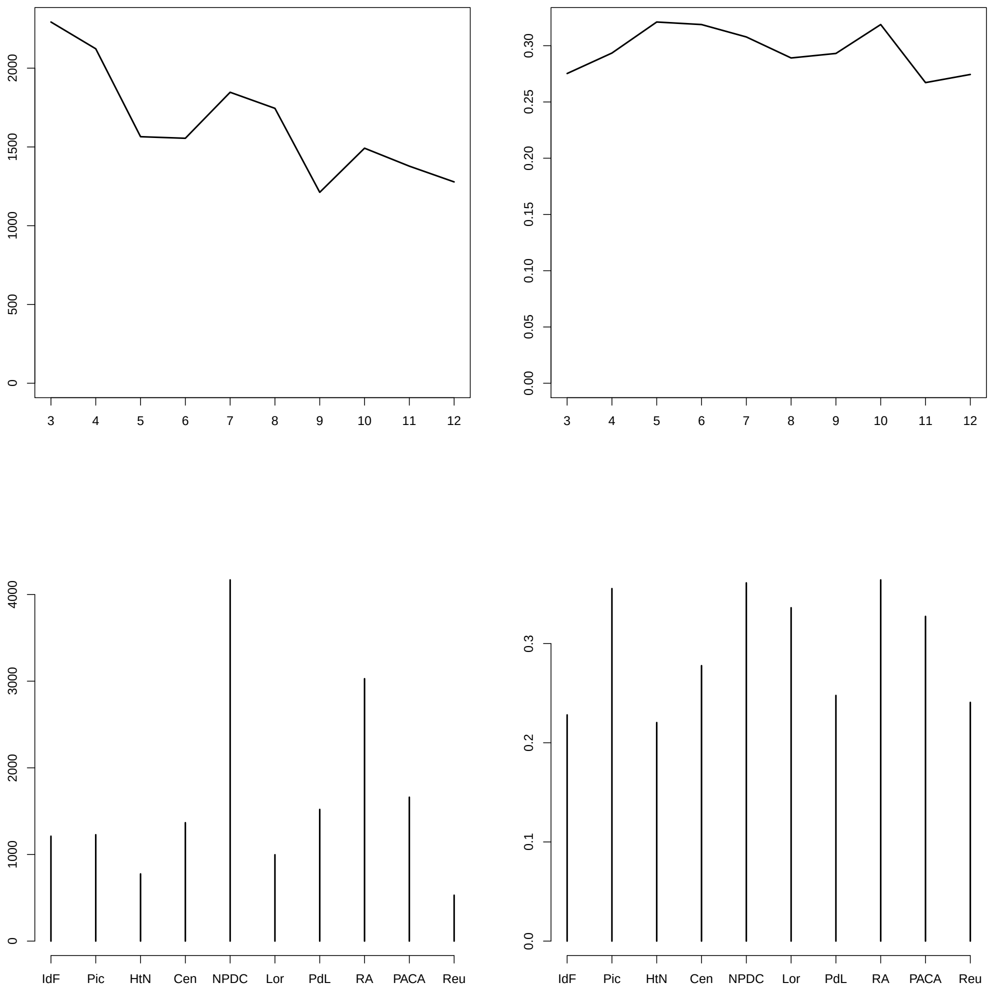
Source: Administrative file (Pôle Emploi) and survey (Dares).

Table 3: Summary Statistics

Variables	Proportions			Balancing stats	
	All	Control	Treatment	Difference	P-value
Age					
Less than 21	0.050	0.049	0.050	-0.001	0.777
22	0.067	0.061	0.071	0.003	0.404
23	0.091	0.093	0.089	-0.003	0.531
24	0.109	0.108	0.109	-0.003	0.620
25	0.136	0.136	0.136	0.001	0.900
26	0.145	0.144	0.145	-0.001	0.881
27	0.144	0.145	0.143	0.001	0.870
28	0.134	0.138	0.132	-0.001	0.875
29	0.125	0.126	0.125	0.003	0.556
Seniority in unemployment					
0 to 5 months	0.166	0.166	0.166	0.008	0.179
6 months	0.111	0.109	0.113	0.000	0.956
7 months	0.312	0.306	0.317	0.007	0.278
8 months	0.087	0.089	0.084	-0.005	0.250
9 to 12 months	0.122	0.123	0.120	0.002	0.722
12 to 18 months	0.112	0.113	0.111	-0.004	0.423
18 to 24 months	0.037	0.037	0.036	-0.000	0.929
24 to 36 months	0.035	0.037	0.034	-0.006	0.062
more than 36 months	0.018	0.019	0.018	-0.003	0.225
Benefit recepient					
Benefit recepient	0.310	0.301	0.316	0.011	0.124
Non benefit recepient	0.690	0.699	0.684	-0.011	0.124
Gender					
Female	0.635	0.643	0.628	-0.021	0.007
Male	0.365	0.357	0.372	0.021	0.007
Highest degree					
PhD	0.012	0.013	0.011	-0.001	0.566
Master from a university	0.112	0.114	0.109	0.003	0.617
Engineer, Business School Degree	0.020	0.021	0.019	-0.001	0.764
Maitrise (Bac+4)	0.065	0.064	0.066	0.007	0.069
Other Bac+4/5	0.031	0.028	0.033	0.003	0.231
Bac+3	0.162	0.162	0.161	-0.006	0.329
Bac+2 from a university	0.026	0.025	0.027	0.003	0.215
Technical Bac+2	0.326	0.320	0.331	0.002	0.804
Other Bac+2/3	0.082	0.082	0.082	-0.006	0.176
Less than Bac+2	0.039	0.041	0.037	-0.001	0.660
Not declared	0.127	0.131	0.123	-0.003	0.534
Employed	0.389	0.389	0.388	-0.001	0.876
Not employed	0.373	0.368	0.377	0.003	0.697
Undeclared	0.239	0.243	0.235	-0.002	0.791
Number of observations	29636	13148	16488		

Source: Administrative files (Pôle Emploi) and endline survey (Dares).

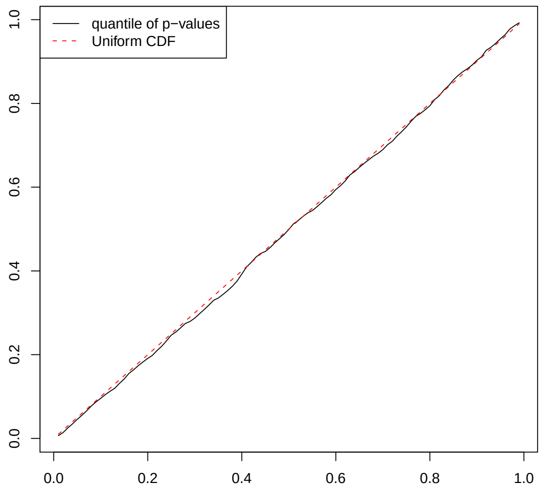
Figure 1: Number of persons assigned to treatment and take-up rate by region and by session



Source: Random draws from administrative files (Pôle Emploi).

Notes:

Figure 2: Check of the randomization procedure: Quantiles of the distribution of the p-values of the test $H_0 : \beta = 0$ in the regression "Being a man" on the treatment status

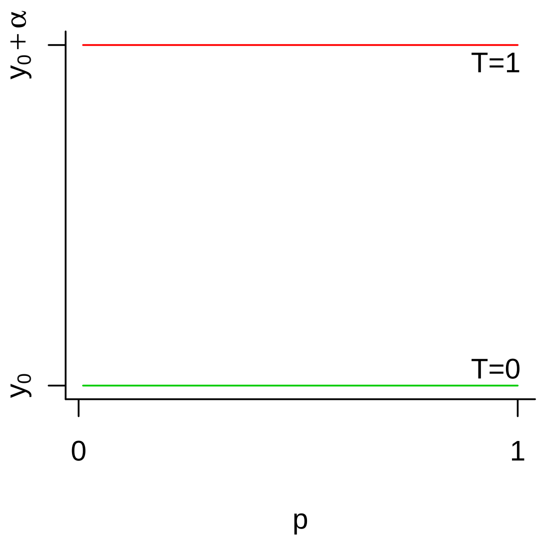


Source: Random draws from administrative files (Pôle Emploi).

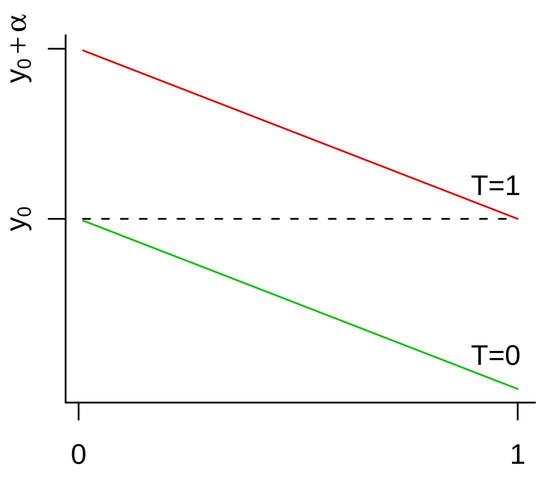
Notes: In the master file, we draw 2,000 independent treatment statuses using the same random procedure than the one that was used to determine assignment to treatment in this experiment. For each treatment vector, the gender dummy is regressed on the treatment status. Under the null hypothesis that the random procedure is really random, the p-value of the t-statistic that tests whether the OLS coefficient is equal to zero are distributed like a uniform. In this graph, the quantiles of the p-values are plotted against the ones of the uniform distribution on $(0, 1)$.

Reading note: In 94.9% of the random draws, the p-values are below the value .95.

Figure 3: Illustrative cases for two couples of values for (r, α)



$\alpha = r$



$r = 0$

Source: Authors' calculations.

Notes: Illustration of the theoretical model for $\alpha = r$ and $r = 0$.

Table 4: Take-up rates regressions

	All		For-profit	
	All	Unemp	All	Unemp
Assigned to T in 25% areas	0.349*** (0.015)	0.422*** (0.017)	0.319*** (0.026)	0.394*** (0.029)
Assigned to T in 50% areas	0.337*** (0.015)	0.431*** (0.016)	0.313*** (0.022)	0.380*** (0.024)
Assigned to T in 75% areas	0.334*** (0.011)	0.410*** (0.013)	0.290*** (0.022)	0.348*** (0.023)
Assigned to T in 100% areas	0.338*** (0.012)	0.419*** (0.014)	0.275*** (0.016)	0.346*** (0.017)
Assigned to C in 25% areas	0.001 (0.009)	0.002 (0.011)	0.001 (0.015)	0.001 (0.017)
Assigned to C in 50% areas	0.001 (0.008)	0.002 (0.011)	0.003 (0.013)	0.006 (0.016)
Assigned to C in 75% areas	0.008 (0.010)	0.007 (0.012)	0.002 (0.013)	−0.008 (0.017)
Assigned to T	0.339*** (0.008)	0.419*** (0.010)	0.292*** (0.013)	0.356*** (0.015)
Assigned to C in 25% to 75% areas	0.003 (0.007)	0.002 (0.009)	0.002 (0.011)	−0.000 (0.013)
Control mean	0.000	0.000	0.000	0.000
Number of observations	25904	14386	9789	5382

Take-up rate regressions - Results of regressions using Assignment to treatment and to control variables interacted with areas variables as instruments - first seven lines provides the results of a disaggregated regression providing take-up rates by assignment probability and by assignment status - line 8 to 10 provide the result when constraining the take-up rates to be constant over areas as well as the over-identification test - all regressions includes a set of control variables as well as quintuplet dummy variables - Standard errors are robust to heteroskedasticity and clustered at the area level- First two columns display results for the whole sample - column 3 to 4 display results for young people unemployed at the date of assignment - columns 5 to 6 displays results for for-profit operators and columns 7 to 8 display results for unemployed and for-profit operators

Table 5: Effect of the program on beneficiaries: Basic results

	Employment				Durable employment			
	All	Unemp	For-profit	For-profit & Unemp	All	Unemp	For-profit	For-profit & Unemp
				A1. Reduced to estimate (assigned to treatment)				
A. Without ALE dummies								
Assigned to treatment	0.007	0.015*	0.008	0.016	0.004	0.017*	0.014	0.036**
	(0.007)	(0.009)	(0.011)	(0.015)	(0.007)	(0.009)	(0.011)	(0.015)
B. With ALE dummies								
Assigned to treatment	0.007	0.014	−0.003	0.005	0.004	0.014	0.006	0.024
	(0.009)	(0.012)	(0.014)	(0.019)	(0.009)	(0.012)	(0.014)	(0.019)
Test of equality	0.96	0.97	0.55	0.64	0.98	0.85	0.69	0.62
				A2. Instrumental variable estimate (treated)				
A. Without ALE dummies								
Treated	0.019	0.035*	0.024	0.042	0.011	0.039*	0.044	0.094**
	(0.020)	(0.020)	(0.036)	(0.039)	(0.020)	(0.021)	(0.036)	(0.041)
B. With ALE dummies								
Treated	0.021	0.034	−0.009	0.012	0.011	0.033	0.021	0.061
	(0.026)	(0.029)	(0.044)	(0.050)	(0.026)	(0.028)	(0.045)	(0.048)
Test of equality	0.96	0.98	0.57	0.64	0.99	0.86	0.69	0.60
Number of obs.	23320	13003	8756	4823	23320	13003	8756	4823

Table 6: Effect of the program on beneficiaries: Basic results on durable fixed-term contracts

	Durable employment			
	All	Unemp	For-profit	For-profit & Unemp
	A1. Reduced to estimate (assigned to treatment)			
A. Without ALE dummies				
Assigned to treatment	0.010* (0.005)	0.018*** (0.006)	0.022** (0.009)	0.030*** (0.010)
B. With ALE dummies				
Assigned to treatment	0.008 (0.007)	0.015* (0.009)	0.015 (0.011)	0.025* (0.015)
Test of equality	0.76	0.81	0.62	0.79
	A2. Instrumental variable estimate (treated)			
A. Without ALE dummies				
Treated	0.030** (0.015)	0.042*** (0.014)	0.071** (0.029)	0.080*** (0.028)
B. With ALE dummies				
Treated	0.022 (0.021)	0.036* (0.022)	0.047 (0.036)	0.065* (0.038)
Test of equality	0.76	0.81	0.61	0.75
	23320	13003	8756	4823
Number of obs.	23320	13003	8756	4823

Table 7: Reduced form Effect on "Durable fixed term contract"

	All		For-profit	
	All	Unemp	All	Unemp
Assigned to T in 25% areas	0.020*	0.018	0.037**	0.031
	(0.010)	(0.011)	(0.017)	(0.019)
Assigned to T in 50% areas	0.010	0.006	0.023	0.014
	(0.011)	(0.012)	(0.020)	(0.020)
Assigned to T in 75% areas	0.006	0.010	0.022*	0.025*
	(0.008)	(0.009)	(0.013)	(0.014)
Assigned to T in 100% areas	0.013	0.027***	0.037**	0.056***
	(0.008)	(0.010)	(0.016)	(0.018)
Assigned to C in 25% areas	−0.002	−0.013	0.021	−0.004
	(0.010)	(0.010)	(0.016)	(0.017)
Assigned to C in 50% areas	0.001	−0.003	0.006	−0.002
	(0.009)	(0.011)	(0.015)	(0.020)
Assigned to C in 75% areas	0.025*	0.012	0.015	0.016
	(0.015)	(0.020)	(0.023)	(0.029)
Control mean	0.199	0.162	0.194	0.156
Number of observations	23320	13003	8756	4823

ITT estimates - Results of regressions using Assignment to treatment and to control variables interacted with areas variables as instruments - all regressions includes a set of control variables as well as quintuplet dummy variables - Standard errors are robust to heteroskedasticity and clustered at the area level- First two columns display results for the whole sample - column 3 to 4 display results for young people unemployed at the date of assignment - columns 5 to 6 displays results for for-profit operators and columns 7 to 8 display results for unemployed and for-profit operators

Table 8: IV estimates Effect on "Durable fixed term contract" - Effect on the treated, and externalities

	All		For-profit	
	All	Unemp	All	Unemp
Treated	0.029* (0.015)	0.043*** (0.014)	0.065** (0.029)	0.078*** (0.028)
Non treated in 25% to 75% areas	0.002 (0.007)	−0.006 (0.008)	0.014 (0.013)	0.003 (0.014)
Sargan p-value	0.553	0.333	0.875	0.502
Control mean	0.199	0.162	0.194	0.156
Number of observations	23320	13003	8756	4823

ATT estimates - Results of regressions using Assignment to treatment and to control variables interacted with areas variables as instruments - - all regressions includes a set of control variables as well as quintuplet dummy variables - Standard errors are robust to heteroskedasticity and clustered at the area level- The first column displays results for the whole sample - the column 2 displays results for young people unemployed at the date of assignment - columns 3 displays results for for-profit operators and the column 4 display results for unemployed and for-profit operators

Table 9: Estimation of the structural model

Operator	All		For-profit	
Public	All	Unemp	All	Unemp
$T_F(\hat{\alpha})$	0.025 (0.022)	0.035* (0.020)	0.043 (0.037)	0.054 (0.037)
$F(\hat{r})$	0.035 (0.023)	0.053* (0.028)	0.116** (0.052)	0.130** (0.060)
Sargan p-val	0.55	0.30	0.89	0.61
$\widehat{r-\alpha}$	0.010 (0.034)	0.018 (0.040)	0.073 (0.067)	0.075 (0.077)
P_T	0.038* (0.023)	0.068** (0.027)	0.123** (0.051)	0.153*** (0.058)
Sargan p-val	0.53	0.21	0.81	0.46
Mean	0.199	0.162	0.194	0.156
N. Obs	23320	13003	8756	4823

ATT estimates - Results of regression 8 using treatment, effective proportion of treated over the area and instrumented with Assignment to treatment Z and 1- Z interacted with dummies for the 4 different theoretical proportions. It includes a set of control variables as well as quintuplet dummy variables - Standard errors are robust to heteroskedasticity and clustered at the area level- The first column displays results for the whole sample - the column 2 displays results for young people unemployed at the date of assignment - the columns 3 displays results for for-profit operators and the column 4 display results for unemployed and for-profit operators

Table 10: Effect on several employment outcomes

ATT estimates

		All operators		Only for-profit ones	
	Mean	All	Unemp	All	Unemp
Employment	0.537	0.019 (0.020)	0.035* (0.020)	0.024 (0.036)	0.042 (0.039)
Durable employment	0.412	0.011 (0.020)	0.039* (0.021)	0.044 (0.036)	0.094** (0.041)
Durable fixed contract	0.178	0.030** (0.015)	0.042*** (0.014)	0.071** (0.029)	0.080*** (0.028)
Number of observations		23320	13003	8756	4823

Results for for-profit operators for all the young people and just for young people unemployed at the date of assignment - Instrumental variable estimations using Assignment to treatment and to control variables interacted with areas variables as instruments. Last two sets of columns present ATT estimates of constrained estimation 6 - All regressions includes a set of control variables as well as quintuplet dummy variables - Standard errors are robust to heteroskedasticity and clustered at the area level

Table 11: Effect on employment outcomes 8, 12, 16 and 20 months after assignment - ATT

		All operators		Only for-profit ones	
	Mean	All	Unemp	All	Unemp
Employment: 8 months	0.537	0.019	0.035*	0.024	0.042
		(0.020)	(0.020)	(0.036)	(0.039)
Employment: 12 months	0.595	0.020	0.013	0.048	0.042
		(0.020)	(0.021)	(0.033)	(0.040)
Employment: 16 months	0.644	0.002	0.023	0.026	0.030
		(0.019)	(0.021)	(0.033)	(0.035)
Employment: 20 months	0.677	−0.013	−0.000	0.035	0.043
		(0.018)	(0.019)	(0.028)	(0.035)
Durable employment: 8 months	0.412	0.011	0.039*	0.044	0.094**
		(0.020)	(0.021)	(0.036)	(0.041)
Durable employment: 12 months	0.485	0.018	0.028	0.076**	0.092**
		(0.019)	(0.019)	(0.034)	(0.039)
Durable employment: 16 months	0.550	0.002	0.005	0.027	0.033
		(0.020)	(0.021)	(0.035)	(0.037)
Durable employment: 20 months	0.594	−0.021	−0.025	0.025	0.018
		(0.018)	(0.019)	(0.029)	(0.032)
Indefinite term contract: 8 months	0.234	−0.023	−0.011	−0.015	0.011
		(0.017)	(0.018)	(0.030)	(0.032)
Indefinite term contract: 12 months	0.303	−0.007	0.004	−0.023	−0.004
		(0.018)	(0.018)	(0.032)	(0.035)
Indefinite term contract: 16 months	0.356	−0.002	−0.002	0.027	0.015
		(0.019)	(0.019)	(0.033)	(0.038)
Indefinite term contract: 20 months	0.403	−0.030	−0.017	−0.031	−0.003
		(0.018)	(0.019)	(0.029)	(0.034)
Durable fixed contract: 8 months	0.178	0.030**	0.042***	0.071**	0.080***
		(0.015)	(0.014)	(0.029)	(0.028)
Durable fixed contract: 12 months	0.182	0.021	0.021	0.084**	0.080**
		(0.016)	(0.017)	(0.033)	(0.036)
Durable fixed contract: 16 months	0.195	0.016	0.016	0.021	0.031
		(0.015)	(0.018)	(0.029)	(0.037)
Durable fixed contract: 20 months	0.191	0.007	−0.013	0.048*	0.026
		(0.015)	(0.017)	(0.025)	(0.032)
Number of observations		23320	13003	8756	4823

Results for for-profit operators for all the young people and just for young people unemployed at the date of assignment - Instrumental variable estimation of equation 6 using Assignment to treatment and to control variables interacted with areas variables as instruments. All regressions includes a set of control variables as well as quintuplet dummy variables - Standard errors are robust to heteroskedasticity and clustered at the area level

Table 12: Effect on the content of the counseling scheme

ATT estimates

		All operators		Only for-profit ones	
	Mean	All	Unemp	All	Unemp
Number of meetings	3.017	1.558*** (0.159)	1.387*** (0.184)	1.648*** (0.249)	1.465*** (0.299)
Human capital services	0.249	0.279*** (0.017)	0.249*** (0.019)	0.308*** (0.034)	0.274*** (0.037)
Matching	0.180	0.023 (0.015)	0.017 (0.017)	−0.010 (0.028)	0.001 (0.034)
Number of observations		23320	13003	8756	4823

Results for for-profit operators for all the young people and just for young people unemployed at the date of assignment - Instrumental variable estimations using Assignment to treatment and to control variables interacted with areas variables as instruments. These are from instrumental variable estimates of equation 6 - All regressions includes a set of control variables as well as quintuplet dummy variables - Standard errors are robust to heteroskedasticity and clustered at the area level

APPENDIX TABLES

Table 13: Summary Statistics (2)

Variables	Means
	All respondents
Type of operator	
For-profit operator	0.620
Not-for-profit operator	0.380
Region	
Ile de France	0.076
Picardie	0.072
Haute Normandie	0.045
Centre	0.092
Nord Pas de Calais	0.232
Lorraine	0.063
Pays de Loire	0.094
Rhone Alpes	0.187
PACA	0.109
La Reunion	0.030
Percent of assigned to treatment in the agency	
0%	0.194
25%	0.207
50%	0.210
75%	0.195
100%	0.195
Cohort	
3	0.140
4	0.130
5	0.095
6	0.094
7	0.109
8	0.107
9	0.073
10	0.090
11	0.084
12	0.078
Number of observations	29636

Source: Administrative files (Pôle Emploi) and endline survey (Dares).

Table 14: Take-up by individual characteristics: probit regression

Variables		Coefficients
Intercept		-0.488^{***} (0.066)
Male		0.044^{**} (0.022)
Highest degree	(Ref: Technical Bac+2)	
PhD		-0.249^{**} (0.104)
Master from a university		-0.266^{***} (0.059)
Engineer, Business School Degree		-0.652^{***} (0.048)
Maitrise (Bac+4)		0.101^{***} (0.037)
Other Bac+4/5		-0.031 (0.077)
Bac+3		0.039 (0.045)
Bac+2 from a university		0.036 (0.060)
Other Bac+2/3		0.001 (0.032)
Less than Bac+2		-0.020 (0.065)
Not declared		-0.030 (0.041)
Seniority in unemployment	(Ref: 7 months)	
0 month		0.116^{***} (0.042)
3 months		0.044 (0.064)
4 months		0.102 (0.065)
5 months		0.190^{***} (0.067)
6 months		-0.005 (0.037)
8 months		0.052 (0.041)
9 to 12 months		0.071^{*} (0.037)
12 to 18 months		0.102^{***} (0.039)
18 to 24 months		0.116^{*} (0.060)
24 to 36 months		0.040 (0.063)
More than 36 months		0.022 (0.084)
Benefit recipient	(Ref: Recipient)	
Non benefit recipient		-0.128^{***} (0.024)
Employed at the time of assignment	(Ref: Not employed)	
Employed		-0.542^{***} (0.024)
Undeclared		-0.117^{***} (0.035)

Table 15: Take-up by individual characteristics: probit regression (cont'd)

Variables		Coefficients
Age	(Ref: 26)	
Less than 21		−0.030 (0.057)
22		−0.055 (0.049)
23		−0.046 (0.045)
24		−0.023 (0.042)
25		−0.008 (0.039)
27		0.005 (0.039)
28		−0.035 (0.040)
29		−0.038 (0.040)
Cohort	(Ref: 7)	
3		−0.097** (0.044)
4		0.036 (0.043)
5		0.061 (0.046)
6		0.027 (0.046)
8		−0.004 (0.045)
9		0.032 (0.050)
10		0.054 (0.047)
11		−0.063 (0.049)
12		−0.103** (0.050)
Region	(Ref: Ile-de-France)	
Picardie		0.399*** (0.056)
Haute Normandie		−0.006 (0.066)
Centre		0.181*** (0.056)
Nord Pas de Calais		0.399*** (0.046)
Lorraine		0.282*** (0.059)
Pays de Loire		0.089 (0.055)
Rhone Alpes		0.408*** (0.047)
PACA		0.303*** (0.052)
La Reunion		0.018 (0.074)
Percentage of assigned to treatment in the agency	(Ref: 50%)	
25%		0.018 (0.035)
75%		−0.022 (0.031)
100%		−0.007 (0.030)
Number of observations		16488

Source: Administrative files (Pôle Emploi) and endline survey (Dares).

Table 16: Response: Panel pattern

Panel response pattern	Number of people
1234	16,514
X234	1,008
1X34	1,185
12X4	1,506
123X	1,113
XX34	446
X2X4	168
X23X	156
1XX4	304
1X3X	201
12XX	1,191
XXX4	370
XX3X	148
X2XX	314
1XXX	1,306

Source: Administrative file (Pôle Emploi) and survey (Dares).